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THE REORGANIZATION OF BIOLOGY
AT THE UNIVERSITY OF CALIFORNIA, BERKELEY

Daniel E. Koshland, Jr.
Roderic B. Park
Louise Taylor

Interviews Conducted by
Sally Smith Hughes
in 1998 and 1999

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INTERVIEW HISTORY—by Sally Hughes

This oral history reflects the viewpoints of three significant participants in the reorganization of the biological sciences at UC Berkeley—a long and contentious process, which began in the early 1980s and continued for a decade. The result in organizational terms was the creation of two mega-departments, the Department of Molecular and Cell Biology and the Department of Integrative Biology, the reformulation of undergraduate and graduate curricula, the construction of three science buildings and an animal care facility, and the renovation of the Life Sciences Building. One goal was to realign the science faculty into research affinity groups perceived to have mutually enhancing research interests.

Daniel E. Koshland, Jr., in a chapter from his forthcoming oral history, reflects the views of the biochemically and molecularly-oriented biologists who saw Berkeley slipping in regard to sister institutions and biotechnology companies in adopting the new genetic technologies, such as recombinant DNA, gene sequencing, and monoclonal antibody technologies. His status on campus as a respected professor of biochemistry, editor of the journal *Science*, and member of the National Academy of Sciences ensured that his arguments for reorganization had weight with the faculty and the administration. He is frank in crediting Rod Park, the narrator of the second oral history, with “selling” the reorganization plan to the faculty, especially to the skeptical organismal scientists, who rightly feared that the molecular biologists had the upper hand. Louise Taylor, the narrator of the third oral history, was the woman in the trenches seeing that the myriad organizational details ran smoothly, attending virtually every meeting of the several committees involved in reorganization, and preserving key documents generated in the complex process.

All three oral histories record the occasional rancor of the process as long-standing departments were abolished and re-formed, office and laboratory space reassigned, and the Academic Senate marginalized—or so it seemed to some participants. The narrators also discuss contending with opposition from animal rights activists and campaigning in Sacramento for building funds.

This oral history volume represents a start on documenting an important episode in campus history. It leans heavily towards those favoring reorganization from the start. Left out are the voices of the opposition, vociferous at first and more subdued as the process unrolled. Also missing is the perspective of members of the Academic Senate. Although Beth Burnside was invited to record her viewpoint of the Senate’s role, she declined in part because of her new appointment as Vice Chancellor for Research at the time the interviews were being conducted. Also of historical interest is the perspective of the animal rights contingent as well as the legislators in Sacramento involved in the campaign for construction funds. A second oral history volume seems called for in order to complete the historical record.

We are grateful to Louise Taylor for the donation of a binder of documents on the reorganization process, a valuable adjunct to the oral history and available for research in the Bancroft Library’s History of Science and Technology Collection. We also thank her for careful annotation of her oral history, a reflection of her dedication to the university and outstanding organizational skills.

This oral history is an important step towards full documentation of the reorganization of biology at Berkeley. Many credit the effort with helping to raise faculty morale, improve faculty and graduate student recruitment and retention, and advance the university's stature as a research university. This oral history may also be of use in guiding other institutions contemplating the realignment of their own academic endeavors.

Sally Smith Hughes
Historian of Science and Interviewer

Regional Oral History Office
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July 2003

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University of California
Berkeley, California

REORGANIZATION OF BIOLOGY
AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1980s-1990s

An Interview with
Daniel E. Koshland, Jr.

Interviews Conducted by
Sally Smith Hughes
in 1999



Daniel E. Koshland

INTERVIEW WITH DANIEL E. KOSHLAND, Jr.

THE REORGANIZATION OF BIOLOGY AT UC BERKELEY

[Interview 12: April 6, 1999; Interview 13: May 7, 1999]¹ ##²

Defining the Problems in Biology at Berkeley, 1980

Hughes: In spring 1980, the Chancellor's office appointed four committees to review academic programs in biology on campus.³ The external review committee had not yet been appointed. They made some preliminary recommendations for a reorganization of biology at Berkeley. Do you know about those four committees?

Koshland: Yes. It really all started when my wife Bunny [Marian E. Koshland] and I were having cocktails at the Men's Faculty Club, at which point Roderic Park asked me what was the state of the biological sciences. Rod at that point was Dean of Biological Sciences in the College of Letters & Science [L& S]. Before I could say anything, my wife said, "Terrible." Rod was very upset and said, "We've been tops in graduate school evaluations in all these ratings."

My wife was in the department of immunology in the Life Science Building [LSB]. The people in immunology were just as smart as the people in biochemistry, but they were trying to recruit people into a building that was old and decrepit. My wife Bunny was a key leader in getting new, young people. But a lot of the people they wanted to recruit just wouldn't come when they saw the facilities.

Rod turned to me and said, "Dan, what do you think?" Being loyal to my wife (I didn't want to be too blunt) I said to him, "Well, she's really partly right. There are some

¹Interviews 12 and 13 were conducted as part of a long biographical oral history with Professor Koshland. Because there was some repetition, the two interviews have been combined, some duplication eliminated, and the chronology improved.

² ## This symbol indicates that a tape side or segment has begun or ended. A guide to the tapes follows the transcript.

³Vice Chancellor Roderic B. Park to Dean Robert Glaeser, March 6, 1981 (Office of Planning & Analysis, Vice Chancellor--Resource Planning & Budget, University Hall, University of California, Berkeley. Hereafter, University Hall documents.)

departments that are doing very well. (I wasn't thinking of just biochemistry.) But a lot of our departments are doing poorly, sometimes due to bad facilities, and sometimes because the current faculty aren't recruiting very good people, even in rather good facilities. There is a problem of program, and a problem of facilities."

A year or so thereafter, Rod became vice chancellor under Mike [Ira Michael] Heyman,⁴ who was then chancellor. Rod remembered the conversation, which is rare for administrators and he decided that we should have an inventory of the life sciences. He appointed a committee made up of four principals. One person represented biochemistry (Alex Glazer), and one person represented cell biology (me), and one person physiology (Beth Burnside), and one person whole-animal (organismal) biology (David Wake). We called ourselves the Gang of Four because in China the Gang of Four took over the Cultural Revolution.

Our mandate from Rod was to make an inventory of biologists: how many people were there on the campus who called ourselves biologists? You might say it's pretty easy; you just look at the departments. But it isn't quite that easy. For example, the School of Public Health had a department which had a mixture of biologists and economists, and the College of Engineering had some people whom you would call bioengineers mixed with others who were largely engineers outside biology.

Our job was to find out who we were talking about, and we came up with a list. But as is inevitable, we poked our nose into other things, which is exactly what I think Rod expected us to do. We went well beyond our mandate and proceeded to say the things we thought were really not so good about biology at Berkeley and needed to be corrected, like facilities in LSB and departments that were being run very poorly. So we fulfilled our mandate in terms of getting a survey of biologists, but we in addition said what the campus needed to do to improve the biological sciences. We indicated to Rod that we thought biology needed a good deal of reorganization.

Our focus was on the very weak departments, of which genetics was a good example. There was also duplication, for example, a plant biology department in the College of Natural Resources and a plant department in Letters & Science, and we thought they should get together. And some departments had outmoded programs. For example, the zoology department was considered one of the best in the country. But, as it turned out, it was the only zoology department in the country because all the others had changed their names to integrated biology and were tackling more modern problems. In other words, the zoology department was not as modern as it should have been.

⁴See the ROHO oral history in progress with Dr. Heyman.

Internal Biology Review Committee

Hughes: Dan is now looking at a document, "Report of Biology Review Committee," August 4, 1981.⁵

Koshland: So this was the internal biology review committee.⁶

Hughes: Which is different than the one that was assembling the inventory?

Koshland: I think so, yes.

[reading] "The Biology Review Committee was given the task of evaluating the programs in the biological sciences on the Berkeley campus and analyzing the space needs of these sciences." Okay, that's what I remember. There was a desperate need for space. "...there are certain organizational and programmatic changes which can be made immediately in the absence of added space...The combination of new space plus organizational and programmatic changes throughout the campus can make an extremely attractive intellectual community which could be in the forefront of the ferment now occurring in the biological sciences..." This was a pretty good report, but a little blander than probably it needed to be.

Hughes: Here is another document of this same period.⁷

Koshland: [skims document] I said in this letter that I felt that we shouldn't just go for a biology building; this was a chance to re-do the program, that the program was archaic as well as the building. The chancellor must have gotten news of what we were doing and complained, and I wrote that letter about it. Probably, I was going around the campus and talking about molecular biology, and the chancellor was very concerned. I don't know who Syme is.⁸

Hughes: [Leonard S.] Syme is a professor in the School of Public Health.

Koshland: Anyway, he was, I guess, concerned that I was going to push reorganization in the direction of molecular biology, and he was right, of course.

⁵ University Hall documents, folder: Biology Reorganization, Reports & Other Documents.

⁶ The signatories of the report are: A. Glazer, D. Koshland, M. Schroth, and D. Wake.

⁷ Koshland to Chancellor Ira M. Heyman, July 21, 1981 (D.E. Koshland, Jr. correspondence, The Bancroft Library, 84/33, carton 6, folder: 1-7/1980).

⁸ According to Koshland's letter of July 21, 1980, Syme had written to the chancellor with concerns about the reorganization plan.

The Chancellor's Advisory Committee on Biology (CACB)

Bypassing the Deans

Koshland: At the beginning of the new Chancellor's Advisory Committee on Biology [CACB], we were at arm's length with the deans and were not going to invite them to join our meeting. But then we decided it was just too difficult to have all our meetings and then communicate with them after, because they would miss the flavor of our meetings. So we invited both the deans to come to our meetings, with the understanding that they were guests and that we could ask them to leave whenever we wanted to have a private discussion behind their backs. But, in fact, we never did. Both of them were very cooperative, and they added appreciably to the success of our meetings.

Hughes: For the record, it was the Dean of Letters & Science and the Dean of the College of Natural Resources?

Koshland: The Dean of Biology in Letters & Science. We started out with Bob [Robert M.] Glaeser, and he was later replaced by Beth Burnside. Beth Burnside was very able and was, originally, one of the Gang of Four. Everybody thought she did a very good job. When the time came to appoint a dean of Letters & Science, she was picked. She was a very good dean and good person in the reorganization.

I suggested a committee which would be called the Chancellor's Advisory Committee on Biology. I remember proposing this, and I remember the reason behind my logic. The logic was, we wanted an excuse to go directly to the chancellor because we had some very bad deans. It's difficult if you're a member of the faculty to go behind the dean's back, but if you went through the dean, he would say, "We've got to wait until we meet with the chancellor," and then the meeting would never come off. The idea behind the CACB was simply to make it possible for a group of ordinary professors to report directly to the chancellor, without violating all the formalities of proper channels. That was important because we really didn't have good deans at that time.

Part of the problem was, the deans were people who had gotten out of research and maybe were ossified, but they had a lot of power. The same thing was happening with the chairmen of departments; the deans tended to appoint people who were friends of theirs or who wouldn't cause them any trouble, and so everything was getting a little ossified. The CACB did address that problem. We wanted to address it structurally. We could have lobbied, I suppose, for getting the vice chancellor to kick out some chairmen of departments, but it would have been very temporary. We decided really to go for the whole enchilada, as they say. So we wanted the CACB to control new appointments and the new selection committees. That controls the area of science for new appointments and the quality of the new appointments.

The deans discovered that we were very useful to them; they actually could do their jobs a lot better by having the advisory council. And the advisory council for its part, as I said, didn't want to do the deans' work. So after we had this big discussion, we'd say to the dean,

“This is the proposed membership for the search committee; you go out and actually appoint them.” So as far as the appointee knew, the dean was phoning him up and asking him to be on the search committee. The dean had all his previous authority but was sort of told that he’d better take the advice of the advisory committee before he used that authority.

Hughes: The deans were “bad” in what sense?

Koshland: Well, nice people but behind the times. Many of the faculty in the sciences—certainly the biologists but I think also physicists—don’t want to be deans. Deans tend to be the people whose research careers have sort of come to an end; then they go into administration. That’s a little unfair because we need good administrators as well as good scientists. But when you’re a young scientist and doing well, you look on your career as becoming a famous scientist, and you don’t look on your career as becoming a dean. There was almost a selection for the less good members of the faculty to be deans because the faculty distrusted deans and denied them real power. The problem became bad because the deans were not up on the latest developments in the hot new areas of science and they had a role in selecting new faculty. I was also aware that most of the best scientists I knew didn’t want to be bothered with administrative duties, and as a result administration fell by default to less able people.

It was a notorious fact in our department that every chairman that was selected didn’t want to be chairman. I remember when I became chairman, a group of faculty came around and asked me to be chairman, and I said my career was in full flourish, and I didn’t want to spend time being a chairman of a department. [tape interruption] I reluctantly agreed because Bruce Ames and a bunch of people came around and said, “Dan, it’s time. You have to be chairman.” So I became chairman, and then, when I finished my five years [1973-1978], the next logical person was Bruce Ames. I remember asking him, and he said he didn’t want to do it; he was in the middle of his career. I reminded him that he had come to me, and I said, “It’s your duty now,” and he accepted.

We found that our chairmen were very conscientious once they accepted being chairman. If they took the job, they did a good job at it. But they never wanted to be chair. This was the lure of science. You could be chairman and still do science; you were still in your department, and you carried on your lab. Whereas as dean, in those days you had to move someplace else, and you could no longer do science.

The Chancellor’s Charge to CACB

Hughes: The chancellor’s charge to the CACB is dated July 14th, 1981.⁹ Heyman’s official charge is [reading]: “The charge to the Council is to provide advice and recommendations to the Deans

⁹Chancellor Heyman to Professors John Gerhart, John Hearst, Daniel Koshland, Richard Malkin, Hiroshi Nikaido, Milton Schroth, and Gerald Westheimer (UCB University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

and the Provosts concerning the development of an integrated and coordinated program in Biology in order to achieve a leadership role appropriate for Berkeley. The Council will be concerned with: all aspects of Biology, including the organization and identification of new areas and potential faculty, resources and priorities for their allocation in order to maintain present areas and develop new ones, the continued review of the quality of teaching and research in the Biological Sciences, coordination and evaluation of teaching programs in the Biological Sciences [,] and reporting annually to the Chancellor on the status of Biological Sciences on the campus.”

Koshland: Okay, that was really my idea. There were all sorts of nuances to that charge, which I can explain to you.

Appointing Members

Koshland: My memory is the following: We had a committee of nine--the Advisory Council on Biology--and the large committee didn't get very far in the reorganization.

Hughes: It was originally seven, but CACB was expanded to nine because there was criticism that the original committee was too molecularly oriented.

Koshland: Yes, correct. So it was nine, and the nine people solved the political problem of representation.

The criteria we had when we invited people to be members of the advisory council was: We appointed you because, for example, you're a biochemist, and we want somebody who knows the area of biochemistry. But the understanding is, you're not just representing your department or your discipline; you're responsible for the good of the university. If it means you have to cut back on the biochemistry department even though you're a biochemist, we expect you to do that. Or a zoologist and so forth.

It really was amazing what happened. People really behaved very well, and they really took membership on the committee on that condition. The job was to help the university, and they knew what was going on in their specialty, but their first loyalty was to the university, not the discipline.

Hughes: Who selected the members?

Koshland: They were all appointed by Rod Park, who was acting for the chancellor, but in fact Rod always asked me who we should appoint. That means he used my advice, but he also consulted others, and if he didn't like the person, he would tell me. But, in fact, he was by then doing a great many chores for the Chancellor, and I really knew a lot about the people involved, so it helped him if I made suggestions. But I think that was very typical of the relation I had with Rod. We exchanged information, so it really didn't matter. But the committee appointment was specifically the chancellor's appointment, for which the vice chancellor was the executive for the campus.

Hughes: What were your criteria for recommending a person?

Koshland: Well, the criterion was, number one, that they had to be an expert in their field, an outstanding scientist, because we wanted these people to select good new faculty. That was a very important part of the assignment. And B) that they were a responsible citizen; they would take membership on the council seriously. The condition for being a member was that you had to meet I'd say for a couple of hours maybe once a month, and then you'd go home and think about a problem. But it wasn't so demanding of time that you had to give up your research the way a dean had to. On the other hand, you had to be serious enough so you'd spend the time to go to the meetings and think about the problems.

Hughes: Did the CACB replace the Biology Council?

Koshland: It did, finally. But at the beginning it was appointed independently.

CACB's Advisory Status and Power to Appoint Faculty Search Committees

Koshland: I remember saying to Rod that I wanted the CACB to be only advisory. They were talking about giving us powers, and I only wanted one power: I wanted to appoint all the search committees looking for new faculty. And we had in the charter that once a year we met with the chancellor. That's all the power you really needed because then the deans knew we met with the chancellor.

The persons who had previously called up the search committee were the deans. The deans were very worried about this new power; it did threaten their power somewhat. But we were really altruistic and selfish in, one, sharing the power of appointment of search committees with them: they could call up the people and make the appointment, but they had to take the council's advice. And number two, we didn't want to do all the work. To be very honest, it was symbiotic. So we said we were an advisory group, and I felt it was a very good thing to be advisory because nobody is paying you; you have no power. So the minute your advice is no good, nobody's going to take it, either the vice chancellor or the deans.

The CACB Subcommittee on Reorganization

Koshland: The first person we put in charge of the reorganization subcommittee of the CACB was John Gearhart, who was a member of the original advisory council. He's a good friend of mine, member of the National Academy of Sciences, excellent scientist, a very nice person. But he just wasn't very forceful; not very much got done. The original committee [CACB] was nine

people representing a whole bunch of biological disciplines.¹⁰ But they didn't focus that much, and people didn't want to meet that often. I had been on the original Biology Council, which is automatic for a chair of biology, biochemistry, zoology, etc., and all of the people there were very nice. But each of us felt we were there to protect our own departments. So there was not the kind of give and take that resulted when we had a committee (the CACB) where the primary loyalty was to the university and the secondary loyalty was to the department.

My memory is that it was something like May or June, and I decided we needed to have a smaller committee, not the whole nine people, and really sit down and do a good job of reorganization. That smaller group, as I remember, was Beth Burnside, Jeremy Thorner, Alex Glazer, and I. It was a subset of the advisory council whose single purpose was to do the reorganization. We agreed to meet every other week, I think, for three hours all through the summer, with the goal to have the reorganization done by September that same year, and then we'd go back to the bigger committee, and if they approved it, then they would go ahead with the plan.

Deciding on a Radical Reorganization

Koshland: I wanted to have a really radical plan in which we would have a bunch of new buildings and a really reorganized program in the biological sciences. That was the bedrock. So we then discussed various alternatives, one of which was tearing down LSB. We came to the conclusion that we couldn't do it that way. We needed to have two new buildings because we had to put people into something while we were tearing things down. Eventually, we decided to have the two new buildings and then renovate LSB.

We started to think of who would be in what buildings and how we would organize them. And then it became very complicated, and I think someone in the middle of the summer said, "Oh, well, let's go back to a more modest plan." At that point, Jeremy Thorner made a famous statement that I still remember. He said, "No, this is the chance of a lifetime, and we shouldn't go backwards." And then everybody got their courage together, and we agreed on a really radical plan.

¹⁰Chancellor Heyman to Professors John Gerhart, John Hearst, Daniel Koshland, Richard Malkin, Hiroshi Nikaido, Milton Schroth, and Gerald Westheimer (UCB University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

Presentations of the Reorganization Plan to the Campus

Hughes: The first reorganization plan that the CACB presented to the faculty was dated December 3, 1984.¹¹ It's fairly detailed. It told people where they might have to go and which departments would be amalgamated.

Koshland: The first one to the campus, you mean.

Hughes: The first one to the campus.

Koshland: But we sent a report to the faculty before this.

Hughes: This December 3 plan was presented at the Men's Faculty Club meeting that Rod Park presided over.

Koshland: Correct. But we had sent one a good deal earlier--it had to be well before '84--where we talked about abolishing departments. That's the one that caused the tremendous fuss.

Hughes: I think it's this one, the report of the internal review committee.¹²

Koshland: I think this may be it. We decided we were not going to send it just to the chairs because we knew a bunch of chairs were deadly opposed to it, so we determined to send it to everybody on the biology faculty.

External Biology Review Committee: The 1981 and 1986 Reports

Koshland: Then we got together an external committee.

Hughes: Who made those appointments?

Koshland: I gave a list of names to Rod, who invited the people. So I gave Rod some names of good molecular biologists and biochemists and so forth, and David Wake came up with some names of people who were organismal biologists, and then we got some names from the College of Natural Resources. Botany people came up with some names. And so we had a

¹¹Chancellor's Advisory Council on Biology to Chancellor Michael Heyman and Vice Chancellor Roderic Park, December 3, 1984 (University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

¹² A. Glazer, D. Koshland, M. Schroth, and D. Wake, "Report of Biology Review Committee," August 4, 1981 (UCB University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

very distinguished list, a big range of expertise. William Barker from Cornell became the chairman of the committee, and he was very good. That committee was great.¹

My approach is, you don't say to an external committee, "What's wrong with the campus?" and expect them to learn everything in a weekend and come up with sensible solutions. What you can do with an external committee is present them with a plan and have them criticize it, from their own point of view, their extensive expertise and experience. When they first came in, we gave them what we had as a rough plan and asked them to make their comments. Basically, what they said is that they concurred with it in general.

When the external committee came back the second time [February 1986], we came up with a much more detailed plan.² But the first time, they basically said we were right that the administration desperately needed to reorganize the program and build new buildings, which are the key things we wanted from the external committee. At that point, Rod and the chancellor supported the general conclusion that we really had to do a reorganization.

Hughes: That was the turning point?

Koshland: There were a number of turning points. That's what's important in a campaign like this. If we had lost our energy at that point, the whole thing would have gone down the tubes. But it was certainly a very crucial point because the chancellor and vice chancellor stood on the side of the people who wanted to do a major reorganization. That's what it really meant: a major reorganization.

Hughes: The external review committee met with some faculty members and administrators on their first trip.

Koshland: Well, of course they did. They interviewed a lot of people.

Hughes: Among others, they met with a group of seven younger faculty, who presumably were particularly interested in renovating the science.³

Koshland: Yes, they were good measures of the problems we had to correct. The people who were pushing it, to name a few, were I and Alex Glazer and Beth Burnside, who were older. But the young assistant professors did help because they saw the elements of decay in various departments. So they supported the reorganization plan.

¹The first report is: "The Biological Sciences; University of California, Berkeley, April 1981." Report of the External Review Committee. It lists the committee members. (University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

²"Biology at U.C. Berkeley," Report of the External Review Committee, February 26-28, 1986 (University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

³ The 1981 report of the external review committee lists the groups with which it met, including "seven younger faculty members selected by the Internal Review Committee..."

Hughes: I didn't mean that they were leading the charge, but I imagine that they represented a vested interest in the new biology.

Koshland: Yes. I'm just adding that the supporters were not all young people. Some people become ossified at an early age and many older people welcome change. The genetics department was hiring very poor choices. What you're saying is right. I'm just saying it isn't exclusively young people who are innovative.

Hughes: Getting back to the external review committee, one point that the committee made was that it was urgent to change the administrative structure of biology. They saw the existing structure as far too complicated--the line of command was too lengthy--so that scientists couldn't easily get to where the power was.

Koshland: Exactly.

Recognizing the Commercial and Scientific Possibilities of Molecular Biology

Hughes: I'll read one more section of the external review report, which is dated April 1981. What is beginning to happen in the external world is the emergence of the biotech industry. Note how the committee recognized that: "The industrial applications of biology will lead to new sources of support for research and bring new students to the field. Both will be foci of administrative and academic concern." [tape interruption]

Continuing the quote: "It will be necessary to construct mechanisms for the conduct of industrially related research. Industrial applications will present new opportunities and responsibilities in teaching. As with advances in chemistry and physics that led to new areas in engineering, new specializations in biotechnology will emerge from recent biological discoveries. To meet these emerging demands will require new lines of communication and collaboration between biology, chemistry and engineering and completely new programs of instruction will have to be developed."¹⁶ It's interesting to see the commercial applications for biology spelled out so clearly.

Koshland: Right. We picked a very good group.

Hughes: This is 1981, just the beginning of the biotech industry!

Koshland: Oh, sure, but remember, recombinant DNA was already out.

Hughes: Oh, it was out, but the industry was just beginning.

¹⁶"The Biological Sciences," University of California, Berkeley, Report of the External Review Committee, April 1981 (UCB University of Hall documents).

Koshland: I would argue at that time that we needed a lot more molecular biology. Then the old-fashioned botanists were saying that I was just trying to convert everybody to a biochemist--because most of the people who did the early molecular biology were biochemists. My argument was that molecular biology was a tool, the way a computer is a tool, and your definition of whether you call yourself a botanist or a biochemist or geneticist is what problem you want to solve, not the tools you're using to solve it.

In migration of populations, the way the old-fashioned zoologists were doing it was by what color a squirrel was, and you could trace his origin from the color. Some of the early zoologists were already beginning to trace the migration of squirrels by their DNA. So today DNA turns out to be a very powerful tool for the zoologists, and most of our faculty at that time were missing it completely (a few were not).

Hughes: And that was happening already in 1981?

Koshland: Not right then, but that was the kind of thing that those of us who thought molecular biology was an exciting tool foresaw was going to happen, and that's why we wanted to start to get it into every department. And this outside committee saw it also.

Hughes: Yes, it did. The external committee stated that the molecular approach, the new biology, will eventually affect absolutely every branch of biology.

Koshland: Yes. And those of us who wanted change made that presentation to them when they came.

CACB Appoints Faculty Search Committees

Hughes: You said the genetics department was hiring "poor choices." In what sense?

Koshland: They were really way below the standards that we would expect at Berkeley.

Hughes: It wasn't merely the fact that they were doing classical genetics?

Koshland: Some of it was. They were doing old-fashioned genetics. We'd go to National Academy meetings, to biology meetings, and we'd meet people from other universities who we knew were very good. And they'd say to me, "Berkeley's genetics is really in terrible shape. You hired so-and-so, and we interviewed that guy. He's really terrible. Why did you ever hire him?" So it's that kind of gossip you hear. And then you look up the records, and this guy is not getting grants, so you realize the genetics department is not doing well.

We realized we had these old-fashioned departments. The logic was, if you have a terrible department, you could announce that they were not allowed to hire any more people, and a very good department, you allowed them to hire their own people. But then

there were some departments that were halfway in between. And so we decided that the tactful way-- [tape interruption]

--was that we would appoint all the search committees. We had decided in the reorganization we weren't going to fire anybody. That's just too complicated in a university. But we were going to make good new appointments. We had to have some device for making good appointments. If the department has a faculty opening, the standard procedure is to appoint a search committee, because the whole department can't do the detailed work of recruitment.

This was devised as the crucial way to improve the place. The CACB would pick the people for the search committee, usually a couple of people in the department and a couple of people from outside. If it was a very good department, most of the members would come from the department, and at least one person from outside. If it was a poor department, almost all the people would come from outside the department.

Hughes: Didn't you receive complaints from the department in the latter case?

Koshland: We got a few but fewer than we expected. If you want my honest opinion--honest but arrogant--this new approach to faculty recruitment was one of the more important things I did in the whole reorganization. I get credit for getting appropriations for campus buildings from the legislature and all sorts of things, but the most important contributions were the CACB and the recruiting idea. The method we chose treaded a fine line between faculty prerogatives and the need for making a change, between giving too little power to good departments and too much power to poor departments.

Hughes: You said, when I read the charge from the chancellor, that there were implications.

Koshland: [reading a portion of the charge]: "The Council will be concerned with: all aspects of Biology, including the organization and identification of new areas and potential faculty, resources and priorities for their allocation," etc. So that is the wording I had discussed with Rod, which I'm sure he had discussed with the chancellor.

There were certain areas of research that were really very old-fashioned. People were studying ferns in botany--did this fern fit into Category A?--when people in other universities were doing molecular biology on plants and genetically engineering new plants and things like that. And we didn't have anybody doing that. So instead of arguing that we could have a botany department that did genetic engineering, it became genetic engineering exclusively in biochemistry. And people argued that doing genetic engineering was converting the botany department into a biochemistry department. So there was that kind of resistance to change.

Secondly, there was a general kind of resistance, which was caused by old-fashioned people. When somebody retired, the conservative people would say, "We've got to replace good old Joe. We've got to teach his course." My attitude was that anybody in the profession who was very good could teach an undergraduate course. A graduate course, you've got to be up on recent advances in a specific field. But anybody who's good should be able to teach an undergraduate course. Therefore, you want to pick the

people in the forefront of research, and then you tell them, "Okay, you're going to have to teach this course as part of your teaching load." You don't hire people just to replace an old-fashioned course,

My standard illustration used the abacus. In the modern era, you don't want to hire one person in computers and five people in abacuses because we always taught courses in the abacus. We were hiring many too many abacus professors and not enough computer professors. But that's the essence of the change that caused the fuss. We also changed departments and organization, but it was the philosophy that was the biggest change.

Hughes: You suspect that Dr. Park wrote the charge?

Koshland: Those were the changes that Rod and I wanted. The chancellor, Mike Heyman, had to depend on his vice chancellor, Rod Park, for advice. And I'm sure Rod explained it to him enough so that he knew what he was getting into. If you're a good vice chancellor, you say, "There's going to be some opposition to this." But he also said, "We've got to do it."

Hughes: Trow says that it was your August '81 internal review committee report that he believes provided the justification for reorganization that was needed in order to present the problem to UC Systemwide administration and to Sacramento.¹⁷ I would have thought it was this whole body of work, including the external review committee report and what the CACB was doing.

Koshland: Trow was correct. The internal review committee report was a key document,¹⁸ but the external committee's support of the internal committee was important in getting the chancellor (a lawyer) and legislators on board. Is it the one we sent to everybody?

Hughes: That's the one that you think was probably sent to everybody.

More on the Chancellor's Advisory Committee on Biology

Koshland: The advisory council, which we proposed in here, is the most important single thing I advocated. In my opinion, it was clever in terms of organization. You see, the great advantage when you have a professor who is advanced in research, he's on all sorts of committees in Washington and in the country. So not only is he himself very involved in the forefront of new developments, but he also goes to committees and he meets all the other people who are in the forefront of science. So he really picks up what's advanced in the world and who's doing the advancing. That's the kind of person you want

¹⁷Martin Trow, "Leadership and Organization: The Case of Biology at Berkeley," *Higher Education Organization*, Stockholm: Almqvist & Wiksell International, 1984, p. 158.

¹⁸ A. Glazer et al., "Report of Biology Review Committee," April 4, 1981.

constantly thinking about the future of the university. That's exactly the kind of person who won't take the job unless you convince him that his time will be well spent.

So this advisory council was a device, first of all, to get those top people who had big research labs to be willing to spend a few hours a month on the future of the university. Even then it takes a little arm twisting, but at least they're willing to do that, as compared to being chairman or dean. Their specific role is slimmed down mainly to get out the areas that should be covered in the new faculty positions where departments are going to hire new people and to make recommendations of names as to the people who should be hired.

A device which I'm really proud of was that the committee would meet once a year with the chancellor. Why did we say that? The answer is that I wanted to have some device whereby the committee could talk to the chancellor face to face, without going through department chairmen or deans. Because if you say to the dean, "I want a private meeting with the chancellor," the chancellor's not likely to arrange it outside the formal administrative structure if he's a non-scientist and you're a group that's protesting. He doesn't know whom you represent. You might be another crackpot group of professors.

If you are law-abiding and send your report to the chancellor up through the dean, then it's never going to make it to the chancellor. So this device was a way whereby the advisory council legitimately can once a year communicate directly with the chancellor. And don't worry, the deans caught the significance of the arrangement right away. That meant that any dean who then ignored this committee would have to face the probability that the committee would say to the chancellor, "Dean X is really old-fashioned, and we've given him this idea and that idea, and he rejected them all."

As I mentioned, the charge also said, this committee is totally advisory. So implementing everything we did decide on the advisory council had to be handled by the deans. Then it finally dawned on the deans that if the CACB was a good committee, the dean was going to get credit for doing a lot of good things. That's why I said specifically I didn't want the advisory council to have any administrative duties. So on one hand you could say, "The advisory council doesn't want any direct-line function. They're not grabbing power." And we didn't want administrative duties because we were recruiting people who didn't want to have a lot of duties; they wanted to give advice and let somebody else do it. When it dawned on the deans it was totally symbiotic, they said, "Oh, boy! This group is going to be advising, and we're the ones who are going to call up the chairman of a department and say, 'You can recruit in this new area.'" It became obvious to everybody that the advisory council would be good. But at the beginning, the reaction was, you're going to change things; it might be terrible. So lots of people were resistant.

The Academic Senate

Bypassing the Senate

Koshland: I knew, mainly because Rod told me, that the Academic Senate would block everything we did. But I didn't know how to get around it. Rod was smart enough to say what we would do is constantly notify the senate but that we would never consult them. We did do the standard thing of putting your hat on a pole and seeing who shoots at it [chuckling], without risking yourself. At the beginning we had a trial balloon: we did consult the Academic Senate Policy Committee about one of our early provisions. Just as we expected, it took them three months to report back. They were against almost everything. It was a big waste of time.

Rod and I discussed it, and we even sent a report to a second Academic Senate committee, Privileges and Tenure. Same thing happened. By then we knew it was going to be ridiculous to consult any committee of the senate. So from then on, we always notified the Academic Senate what we were doing, but we never asked them for any advice.

Carol D'Onofrio was not the chairman of the Academic Senate at the beginning, but she became it. She was smarter at discovering bypasses than the others. She said to me at some luncheon we were at, "You're being very clever." I said, "Gee, we're telling you what we're up to. What more do you want?" You know, innocence all over my face. She said, "Yes, but you're always notifying us; you're never asking us." Then I just sort of waffled and went on.

Hughes: Rod Park yesterday confirmed what you're saying. He said it was a deliberate strategy to bypass the Academic Senate.

Koshland: It was his idea basically, which was very smart. By sending all these notices and telling them, the Academic Senate couldn't say they didn't know what we were doing, you see. But they would have to overrule a vice chancellor to stop it. [tape interruption]

I will guarantee if we had gone through the Academic Senate there would have been no reorganization. At one point I had eighty faculty members working for me on various committees. I got very good people, like Gerry Rubin, who's very reluctant to get on committees of this sort. And part of the reason they accepted membership was we were really getting things done. Important, active scientists didn't want to be in the Academic Senate because it really doesn't do anything--it comes up with a report, and it's almost always ignored. People are willing to do a lot for the university if they really feel it's going to be implemented. So my promise to people I recruited was that we were really going to make a difference. [tape interruption]

To achieve real reform in the world--this is a pontifical statement by D. E. Koshland--you need a good committee, and you need them to maintain interest. There are a lot of people in this world who become very interested in a subject--all the way from the U.S. Congress

to the University of California to the PTA of your local school. And they will stay interested if they see their ideas are being used. Rarely, even if you have a brilliant idea, does it get implemented. To do so, you have to build up allies. If people who are not good administrators then fail to follow up, the people with good ideas lose interest and go back to their individual efforts.

So anyway, I heard some gossip late in the game when we were ready to implement the reorganization that the Academic Senate was going to turn down our plan--not really turn it down--but delay it by not letting us announce it in the catalog. I fortunately knew somebody named Professor Sandy Muir, who's a very prominent political science faculty member, not a scientist. We were friends. We had been on committees together. He was on an Academic Senate steering committee and told me about this. Anyway, Sandy headed it off, and our reorganization plan went through.

That event was an indication that Rod Park had bypassed the Academic Senate. Carol D'Onofrio was head of the Academic Senate. She apparently felt that these maverick people had gone off on their own.

Hughes: I found a letter from D'Onofrio.¹⁹

Koshland: Which says?

Hughes: She's writing concerning "consequences of Senate non-involvement in discussions to date about the reorganization of biology." And then she goes on: "...the Berkeley division has not formally been asked to review specific proposals for the reorganization of biology...[M]any view the course of events to date as a profound break with Berkeley's strong position of shared governance. Indeed, it is widely perceived that the changes taking place in biology represent a 'coup' engineered by the Administration and certain faculty members of the biological sciences. This group has been referred to as an oligarchy and an 'outlaw government'."

Koshland: [laughing] That's right.

She is not quite correct. She said she wasn't informed, but we consulted them [the Academic Senate Policy Committee]. The policy committee came back after a couple of months--a long period of time. If we had waited--but in fact we just kept going with the reorganization.

It was clearly evident that if we had to go step by step through the Academic Senate, reorganization would never have gotten done. As I said earlier, Rod said, "We got their attention"--reorganization really was important. Everybody thought reorganization was an exciting event that was going on. The Academic Senate committee was not on a realistic time schedule. Rod and I were very worried--at least we discussed it--that maybe

¹⁹ Carol N. D'Onofrio to Roderic B. Park, September 29, 1987 (University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

the Academic Senate would appoint a committee to look into the reorganization plan. That would have stopped everything dead in its tracks.

Hughes: But they didn't do that.

Koshland: They didn't do it.

The next thing was, the Academic Senate has to approve the courses, changes in the catalog, so that keeping good relations was a very important move.

Hughes: There's D'Onofrio's letter to you, and here in my other hand I have your response.²⁰

Koshland: Because reorganization would move professors around in departments, they could argue, Privileges and Tenure really had to take it up because now you were hired by one department and you were going to be judged for tenure by another. So it could have taken years for senate approval.

Hughes: She says in this letter that there's a procedure for creating and dismembering departments.

Koshland: Correct.

Hughes: Which you hadn't followed.

Koshland: Yes, we had had all these meetings with faculty about the reorganization plan. She couldn't really say we didn't tell the faculty. But the procedures of the Academic Senate dealt with getting rid of or creating a new department. We were saving seventeen departments and creating new ones with no simple one-to-one basis. That would have taken decades by Academic Senate procedures. And second, I think Rod said to me privately that the senate will never turn down something that 80 percent of the relevant faculty is in favor of. And we were pretty sure that it was more than 80 percent.

However, this plan for reorganization--I've forgotten the exact events--had to go to the Regents [of the University of California] at some point. Establishing departments, I think, has to be approved by the Regents. I remember being worried at the time, but Rod wasn't that worried. Rod said to me when we broke ground for one of the new buildings that we were making facts on the ground and the senate would have to go along.

²⁰[Daniel E. Koshland, Jr.] to Carol D'Onofrio, February 9, 1988 (UCB University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

Scientists' Opinion of the Senate

Hughes: My understanding is that the Academic Senate on this campus has a reputation for being a very strong organization.

Koshland: Yes, a very powerful force.

Hughes: So this took some doing.

Koshland: It took some doing. But, on the other hand, the Academic Senate is not considered as powerful an organization to the scientists as it is to the humanists.

Hughes: Why is that?

Koshland: It's because your interests in the sciences are elsewhere--making your reputation in the world. To be appointed to an Academic Senate committee in the humanities is a big thing. In our department, very few people will even accept a committee appointment.

Hughes: It is a diversion from their science.

Koshland: A real diversion from science, right. At other universities, the reputation of the University of California Academic Senate is of a very powerful force that the chancellor must bow to. So it really was something.

More on the External Biology Review Committee

Koshland: It was important in this case that the internal committee prepared the groundwork and followed up with adjustments to the plan. But then we knew that when we had the final plans, we had to have some outside group come in, mainly to give the chancellor a good argument based on outside peers to present to the rest of the faculty. The outside committee was such a good committee, they had our respect as well as that of everybody else's. They had the respect of people based on their personal credentials and the positions they occupied; they were very distinguished people. They were chairmen of departments, so they could say, "This is the kind of thing that works, and this is the kind of thing that doesn't."

We had them come back for a final report after we had had all these faculty meetings,²¹ and we had taken account of the critiques of our plan. Remember, that was a big change to ask the chancellor to go along with, the abolition of many departments, and then we asked for two new buildings and the renovation of a third.

²¹"Biology at U.C. Berkeley," Report of the External Review Committee, February 26, 27, 28, 1986 (University Hall documents, folder: Biology Reorganization, Reports & Other Documents).

Of the few people who still complained at the end, almost nobody wanted to go back to what it was before. They might have argued a little about the details of the reorganization, but if we had had to ask for a vote at the end, "Do we want to go through with the reorganization or go back to what we had before," I don't think anybody would have voted to go back. So we really had the support of the faculty by that time.

Hughes: Yet this really was an end run, was it not?

Koshland: Oh, yes.

Reorganizing the Academic Program in Biology

The First Reorganization Plan, December 3, 1984

Koshland: And then the question was how do we do it? The advice I got from a number of people was the way you should do this revision of the academic program is you go around to the senior people in the university and explain your plan to them, and then, when you've got a fair amount of support, you break it to other people. This was going to be a big, big shock--which it was.

I wish I could say I was brilliant and saw this was not the way to do it, but I didn't. I was doing my research; I was publishing a lot of articles, and I didn't want to spend a lot of time talking it over with everybody. So I said we'll just write up our plan, send it to all the affected faculty, and we'll write in large letters on the first page that this is a draft of a possible reorganization plan for Berkeley, and we welcome faculty criticisms. We had heard of cases where the department chairmen who didn't like a proposal didn't even send it around to the members of their department. So we decided to avoid that problem, and we'd just send it to every faculty member directly. So we sent the whole original plan to every biology faculty member of the Academic Senate. You can imagine what happened.

I don't think many people knew that we [CABC] were in existence. They sort of knew that Rod had appointed this committee to think about reorganization. Nothing was kept secret, but people are busy and never really hear about things. And so, as a faculty member, you get on your breakfast table an outline in which your department has been abolished, and you've been assigned someplace else.

Hughes: Dan, is this it? I have a thirty-seven-page document dated December of 1984.²²

²²Chancellor's Advisory Council on Biology to Chancellor Michael Heyman & Vice Chancellor Roderic Park, December 3, 1984 (University Hall documents, Reports & Other Documents).

Koshland: Yes, this is probably it. This was from the Chancellor's Advisory Committee, the big committee that approved the final plan from its subcommittee.

Hughes: You see that the plan is very, very detailed. There are general statements, and then there are plans for the reorganization of specific departments.

Koshland: We really made it detailed.

There were three or four of us who really did the reorganization. We amalgamated a number of departments, eliminated some departments. We put the botany department, which had department offices and labs in the Life Sciences Building, together with the botanists in the College of Natural Resources. And they, at the beginning, really largely hated each other's guts. The College of Natural Resources people were out in the field, and they felt the academic botanists were totally impractical dreamers, ivory tower types that weren't any help to the farmers. And the botany people in the College of Letters & Science, who were much more the well-known botanists, thought the College of Natural Resources was just farmer types and not at all academic or prestigious.

Today, they're getting along fine in the College of Natural Resources. But at that time, the idea of amalgamating would have sounded terrible to those involved. Our plan put them together. That was pretty radical at the time. We eliminated the Department of Zoology, picking all the people who were interested in whole animals and putting them together and calling it the Department of Integrative Biology. We eliminated the department of biophysics. I won't go through the details, but we made a lot of radical changes. And then the small group brought all these ideas back to the advisory council, which then made some further suggestions.

Approach to Improving Departments

Koshland: One of the early ideas was to create some really strong departments by taking all the good people and assigning the people who are not so good to some "garbage department." One decision at the beginning was that nobody was going to get fired. Departments would gradually get better by getting new good people after losing people by attrition. Secondly, we decided it really was wrong to put less good people in a "garbage department," even if we could define them. The departments always had a certain number of "stars" and a certain number of "dogs," and it was unfair to give good departments all the "stars" and poor departments all the "dogs." The "dogs" had to be distributed somewhat fairly, and the "stars" had to be distributed the same way. We were trying to make every department as good as it could be.

Starting with a Written Plan

Hughes: The subcommittee made most of these decisions?

Koshland: Yes, initially. But we referred the final plan to the full committee, which polished it further. We then sent this first reorganization plan to the entire biology faculty, not just chairmen or special people. And it caused an enormous reaction. People denounced me. I was walking in the middle of the campus, and my friend Gunther Stent was near the Campanile--a small figure in the distance. He started running down, waving his arms, shouting, "Dan, you are doing a terrible thing. It won't get anywhere." Gunther eventually supported the reorganization and was very helpful. But at the beginning he didn't like it.

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Koshland: I said to Rod at one point, "Maybe we shouldn't have done it this way, but I was just too busy to do it any other way." As I told you, he said, "Well, Dan, you certainly got their attention." That was a great understatement. A university is very busy, and everybody is going about their own business. In something like this, it was worthwhile getting everybody's attention at once instead of one pocket of faculty learning about it in January; another pocket gets around to it in April, etcetera. By giving everyone the same document at the same time, everybody was talking about it at once. It was big excitement.

Hughes: Talk about your idea of starting with a written document, instead of negotiating with the faculty first.

Koshland: We really started with a written document, this 1984 plan, partly because Berkeley is so big. To try to explain the contents of that document to individuals one by one would have taken ages, even for a small group. To think of explaining it to a large fraction of the faculty was even more daunting. The other problem that occurred to me was, whom do I select? If I select a very small group of highly influential people, then a lot of people don't know about it. And if I select a larger group and anybody is left out, they're going to be very hurt and say, "Am I not important?" So it really was just a lot easier to write it all down and send it to everybody. As I said, the idea of doing it wasn't sheer brilliance. It was sort of a compromise between the amount of time I was willing to spend and some good arguments that it was more democratic.

Opposition

Hughes: One downside of this approach that I picked up by looking at the documents was that some people apparently accused the CACB of not just thinking about reorganization but having done reorganization.

Koshland: That's right. I've forgotten exactly what we said in the cover document, but we said something like, "We invite your comments, and we'll change the plan based on your suggestions." People said to me at that time, "Dan, we're not fooled. Anybody who turns out a thirty-seven-page, single-spaced document--it's all decided. That's the way the university does things, and it's just token democracy that you're pretending to consult us."

I think the first thing that I would like to mention is, it really was good of the vice chancellor, Rod Park, to support the reorganization plan. I think the whole thing could have been aborted right then because of the amount of furor that it caused--it really did cause a big furor. If we had had a conservative administrator, the plan would have died right then. Most people would say, "We just can't do this [reorganization]. There are just too many people opposed." A third of the faculty in biology were opposed. Remember, there were three hundred to four hundred biologists when we did the census. That's a big fraction of the Berkeley campus, with a thousand professors. A lot of them were very angry, and a lot of them had been colleagues of Rod Park in the botany department and I knew had a first-rate "in." They could call up Rod at any point, and some of them were among the angrier ones.

We countered this as much as we could. We said, "Write in your criticism, what you want changed, and we'll consider it." We got a certain amount of flak from people who said, "You're not going to change this plan. It's all written out, and you're not going to make any changes." A lot of people did complain, and--I should be fair--there were a lot of faculty that thought the plan was wonderful. I remember one person, Fred Wilt, who was a voice that I loved because he said, "Dan, this is the most exciting thing. Since I've been at Berkeley, nobody has proposed anything as big and exciting and new as this." So there were a number of people who really liked it.

My department, which was in a new building and on top of the world, was sort of resentful of the reorganization plan because it leveled its lofty status. My wife's department [immunology], which was in the rickety old Life Science Building and having a hard time recruiting, liked the idea because there was going to be a big change.

Meeting with the Biology Faculty

Koshland: We decided one of the first things we'd do would be to get all the biology faculty together in a big room and talk about the plan. They could comment on anything they wanted. My experience was influenced by the fact that I had been president of the school board in Long Island. It shows how experiences coming from all sorts of directions help. I discovered that having a general school meeting and letting everybody speak usually ends up in chaos. But everyone learns of all the other opinions and realizes the school board has to come to a decision that won't please everyone.

I figured that would be much the same thing here. And so we had a meeting, and I remember specifically telling Rod that he didn't have to come. I figured he needed to be

sort of like a supreme court: when all the fights were over, somebody had to decide. So I wanted to preserve his unbesmirched reputation. Rod really wanted to be there. He was wonderful. He answered questions directly and took a lot of the heat. It was very good he came, because even though he got accused of letting us go ahead, he left the clear opinion that the reorganization was going forward and the faculty should cooperate or their opinions would get lost.

Hughes: This was the first faculty meeting to discuss reorganization?

Koshland: This was the first meeting. What we had announced was a meeting on the reorganization--anybody on the biology faculty could come. Remember, we weren't a department then; we were biologists in many departments and four different colleges. It was a free-for-all. Everybody spoke up.

As a result of that meeting, we appointed a second subcommittee to make corrections in the first draft. I remember Bob Zucker was on one of the committees, and we called it the "second iteration." They were really a subcommittee of the original committee to reform the original plan. It came up with a second report, and after that was circulated to all the faculty, we had another meeting and another subcommittee, which we called the "third iteration." The people who were infuriated by the first iteration got some of their way in the second iteration. But then a lot of the people who had been enthusiastic in the beginning didn't like the second version. So there was still a lot of fuss.

Affinity Groups

Hughes: Was the disagreement mainly in terms of the affinity groups?

Koshland: No, differences ranged all over the map. There was a little difference between Rod and me about choice of affinity groups. Rod said we should let people vote themselves into their own preferred affinity group. I was more for assigning the people on programmatic grounds and letting them argue if they felt we were wrong. There were affinity groups in the sense that we put the zoologists together and the biochemists together and the geneticists together. There are people who just obviously fit in those categories. But there are other people right on the borderline; you don't know whether he's a geneticist or a biochemist. So you had to adjust the arrangements. So it became a mixture of what Rod and I had suggested.

Hughes: Affinity group meant affinity by discipline, right?

Koshland: Right. And use of equipment.

Hughes: You modified the concept of an affinity group because of practicalities?

Koshland: The affinity group, as I said, was never that dominant a theme in the sense that we never defined affinity group precisely. It was something Rod pushed and something in the back

of all of our minds. But I think Rod's idea was that people would just say whom they wanted to be with. That was a little too vague, so we had to assemble people by discipline. But we did have many exceptions and we did take people's wishes into account.

The disagreement was mainly from people who didn't want to be in the group to which they were assigned, or they felt their group was too big, or they wanted a different mix of disciplines. The net result was that the affinity group concept was modified. Then some people who liked the original plan didn't like the modification, and so I think we finally went through three iterations.

Hughes: Four.

Koshland: Four?

Hughes: At least there were four meetings to which the faculty was invited.

Koshland: Okay, well, whatever--it was three or four. I think there were three written plans.

Hughes: Louise Taylor told me that faculty attendance at the meetings on reorganization gradually declined.

Koshland: Declined, yes. People were getting happier with the procedures after each iteration, so there was less vocal opposition. Our subcommittee was very good and really modified the reorganization plan to improve it. The fact that we did change in response to the criticism also mitigated the charge that we had no intention of changing.

The third iteration brought it back closer to the original plan than the first iteration. In other words, our original ideas were really pretty good. It was quite clear when the second iteration people made some changes that were more radical than was wanted. Then other people saw they weren't so good and changed them back closer to the original. But there were real changes that improved the plan.

Merging Biochemistry and Molecular Biology into a Mega-department

Hughes: You mean you took Professor A out of Group B and put him or her somewhere else?

Koshland: For example, I remember in the beginning, we did keep molecular biology and biochemistry separate. Then it became very logical to merge them. Those were two of our better departments. We had decided it wasn't logical to put two of our best together. But then in the final plan we decided to merge them. This was a big decision for the following reason: biochemistry and molecular biology were two of the bigger departments, and they already filled up buildings. We had no building to put both of them in. We finally decided, educationally and structurally and organizationally, it was better to have them in one department. They would be in two buildings, but they'd be

one department. That meant we had to think of ways of running departments even if they were in separate buildings.

Another change was that we had people up in Stanley Hall doing animal experiments, and we were told by the animal care committee that we couldn't possibly have everybody in every department using animals. The new rules required animals to have very special care, and they had to have them in central buildings. So we had to change the plan from that point of view and put all the animal facilities at the other end of the campus. So there were really substantive changes from the initial plan. [tape interruption]

Hughes: Why put molecular biology and biochemistry together?

Koshland: Well, we said they went so logically together in terms of interest and common equipment. We were concerned that it was too big a department, and then we decided that wasn't true; it was better to have a big department. And then people argued, well, the genetics department wanted to be in the molecular biology and biochemistry department because it's very similar, and genetics needed the association with stronger departments. So then the department became even bigger. I remember one of the criticisms of the plan: people called it the mega-department, which is the department we now have, which is a hundred people. That is the biggest department on the campus. The argument was, "it was ungovernable; it was too big."

Hughes: Isn't molecular and cell biology an affinity group?

Koshland: That is an affinity group. I think the idea was that it included all the people using ultracentrifuges and molecules and that kind of equipment versus the people using cages and whole animals. If you define affinity group by concepts, it's one way; and if you define it by equipment, it's another way. In fact it was good to have people together who thought along similar lines because they read the same literature and they had more reason to talk to each other. But in fact, there were people using cages who were all the way off in biochemistry, and there were people using ultracentrifuges all the way in genetics. So even though you had general concepts, you finally had to decide that Professor X, who was a geneticist, belongs in the biochemistry group, rather than in the population genetics group.

At the beginning, I thought we would keep the old department names the way they were and lump the departments into a division. Our feedback from the faculty was they didn't like that. They wanted to call the overall thing the department and then call for divisions within the department. Some explained it to me in the following way: I want to be in the department with Professor X, who is very well known; being in the same division isn't the same thing. The outside world uses "department" as the smaller unit, like biochemistry, and "division" as the amalgamation of departments, like Letters & Science. So people wanted to be in the same department as the stars.

Several Iterations of the Academic Reorganization Plan

Koshland: We had the third iteration, and we got a pile of letters back. We always invited letters. Each person on the CACB got a packet, so each member of the council could read these letters. At a meeting after the third iteration, the members of our committee were all a little depressed at the number of people who wrote in criticizing. I came in late, hadn't heard the conversation, and dropped my packet on the table and said, "Well, there's nothing important in that stack." And everybody burst out laughing, because they had all been depressed. What I meant was not that it wasn't important to answer each person, but the criticism was all by that point about petty things that we'd expect from many individuals. These were a myriad of little problems that had to be fixed up, but it was quite clear that the general principle of reorganization was now pretty well accepted as there were no criticisms of the general structure.

At one other subsequent meeting I said, "Well, I think 90 percent of the faculty is now behind the reorganization." And I think Beth Burnside corrected me and said, "Dan, you're wrong; it's 95 percent." So it was pretty clear to everybody that we were on our way in the final lap. The adjustments we made in the iterations were very important to people, so they were real improvements. The mergers and movement were not just cosmetic changes; they were substantive changes.

Support from the Chancellor and Vice Chancellor

Hughes: There's an oral history in progress with Dr. Heyman in which he says that he bought the idea of reorganizing the biological sciences. Why was he supportive?

Koshland: I think we made a good case. I think biology is easier to sell in the modern era than, let's say, physics. Physics was easy to sell after the atomic bomb. You had to have physicists around to keep ahead; otherwise, other nations would get ahead. The Russians were now getting the bomb, and they weren't very friendly. The same need is true nowadays for biology. It was pretty obvious that health and disease were becoming very big factors in the life of everybody, and therefore biology was a pretty good sell. It's even more in the headlines now.

Heyman is a smart person, so A) he felt that it was a very important subject, B) a good fraction of his campus, I think 30 percent or something, were biology professors, so it was an area that he knew was important, and C) I think we did a pretty good job of selling him that we really were archaic in certain programs and needed them improved. And then we had some physical plant inadequacies. The age of LSB was the same as San Quentin. San Quentin was so dilapidated, you couldn't ask prisoners to go there. We were asking students to go in a building that was equally old. So anyway, we exploited the negative that the buildings were old and the positive that our new program was good.

The mood changed from a majority in opposition to a majority in favor about the time of the second iteration. It became pretty obvious that reorganization was going to be achieved. Part of the reason was that the vice chancellor was just very good. He didn't meddle individually, but he made it clear in private discussions that reorganization wasn't some crazy idea of a couple of nuts, that it was something the chancellor was really behind.

Hughes: Did he do that by relying on his own network of science colleagues?

Koshland: No, I don't think there was any one network. Rod was very supportive, right from the beginning. I urged him to be less supportive because I thought he'd damage his credibility, that he'd be considered too much on our side. He was much more willing to be there early than even I thought was a good idea. But in retrospect, he was right and I was wrong. But what I did say to Rod was that anybody who complained should be allowed to go to him. Rod was a little concerned that I would worry if people were encouraged to go behind my back. I said, "Absolutely not, Rod. I want everybody to be able to complain to you. This is a free speech area. We're not going to fiddle around with that." He suggested letting everybody come to him, but that he would always inform me of the criticism and preserve the anonymity of the complainer. We used that system.

For example, if somebody said, "Well, Koshland is doing everything just to help the biochemistry department." Rod would sometimes answer just from what he knew, but sometimes, if he didn't know, he would call me and say, "I heard this gossip." But he'd never say which professor had said it, and I didn't want to know. I just gave my answer and let him handle it from there.

Hughes: And then what did you do?

Koshland: I had plenty to do. I didn't have time to go around answering all gossip, so I just depended on Rod. Sometimes it worked in reverse. If I heard various people were criticizing him, I'd give him what I called my early warning system. I'd call up and say, "They're saying this and this about you. I thought you'd like to know."

Hughes: And then he would take steps to--

Koshland: --counter that.

Importance of the Koshland-Park Relationship

Koshland: The informal relationship that I had with Rod was very important to the success of the project. His willingness to stand up to criticism and the fact that he was honest with me, and that we both could take criticism without reacting, "I want that guy fired," really

helped. That's the kind of informal factor that doesn't appear in Trow's report.²³ I think in many of these things, the personal relationship between the principals becomes very important. Rod had to handle the reorganization from the governing structure, the Academic Senate point of view, and I had to handle it from the faculty's. It's just better if a faculty member communicates with other faculty members, and it's better for an administrator to go around with other administrators.

Hughes: Had you known and worked with Roderic Park before this?

Koshland: No, not at all. It worked out wonderfully, and we've become very close friends. One might say, reorganization would have worked with anybody as vice chancellor, but I know it wouldn't have. The people who followed Rod were one named Kui and a second named [John] Heilbron.

If either [Ken] Kui or Heilbron had been vice chancellor instead of Rod at the time we started, it never would have worked. And--I'll be honest--I don't think it would have worked without me. You needed a faculty member who was willing to spend time on the reorganization. The faculty member has to be pretty prominent. In other words, I had to have credibility as a scientist, and the fact that I was a member of the National Academy and things like that was a big help. I had, in addition, to be willing to go around and talk to people.

Hughes: What about having a well-placed spouse?

Koshland: That was very important. But I got feedback from a lot of people of whom she was the most important.

Hughes: She was another pair of ears.

Koshland: Yes, she was, and a very honest and perceptive pair of ears. I'd come home and I'd tell her what happened during the day, and she'd tell me, "You should do that over again," or "You made a mistake here." She was very wise and very helpful.

The Construction Phase

The Building Plan

Koshland: We then had to go to the legislature, and we got approval for the building funds.

Hughes: Who is "we?"

²³Trow, pp. 148-178.

Koshland: We ought to go back one minute: Mike Heyman entered into it very importantly on the overall scheme. Of course, Rod was the person I dealt with day-to-day. Our committee finally decided that we needed two new buildings and to renovate LSB. The ideal plan was to tear down LSB and build two new buildings. I've forgotten how expensive it was, but it was plenty, over \$200 million. And then there was a middle plan, which was the one in which we would build two new buildings and refurbish LSB. And then a minimal plan, where we took people out of LSB and sort of refurbished it.

What we said to Heyman was if we did the minimum plan, we were going to lose faculty and would not get very many good new ones. That would really be a disaster for Berkeley if it still wanted to be number one in the country in biology. And the ideal plan, we were pretty sure, was going to be too expensive. So we recommended the middle plan, and that's what Heyman went along with.

The committee came up with a building plan and a program plan, and the two were inextricably related because we knew we'd never go to the legislature and get the money just to fix up the buildings. You had to have an exciting new program. The legislature always says, "Berkeley has got plenty of money; we don't need to give them any more." [tape interruption]

One third of the money was to renovate LSB, the idea being that you would build the two new buildings and move a bunch of scientists into those buildings, and then move the people out of LSB into temporary quarters, refurbish LSB, and then move the people back in. And that's what was done.

I remember I was called by the president's office--Bill Baker, I think, who told me the people on the governor's budget committee in Sacramento would give us two-thirds of the money if we'd raise one-third. I was then called into the chancellor's office, and I presented that proposal to him, thinking this is the end of it. He's going to say there's no way Berkeley is going to raise \$50 million or something like that. A public university had never raised anything close to that amount. I remember Heyman jumped up from the table and he says, "All right, let's get it."

Hughes: Really!

Koshland: Just like that. I mean, it was just wonderful. I reported this to people all over the campus.

Rod also did something else clever near the end when some faculty were still dragging their feet. He put up a chain-link fence around the construction site and tore down some trees. It sort of indicated it was a fait accompli; you can't stop it now. By then, the biology faculty was pretty much in favor of the reorganization plan, and so we were really over the hurdles.

A Shift in UC Fundraising Policy ##

Koshland: The chancellor not only said, "Let's go," but he really started putting fundraising on a professional basis. I think that is a blessing of the biology reorganization, that it really started a major public institution depending more and more on private support. But that was the dramatic shift.

Hughes: Was it?

Koshland: Oh, yes! Berkeley had always had little alumni groups asking for support, but this was a major undertaking.

Hughes: Before that, the state was the main source of UC funding?

Koshland: Yes, but in the 1980s state support was constantly dwindling. This was the first time anybody really got enormous commitment to raise a major part of the money from outside the campus.

Hughes: Did you go to Sacramento and lobby?

Koshland: I've forgotten whether I went once or twice. I remember a guy in Oakland offered to fly me up in his private jet. I refused, and it turned out he was a very big developer who was trying to influence Willie Brown, who was the speaker in the legislature. He wanted to get something out of Willie Brown. I turned him down because it was easy to drive. It's only about an hour away. I just felt I wanted to have a car and be able to get out on my own time table. But it would have been very bad if I had gone up in a plane with him.

Dealing with the California Legislature

Koshland: Heyman, who was chancellor of the UC Berkeley campus, was the main person dealing with the legislature, and Heyman's relation with the legislature was excellent.

Hughes: Why was that?

Koshland: He was a good liberal Democrat. He spoke their language. He's a very personable person. David Saxon, who was a physicist, was president of the UC system at the time, and he had terrible relations--

Hughes: With the legislature?

Koshland: Terrible relations. A lot of the legislators would call up and get advice from Heyman on things in relation to the university when they should have called Saxon. The legislators didn't like the chancellors of each individual campus coming up to them and lobbying, so they made a rule that it was only the president of the whole university that could talk to

the legislature. But you couldn't stop an individual legislator from phoning Mr. Heyman and saying, "What's going on here?"

I really wasn't involved in that, so I don't know for sure. But I talked to enough legislators and people at the governor's office, and they all said that Heyman got along very well with the legislators. This was money in the bank of cordial relations for the approval of the final project. Those relationships held us in very good stead.

Hughes: How hard was the proposal to sell to the legislature?

Koshland: Oh, it was very hard. At the time, Jerry Brown was governor, and he was not very friendly to the UC campus. That was well known. When Brown started out, there was a state deficit. Certainly it didn't look very good. But there was one big advantage I didn't realize. What we were proposing was a construction program that would have taken up the whole university allotment for buildings and capital campaigns. What had happened was Berkeley in the previous ten years had asked for very little money from the legislature.

Hughes: Trow made the point that one of the reasons that biology had slipped at Berkeley was that the UC system as a whole was putting its attention on building up the new campuses.

Koshland: That's correct. Berkeley had built very little in the previous ten years. As a result, when we came along with this project, it was considered by the other campuses as only fair; now it's Berkeley's turn. Remember, you had to do two things: You had to convince the president of the UC system to back this thing, and then the president of UC and the local campus had to get it in the governor's budget and go to the legislature and argue for it.

Hughes: How were Heyman's relations with Saxon?

Koshland: I think they were good enough. Near the end, it switched to [President David] Gardner. I'm not sure Heyman's relations were always great with Gardner. At least I heard rumors. But good enough.

Hughes: Getting Gardner's support wasn't a problem?

Koshland: That wasn't a problem. Gardner certainly felt this was a good project, and he was completely behind it. I think the person who presented it and who had a key influence in Sacramento was Heyman. That was very lucky for all of us.

The other person who was key in the legislature was Bill Baker, who was in the [UC] president's office. He was the one that gave me all the advice about dealing with Sacramento. Heyman really did it in the final persuasive presentation. I never dealt with any of those people, although they came down here and asked us questions. But the rules were, I didn't go up there independently, only chancellors and presidents dealt with the legislature. But we had to provide him with background.

Hughes: Did the CACB provide the background data?

Koshland: Yes, and we had to have new special data with new committees once the campus approved the project. Then, when we started to meet with architects, the CACB couldn't be at every meeting. So Rod would delegate it to me, and I in time got Alex Glazer to head the faculty committee on construction. With information I gave him, Rod appointed Louise Taylor to be the administrative liaison for the whole project.

It went sort of fast. They announced a legislative committee was going to come down in two months, and we had to have a presentation--how much the buildings were going to cost, what professors were going to go into this building. Those things all sound very easily done, but it's not so easy. [tape interruption]

A Dinner with the Governor

Koshland: I hosted a dinner in San Francisco with Governor Brown and Willie Brown and Julius Krevans. Krevans was chancellor at the time of UCSF. The person who arranged it for me was Bill Coblentz.

Hughes: And who was Coblentz?

Koshland: Bill Coblentz was a regent and a very prominent lawyer in San Francisco, who was also very prominent in Democratic circles. He was very, very helpful. He's a friend of mine and a distant cousin. I wouldn't even know how to go about getting the governor and Willie Brown to attend a dinner with a bunch of professors, but Bill arranged it. Governor Brown came in late. He had met Sargent Shriver that day, and he proceeded to describe how Shriver had said there was a new development in--remember, this is 1980--computers such that kids--for example, in Africa--wouldn't need to learn to read and write; they would just learn computers, so that in this modern age, we wouldn't be teaching kids in Africa the same way we were teaching kids in the United States. Everybody was very skeptical of it. He was going on and on.

It was quite clear that the tone was kind of condescending about the kids in Africa. I could see on my left that Willie Brown was getting madder and madder about the whole thing. It went through my head that I had to stop the governor but here's a little professor at Berkeley, and it wasn't my role to interrupt the governor. On the other hand, nothing was going to get through the legislature if Willie Brown didn't want to do it.

This dinner wasn't to propose the plan; it was to get people together and talk about the background a little bit. I could see the plan was going to hell in a basket. At this point, Julie Krevans spoke up right across the table and said, "Dan, I want to tell you about a great program we're doing here at UC San Francisco." And he talked for about ten minutes about some cardiac clinic. But I knew exactly what he was doing. He was just so smart. He figured out there was no way that I could interrupt the governor because I had a program which needed his support. He had no immediate program. He could afford to have them furious at him for a brief period if he appeared like a bore who wanted to talk about his thing. And he talked just long enough so everybody forgot what

they had been saying. We eventually resumed talking about something totally different. I said to him after the meeting, "Julie Krevans, anytime you want to ask anything of me in the future, I'll do it for you. That was such a wonderful thing to do."

Hughes: Did Krevans admit that he had wanted to interrupt the governor?

Koshland: Sure, he knew exactly why he did it. It was just wonderful. I knew Willie Brown was about to get up from the table and walk out. It was really funny how well Krevans did it.

Hughes: So Dr. Krevans saved the day.

Koshland: Dr. Krevans saved the day.

Finding Support from the Legislature

Hughes: You said you went to Sacramento once, but I gather Heyman and Rod Park were handling the legislature.

Koshland: Yes, I don't think they went that many times, either. The main person who did the Sacramento thing was Bill Baker.

Hughes: Who is he?

Koshland: Bill Baker was in the president's office in University Hall in Berkeley. They've now moved to Oakland. He was legislative aide to the president of the university, and he was very, very helpful.

Baker really knew all the ins and outs. In spite of the fact that [Governor] Brown was sort of lugubrious: he didn't have the money, and he clearly didn't want to help the university. But Baker would come back and tell me that he knew people on the staff of Governor Brown who were Berkeley grads, real aficionados of the university. Brown is a person of low attention span, so that he wouldn't even know the details of the budget till almost a week before. The governor would have to present the budget to the legislature. The staff, with many UC loyalists, would include this big construction project for the University of California at a stage when it was very difficult to turn down.

Baker knew how to maneuver. The state had committees, and the people in the governor's office came down and visited us. Essentially we got the plan almost done, including the money and what the buildings were going to be, without the governor ever even seeing the plan. It wasn't concealed from him; it was just routinely done, and they just didn't discuss it very much.

What happened was--and this was Baker's cleverness--it finally came to the governor a very short period of time before the legislature for the whole statewide appropriations met, of which, you can imagine, this was a large amount of money to us but quite small to

the whole state budget. So Jerry Brown was not going to hold up the whole budget just for this little item for the University of California. His advisors all said yes, it's the right thing. I don't know whether he opposed it or not, but whatever happened, he went along with the process.

Hughes: What is there to say about the construction?

Koshland: Well, the actual construction was also something I thought Rod handled very well. Everything, except the animal facility, was more or less routine. Rod had the area where the construction was going to be down by Oxford [Circle] fenced off well ahead of time. With no announcement, all of a sudden there were bulldozers and they were doing it. So it was a fait accompli very quickly. As a result, there weren't any sit-ins.

Diffusion of the Molecular Approach

Hughes: We've talked a lot about the internal pushes for this reorganization, but what about the external ones? Namely, the flowering of the biomolecular sciences and the growing need for interdisciplinarity.

Koshland: That's very important, and I'm glad you brought it up. Molecular biology, the recombinant DNA revolution, was one of the things that I felt we had to introduce into some of the departments. Particularly botany, integrative biology, and zoology were way behind the times. Since molecular biology really started in biochemistry departments, the organismal (whole animal) people identified this field as biochemistry. When I said I wanted people doing molecular biology, they thought I was trying to impose biochemistry on everyone in the biological sciences. A lot of the older people thought Dan Koshland is just trying to convert all of us into biochemists.

I responded by using computers as an analogy. "What you're saying to me is, we've had the abacus for years, and we want to continue to use the abacus. I'm asking to replace those old standard things we've had for years with a new fad called computers. What I'm saying to you is this new molecular biology is just like computers. It isn't a science by itself; it's a way of doing science. It's a new, powerful tool that will be useful across the entire range of biological science. You're going to be studying things like populations and lineages using molecular biology, not the way the way I'm using it. But you're going to find DNA and those things very useful to your goals. You've got to start getting people in that area in your department or Berkeley is going to be left way behind."

Hughes: How receptive were they to that argument?

Koshland: At the beginning, there were some people who were dead-set against it, but there were others who were for it. One of the people on our advisory council [CACB] was James Patton, who was a professor of zoology. He was all for doing molecular biology. In fact, that department held out longer than almost any other department, but now they're doing molecular biology all the time. Some of the foresighted ones saw that they had to start

doing it. But I wasn't trying to impose biochemistry on them. I just wanted them to be more modern.

More on Faculty Recruitment Policy

Recruiting in New Fields

Hughes: Louise Taylor said that one of the premises of this reorganization was that because Professor X had left or was retiring, didn't mean that you necessarily appointed a new person in that very field.

Koshland: That's correct. So now we come back to the advisory council and appointments. What we used as conditions were, we, the CACB, would name the members of the search committee. If we proposed radical people, the dean could always say no. Moreover, the department was making the final decision on the appointment. They could say no to a proposed member of the search committee. But they would have to give a reason, and the dean could overrule them. In fact, it never arose; I mean the department never used a veto against a search committee.

Moreover, what we asked is that the search committee report back to us. You generally have a list of candidates, and you decide to offer the job to number one, and if he doesn't take it, you offer it to number two. So if the department made a preference list of A, B, C being one, two, three, and the search committee made a list of A, B, C being one, three, two, we didn't worry. If the search committee was totally out of sync with the department, the advisory council would look it over and say the department is not doing very well. But mostly what happened was that the department and search committee agreed.

The advisory council never imposed anybody on a department. We always said the department has veto power. However, if the department chose somebody we didn't want, we felt free to recommend that the faculty position we preferred be given elsewhere. So the department would lose the appointment. They couldn't argue we were imposing somebody on them that they didn't want, but if they weren't willing to be modern, they wouldn't get a new appointment.

But the main conflict was about going into new scientific areas. Let's say we didn't have anybody on the campus in developmental biology. We said, "Okay, we'll give the botany department a chance to recruit in developmental biology." And if botany said they didn't want to, we just said, okay, the zoology department will be developmental biology. So then the botany department would lose the chance of going into it, and they'd lose the FTE [Full Time Equivalent position]. There was no obligation to replace Professor X with someone in the same field. The rules on this campus are if somebody doesn't get tenure in department X, then the FTE automatically goes back to the same department. But when somebody retires or leaves the campus, then the FTE goes

wherever the chancellor assigns it. And then the chancellor can assign it to a new department. Most of the time, he assigns it to the department that lost somebody, but he doesn't have to.

Hughes: Who advises the chancellor?

Koshland: Oh, all sorts of people--chairmen, deans, and everybody. The department can write a letter saying, "We want the FTE back." But if somebody retires in English, the chancellor can give a new FTE to physics.

Hughes: Does the CACB enter into discussions of that nature?

Koshland: Of course. For example, one of the first things that really pleased me--one of the key events right after the reorganization was approved--molecular biology put in a request--that was one of the better departments; it had a lot of prestige--to replace Robley Williams, who was an electron microscopist, with another electron microscopist because they had all this equipment left over, and they really wanted it to be used.

At that time, we felt that developmental biology had no representative on the campus and was an area we all felt was going to be big. So we said, "No, we won't give you the FTE to hire somebody just because there's a lot of equipment lying around. It will be difficult for other departments that don't have the prestige of molecular biology to get somebody in this new area, whereas molecular biology would be perfect. If you want to recruit for a developmental biologist, we will say 'go.' If not, we'll give the FTE someplace else." They said, "Sure, we'd like to do it." In fact, there was a fair amount of pressure from the advisory council, but it was all very polite. It helped our credibility, since I was identified with molecular biology and biochemistry, that we were willing to tell molecular biology to change and that we did not just give in to them. So that was very helpful.

Gerald M. Rubin

Koshland: We were lucky that one of the first recruitments after the reorganization was Gerry Rubin. We had figured out that he was a very logical candidate. He was young but already famous in molecular biology and in *Drosophila*, fruit flies. Thus, he had interaction with entomology, biochemistry, and genetics. Many departments were very, very excited about having him. They knew about his work. We said when we recruited him that he could go to any department he wanted on the campus. All the various departments made overtures to him. It helped a lot as people realized that we were recruiting for the good of the university. No matter what department Rubin ended up in, the entomology department would be pleased he was here, the genetics department would be pleased he was here, the biochemists were interested. He was a very important appointment because he was the first after reorganization and a very prominent young man at the time.

The Howard Hughes Professorships

Hughes: Now, he's a Howard Hughes professor.

Koshland: Correct. He wasn't a Howard Hughes when we recruited him.

Hughes: That wasn't a plum?

Koshland: No. In fact, the Chancellor, Heyman, had given me a MacArthur chair to bestow. He called me up and said, "You can use this for recruiting." It was very important to me that the first major appointment after the reorganization would be a plum appointment. I wanted it not only to be excellent so various people would say it's very good, but I wanted it to be seen across all of biology as a very important appointment. He was absolutely ideal.

Hughes: Did the Howard Hughes professorships come as a result of reorganization?

Koshland: I would say yes and no. The yes was, the caliber of the people we were recruiting made it more probable to get a Hughes professor. Tj [Robert Tjian] was here during the reorganization. We had hired him before. Gerry Rubin was one of the people we hired after the reorganization. And they were the two first Hughes professors at Berkeley. That was very, very important because they had never given Hughes professors to any nonmedical school before. That required a great deal of maneuvering, which I did. And I was helped by the fact that I was editor of *Science* at the time.

The Project Planning Guide for Construction

Koshland: The architects were supposed to draw up a plan to present to the legislature for the new building and answer to the state legislature's code and other things. They basically didn't do it.

Hughes: They didn't do it at all?

Koshland: They didn't do it at all; they didn't even start on it. Somebody in the chancellor's office paid them their final bill before they had done this. As a result, Alex Glazer and Louise Taylor--I've forgotten who else--spent a weekend preparing the whole document. It's called a PPG.

Hughes: Project Planning Guide.

Koshland: Okay, the Project Planning Guide. That had to be submitted, and that had a lot of detail in it: the size of the building, how it was going to be taken care of. For example, the legislature would only allow us to increase the amount of space per professor by a certain

amount. We were already going over the code they had established statewide. We said this was the last building, and if we were going to compete with Harvard, we had to give our professors more space.

So then this overall plan had to show we didn't exceed those standards by more than agreed upon for the average space given to professors. Space for teaching classes or for the library or things like that didn't count against research space. But it meant you had to look over the whole space and what each professor was getting. This group from the legislature was coming down Tuesday, and on Thursday we found out this other group, the architects, hadn't done it. Alex and Louise and somebody else spent the whole weekend writing a PPG, which is unbelievable.

Hughes: Yes, you had some good people.

Koshland: Well, we had good people, but I said we're never going to do that again!

Appointing a Judicial Council

Hughes: I read in Trow's article of a judicial council that was appointed to deal with complaints.

Koshland: Correct.

Hughes: What was that about?

Koshland: I've forgotten--some of it is pretty vague--but Rod and I agreed right at the beginning. We said sooner or later we're going to be assigning people to buildings, and the biology faculty as a whole is going to vote on whether or not we do the plan. But then if you do the plan, you're going to eventually say Professor X has to go to Building Y and he won't like it.

We wanted to have some device to adjudicate, since Rod was very closely identified with reorganization and so was I--we wanted a supreme court of people who were not involved in this, to make a decision on whether or not this guy who, let's say, was an outspoken critic of the plan, got a fair judgment.

If you had to have the judicial committee made up of somebody, say, in the law department because everybody else had a big axe to grind, they might not understand the problems. But fortunately, at the end of the thing, there were some people who were not directly involved. So Rod appointed a committee to take up complaints. My memory is, the complaints were almost zero. We had maybe two or three cases at most.

Hughes: Over time, you had a lot of complaints.

Koshland: Oh, we had a lot of complaints about the plan, particularly at the beginning, but, in the final plan that went through, I think we had only one or two professors that really objected to where they were being asked to go.

Failure to Create a College of Biology

Hughes: Why was the idea of forming a College of Biology rejected?

Koshland: The College of Biology was really a good idea, and the main reason was because you could form some cohesiveness to all the biological programs. We were getting cohesiveness by informal cooperation anyway, so it wasn't quite that crucial for the programs. What it was really good for was raising money. The College of Engineering and the College of Chemistry were the two best groups on campus for raising money. I don't know about the law school. They have their own colleges, and they can hire their own development officers. It's something we still feel.

The opposition came from two groups: The College of Natural Resources--a lot of the chairmen were against it because they were still representative of a more old-fashioned group, and they didn't want to be overwhelmed by the College of Letters & Science, which they felt had more prestige and would be likely to outvote them. The second opposition came from the College of Letters & Science. The provost, Ken Kui, an astronomy professor, and many other people were opposed. The biologists represented a big, major intellectual and financially powerful group. They didn't want them to leave the college. They felt it would create a big vacuum. So we had opposition from the College of Natural Resources people, who didn't want to be engulfed in the college, and from Letters & Science which didn't want the biologists to leave. I think it could have been done. It's just we couldn't do everything all at once, and so nobody got in charge of it.

Hughes: Is forming a College of Biology still a discussion point?

Koshland: I think it's still discussed. They haven't found any good leader to do it.

Reorganization and the Teaching Enterprise

Hughes: We haven't talked specifically about how reorganization affected teaching.

Koshland: No. The outside group complained to me that we had organized this [reorganization plan] pretty much around research and that we hadn't organized it around teaching. In fact, they called me in to a special session. I basically said to them, "You're absolutely right. We have not focused on teaching for a couple of reasons."

First of all, the feedback in general was Berkeley's teaching was very good. When the kids from here went to graduate schools or medical schools, in general we got no complaints. They were really well trained compared to others. In fact, many of the students complained that they were hearing the same things over again in medical school as they had heard as undergraduates.

So A) we didn't have a major problem. And B) I had exhausted all the people who were willing to work on committees. I had so many faculty on so many committees that I felt there was no way I could organize a whole separate group of committees. We'd just run out of people to do it. I felt that by getting good professors, they'd teach well. We did have to consider teaching and do something about it, but it wasn't urgent. It was not as urgent as getting people in the right areas, because you can't teach if you don't have the right people. And so I just threw myself on the mercy of the court and said, "We just can't do it all at once." So that's the way it ended up.

Hughes: When you actually began to move people--first out of LSB and then back in--was teaching disrupted?

Koshland: Well, sure. One of the great things was that Beth Weil, as biology librarian, kept the whole library going. It was in quonset huts and special facilities. She did a great job. You always have to cope when you're building a new building and people are moved. But in general we moved pretty fast. There was a certain amount of money set aside to make the faculty have an easier time moving than they might have otherwise.

Some of that money got used up in overruns on the building. But even so, we had a fair amount. I didn't want people, in addition to being forced to move, that they were also forced to pay for their own move. Most of the money was provided as part of the reorganization.

Animal Rights Activism

Opposition on Campus

Hughes: How did the animal rights issue work out?

Koshland: The animal rights issue ended up as a big plus in a very peculiar way. Because it was biology, animal rights were always important. There were a couple of scandalous things that had occurred--scandalous from the newspapers' point of view but not really important. We had broken a rule because the walls in the rat quarter weren't washed down once a week.

Most of the professors were very conscientious about their care of their animals. But there was a case where somebody had some cats and rats, and they were taken care of by a student who was doing the research, and the student left on a vacation and delegated it

to one of his friends, who forgot about it and then didn't feed them. The rats were starving and ran out of water and things like that. This got in the newspapers and added fuel to the fire. As a response to these problems, we decided to build the Northwest Animal Facility. We needed more animal facilities anyway. So that became part of the reorganization. It wasn't in the original plan.

Episode in the California Legislature

Koshland: Elliot Katz, a lawyer and an outspoken opponent of the university, was always denouncing the university on every occasion. The reorganization plan had gone through legislative committees and been approved by the governor. Then the whole appropriation had to go through a final joint Senate-Assembly committee for approval, which was chaired by Senator [David] Roberti, who was a big animal rights advocate. He was supported by a whole bunch of movie stars, who complained about fur coats and all sorts of animal agenda items. The fundamental plan was 95 percent with a 5 percent animal facility tied into it. We were very apprehensive about it.

The animal rights people were mainly interested, of course, in the facility for the animals. But it was a facility which would be a Waldorf-Astoria for animals. It [chuckling] would have animals living under more palatial conditions than any animal had ever lived before.

Hughes: Including the faculty [chuckling].

Koshland: Correct. But even so, we knew the animal rights people were going to oppose it one way or the other. Heyman was to give the final speech for the plan, because he was the chancellor. It was decided I wasn't needed, and I'm not sure whether Rod went with him or not.

It made us very apprehensive because Heyman was a lawyer and couldn't know many of the details. We filled him in as much as we could. We had been a year or so into the actual construction, and so he had a fair amount of knowledge. But he, himself, was a little nervous if these guys started to sharpshoot at him.

When Heyman came back, I asked him what happened. He said, "Dan, you're not going to believe it. This committee called me up, but before I got up, Roberti called on this guy Katz to make a statement. He gave a statement which I thought was going to be the end of the world, saying how terrible the university was and that this whole animal facility was for torturing animals, and it was a sadistic chancellor and sadistic professors and sadistic students, and this wasn't a reform at all. And he went on and on like this. It was so beyond logic that you could see after a little while, everybody was saying this was stupid, even Roberti. By the time I got on, Roberti was apologizing to me about Katz. He was saying, 'Chancellor, I want you to understand that we all think the university is a great place' and things like that."

Heyman has great political instincts, so he said, “I realized I almost didn’t have to say anything. The less I said, the better. So I said something like, ‘You have been through this plan. It’s something that’s very important to the university. We want to be in the forefront of teaching our students the best subjects, and we really have to do this reorganization to teach them good biology, and there’s no point in my going into details.’” He made it extremely short. Almost all the preparation that we had given him, he didn’t need. His instincts were absolutely right because it just went through.

Hughes: An amazing story.

High-Tech and Low-Tech Science Buildings

Hughes: I read of some tension over the fact that the two new buildings--Koshland and Life Sciences Annex--had been described as “high tech.”²⁴ I thought it might bring out an undercurrent of tension between the molecularly oriented scientists and the organismal scientists.

Koshland: No. We used “high tech” mainly for the legislature. It was not a matter of condescension; it was to characterize for the legislature in a word, which is what you have to do with legislatures, that LSB was a low-tech building and we couldn’t do certain things in it. Old LSB was really a hazard to everybody. Therefore, we would put research that was high tech, like viruses, in these new buildings. We had rooms where you have everything sealed--no cracks in the walls. That means you can get in there with a hose and wash the whole room down. If scientists in LSB had to do high-tech experiments, they could do them in the other building.

Incomplete Reorganization of the College of Natural Resources

Hughes: In the end, only one new department was formed out of the College of Natural Resources. Was not the original plan to fully incorporate the entire College of Natural Resources in the reorganization plan?

Koshland: Correct. What happened is the following: at the beginning, when we wrote these letters to the Academic Senate, I also wrote a couple of letters to Doris Calloway, who was the provost for the colleges, including the College of Natural Resources, to ask some routine questions about cooperation. I was leaning over backwards to be cordial and helpful. Apparently, she had heard enough about me or my intentions that she never answered. I’d see her occasionally on the campus. She was pretty curt, so I could see she was not very happy about any reorganization.

²⁴Trow, p. 161.

The dean of the College of Natural Resources was named [David] Schlegel and he was followed by a guy named [Albert R.] Weinhold, both of whom were very helpful. They were a little apprehensive in the beginning because they had had some bad relations with Letters & Science. I had not been involved with them at all. They really liked the idea of merging the botany groups--the plant group in Letters & Science and the plant group in the College of Natural Resources. They cooperated with us, and so that was the biggest single department that was involved. There were other departments, like nutrition, that should have been involved. Later on, public health did become involved. But Calloway was so negative and we had so many big problems, we eventually decided we were just going to forget about anything other than the plant scientists. We'd go back to the others later if we got a chance. But as a result, we didn't really do a good job on reorganizing the College of Natural Resources.

There's an ironic final touch because later on, when the Chancellor's Council on Biology was working, Wilford Gardner became the dean of the College of Natural Resources and asked, did he really need to complete the reorganization of the college. We said, "Yes. Please do it." He was an interesting guy. He said, "Dan, I think you did it all wrong. What you should have done is consult the Academic Senate and do the reorganization of the college with their advice." He was a relatively new dean at the time. I told him why we hadn't done it. I said, "You can reorganize the college with the Academic Senate if you want, but my prediction is it will be a disaster." Well, he tried it, and it was a disaster. He didn't get anywhere. So at least we believe Rod's end-running the Academic Senate was a good move.

Hughes: Dr. Park said that one of the stumbling blocks was that many of the people in the College of Natural Resources were paid on an eleven-month schedule.

Koshland: Right.

Hughes: And they were also apprehensive about being included with the Letters & Science scientists because their track record in terms of grant-getting wasn't as good. They were afraid of being left in the dust.

Koshland: Yes. There was tremendous hostility in the two groups. The summary that I can give you is that the College of Natural Resources people were very practical. They could advise a farmer, and they looked down on the botany people as being ivory-tower people who weren't doing anything of any use to anybody. The botany people, on their part, were members of the National Academy and had all kinds of academic honors, and they looked down on the College of Natural Resources people as sort of dirt farmers who weren't intellectually worth anything. So that was a really big thing when we merged the two departments. I think they're really pretty happy now to be together.

Hughes: Are they?

Koshland: Well, you'll have to find that out. But they're in a building together, there are joint appointments--so it worked.

Louise Taylor's Role in Reorganization

Hughes: You said last time that you wanted to say a word about Louise Taylor.

Koshland: Oh, yes. It is true that I was the sparking force behind the academic plan, but we also really needed the buildings. The buildings was a carrot to offer the faculty, to be willing to go through this reorganization. On the other hand, we needed the reorganization because we needed to go to the legislature and say we had a new program. If we just went and said Berkeley deserves the buildings because they're a bunch of good guys, that would not be a very salable proposition. By saying we were revolutionizing biology--and we were--it was a much better selling program. So I was focused on the academic plan.

Rod was smart enough to say, "Dan, this is going to be a big, big program, and you're going to need somebody in the administration who helps keep an eye on it." So he said, "Would you identify somebody to do this?" I didn't know anyone in those echelons. I was a good professor, working at my lab, and so I didn't want to bother with anybody up there.

I had lunch with Horace Barker, a very distinguished professor in our department who had been chairman for a while, and was discussing this problem. He said he had been on a committee with Louise Taylor, and he thought she was very good and I ought to try to get her. So knowing nothing more, I suggested Louise Taylor. She had heard about me through a relative. She had gone to a swimming pool of a relative of mine. But that was the only connection I had with her.

Anyway, Louise was put in charge of the administrative part of the reorganization, and that was wonderful because she was just excellent. She wasn't a Ph.D., but she caught the atmosphere, she understood the academics, and she could deal with the administrative people, so she kept charge of the program. It became, as you know, a big, big program. It was big construction, changes of academic personnel, everything. So it was extremely lucky we had her.

More on the Role of Personality and Personal Characteristics

Hughes: Rod Park was a botanist, so he could presumably immediately appreciate the importance of what you were trying to do to reorganize biology.

Koshland: I think it saved both of us a lot of time. In the long run, I'm not absolutely sure it's an advantage to be working with a scientist. If I'm dealing with somebody who is out of my area and they think I'm sensible, they may defer more because they just don't know anything about science. But if the two scientists like each other and can complement each other's ideas, which is what we did, it works out very well.

That Rod was open to the idea of change, number one, and was willing to stand up there and take the heat, number two, were the two personal characteristics that were most important. I think both of us knowing about biology was a help but not as important.

Hughes: Less so. But there must have been an ease in your day-to-day communication.

Koshland: That's absolutely true.

Hughes: Do you think that without Rod Park you could have kept reorganization out of the Academic Senate?

Koshland: Each of us contributed. I think I contributed things that were important, and I think Rod did. Rod was really very good about a large number of things. He was the kind of person who could take criticism. If there are many people complaining, many administrators would have just said, "That's [reorganization] not worth doing. The campus is running pretty well."

One of the worst and most important apothegms of the English language is: "The rich get richer and the poor get poorer." It's true. By "rich" I mean the people who are advantaged. The thing is if you have certain advantages and you know things and it goes well, you learn to call people who also are successful, you are secure enough to take criticism so the program goes quickly. If you're deprived of those resources, it's much harder for you. That's partly what happens in something like this.

The more Rod and I trusted each other, the easier it was to do everything subsequently. I'd call up and say, "Rod, what do you think of this?" And he'd say to me, "Dan, I don't think that's going to be much of a problem. Go right ahead." You may think that's trivial, but a lot of things have to occur quickly.

Hughes: What you seem to be saying is that attitude and personality played an enormous role.

Koshland: Absolutely. I think so much depends on mutual trust. I think personality is an enormous factor. There are certainly procedural things that really involve good organization and good management, and I think you make life very hard for yourself if those are done badly, and impossible if they're done very badly. But I think you could do everything mechanically perfectly, and if the personalities don't mesh, you really don't have much chance in a big controversial plan like reorganization. If you're an efficient department chairman and you're very fair but you have no rapport with anybody, then I think you probably can carry department administration off well by just being mechanically correct. But in a big deal like reorganization, my reaction is there is no way you can do it if the leaders are not compatible personalities.

The personality factors involve all sorts of combinations. I will give you a couple. First of all, egotistically, to start out with myself. I think that it was important that I be pretty well liked by a number of the faculty. There were a number of the faculty that were going to hate my guts without knowing me personally at all because I was advocating something that would be threatening to them. So you have to have a certain number of people who will loyally support you.

I would get a certain number of people who would support me anyway, because they agreed with what I was advocating. But a friend is willing to do a little extra. He's willing to write a letter to the chancellor. Some people who may be just academically on your side but emotionally they may not be willing to do that. I think that interaction I had with a fairly large number of the faculty was important. The second personality factor that was incredibly important was my relation with Rod. And I really didn't know Rod almost at all before this started. But we just got along very well. I liked him, and I think he liked me. We just were very informal with each other.

Rod did something which I think was partly because he liked me but partly just smart machinery. In the beginning he was very loyal and was going to say, "I'm not going to listen to any complaints." I really felt that was wrong. He devised the following system. He said he would let anybody come to him, but he would always tell me what the complaints were, so that I would always be able to answer the complaints. He wouldn't say who made them, so the person would have confidentiality. But if they said, "Koshland has put his daughter on the payroll" or something like that, he [chuckling] wouldn't let that become a big gossip thing without my having a chance to put the record straight.

That was done right from the beginning. I consider that a very smart move. So we never really had any big problem because if somebody said, "Koshland is trying to impose molecular biology on us plant morphologists," and Rod would come to me and say, "Dan, is this true?" I'd say, "Yes, that's exactly what I'm trying to do, but it's for their benefit."

We each served as an early-warning system to the other. If we heard some rumor, we would warn the other that it was something he was going to have to face. It worked very well. And then, of course, we got together on the strategies in regard to the Academic Senate. I think that was very important. And Rod's relation with Heyman and my relation with Heyman were important. We both got along well with him. The great thing was that Rod and Mike really listened to all the criticism, but they didn't get deterred, so we went on.

Colleagues would ask, "How are you so sure that Rod's behind you on this?" I said, "I'd know the minute Rod is not on my side." They'd say, "How do you know that?" I said, "The minute Rod is not on my side, he'll just say, 'Dan, we'd better refer this to the Academic Senate.'" I said, "Then I know he wants to kill it." And that was my standard explanation. And since he never said that to me, I knew he was for going ahead with the reorganization.

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REORGANIZATION OF BIOLOGY
AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1980s-1990s

An Interview with
Roderic B. Park

Interviews Conducted by
Sally Smith Hughes
in 1999



Roderic B. Park

INTERVIEW WITH RODERIC PARK

THE REORGANIZATION OF BIOLOGY AT UC BERKELEY[Date of Interview: May 6, 1999] ##¹**Recognizing Deficiencies in Campus Biology, 1960s**

Hughes: Dr. Park, you kindly gave me a draft of the chapter that you're writing for your book.² You made the point that the seeds for reorganization started in the early sixties.

Park: That's correct.

Hughes: Could you tell me what those seeds were?

Park: Back in the early sixties there was a group of associate and assistant professors [in zoology, botany, and biochemistry]. The ones that immediately come to mind are Dave Hackett, who was initial chair of our group, Oscar Paris, Fred Wilt, myself, and a few others. We had indicated to our chairs that there was tremendous duplication among the departmental courses in beginning biology at Berkeley. The problem was that these introductory courses were aligned around organismal and taxonomic disciplines, and this occurred because the university was founded [1868] just a few years after [Charles] Darwin published *The Origin of Species* [1859].

At that time, the great excitement was evolution and how various groupings of animals and plants were related. Departments were organized around botany, zoology, microbiology, anatomy, and related disciplines. Then cross-cutting disciplines were introduced, such as genetics in the early 1900s with the rediscovery of [Gregor] Mendel's work, biochemistry and physiology in the 1920s. Suddenly it became more and more evident that it wasn't that organisms were different in appearance but they were similar at lower biological and cellular levels; there were enormous similarities.

¹## This symbol indicates that a tape or tape segment has begun or ended. A guide to the tapes follows the transcript.

² Chapter from manuscript in preparation: "Biology: A Lesson in Reorganization," *It's Only the Janitor--A Guide to the Customs and Inhabitants of the Academy.*"

A group of us who had been trained as graduate students in the fifties suddenly entered the Berkeley scene. We were very familiar with the cellular and chemical similarities among organisms, and we wondered why all the courses were being taught along disciplinary lines. So we started raising some questions. There was a Biology Council at that time, which had been set up by the dean of the College of Letters & Science.

The dean set up a group of chairmen who went actually across college lines because it included chairmen--at that time, the word *was* "chairmen"--from the College of Natural Resources, which was then the College of Agriculture. They took the view that the best way to let the gas out of the balloon was to invite the Young Turks to come up with a proposal.

We produced a consolidated introductory course, which would take students a full year and which would feed into all the departmental majors. That proposal became the Biology 1 course. We put that together in the early sixties, prior to the time the FSM [Free Speech Movement] really got rolling. There was more money in the university, and we were given money to remodel whole laboratories on the third floor of LSB [Life Sciences Building], which belonged to the zoology department.

We designed them elegantly for new and traditional kinds of work, including such things as libraries in each lab, which had many of the seminal books in biology that had been written over the last hundred years or so, important ones, that students could check out and read and that could be brought out for demonstrations. We had laboratories repeating some of the famous experiments, such as Kaelin's experiment for observations of cytochromes in the flight muscles of honeybees. As a matter of fact, I think some of those tapes still exist. You can find tapes made thirty or thirty-five years ago of myself teaching the students on television how to do this set of observations. As a matter of fact, they were using them up till about ten years ago. [laughing] I don't know if they still are used or not. But at any rate, I think the Biology 1 course really sowed the seeds of an idea of a biological reorganization.

I taught in that sequence until the late sixties, at which time I went on leave for a year to become a Miller Professor. Shortly after that, Biology 1 was carried on by other people. Mostly the instructors came from molecular biology or the younger faculty members, though we did have contributions from Morgan Harris, who came in and taught part of zoology, and Ralph Smith. But other than that, it was mainly the younger faculty members. I don't remember in great detail just who taught each year.

Hughes: Largely younger faculty carried the teaching load because they were closer to the cutting edge?

Park: No, I think it was, again, a continuation of what was expressed by the Biology Council: the young Turks want to do something; let them do it. But we're quite happy with what we've been doing for the past forty years.

Francis McDonald Cornford wrote a little book in 1908 on academic politics called *Microcosmographia Academica*. It's only twenty-five pages long and there are ten chapters. One chapter is called "Argument." It starts with a one-sentence paragraph. "There's only one argument for doing something; the rest are arguments for doing nothing." I think that is

true of all institutions. It's certainly true of some of the older faculty members. As long as they weren't made uncomfortable, and were allowed to continue what they had been doing, they were fine. So it tended to be more the younger people, though people like Gunther Stent always were very active in this program.

Of course, the people from molecular biology had a natural inclination towards understanding the universality of biology, but they also were primarily a graduate program which needed to generate student undergraduate credit hours. So that was another reason some of those departments joined.

Anyway, I think that the seeds of the biology reorganization were really sown with people who had been trained after the Second World War, who were part of the DNA revolution which started with the work of [Oswald] Avery and others and of course was continued by [James] Watson and [Francis] Crick in the elucidation of the structure of DNA. It was really that whole development of research that led people to try and teach biology in a somewhat more cohesive way.

Hughes: At that point you were focusing on teaching? You weren't thinking in terms of more coordination of research?

Dean, College of Letters & Science, 1972-1980

Park: No, not at all. That really didn't come about, as I pointed out in my unpublished chapter, until I was asked [1972] to become dean of Letters & Science, which was a pretty unexpected occurrence for me. I had never thought about suddenly becoming head of the unit which had some nine hundred faculty and most of the student credit hours at Berkeley.

Hughes: Did you welcome the opportunity?

Park: Well, I didn't really seek it out. I never really sought out any of the administrative jobs that I had. I never regarded myself as a professional administrator. I've never taken a course in administration. I've never been trained in administration. Particularly evident in those days was that all the administrators tended to be scientists. But that was largely because to get tenure as a young assistant professor of science, you had to be an administrator. You had to apply for grants; you had to run a budget; you had to do personnel process with your people--you had to do all of those things that an administrator does, that the humanists never have to do. They could go to a small library somewhere and write up a thesis and publish a book, but they didn't have on-the-job training in administration.

I had, of course, done that. I had been head of the committee, after Dave Hackett, that pulled the Biology 1 program together, and I seemed to have an ability to listen to people and to accommodate interests and to get the herd headed in the same direction, rather than fifteen

different directions, and build some enthusiasm and get people involved. I often said that building a team--whether it's in Biology 1 or at the University of Colorado or whatever it happens to be--is very much like handling a crew on a racing yacht going to Hawaii. You get people on board that have certain skills and certain abilities. Some of them are prima donnas. And you very rapidly have to find out what people can do best and put them in those jobs and make them feel good about it and get them to work together as a team. In the same way that you can't throw anybody overboard in the middle of the Pacific, you can't fire a tenured faculty member very easily. So it's a matter of getting people to work together. Again, it's like a racing yacht where people either pull together and get stronger, or they go below and pull a pillow over their head and disappear.

I also knew at the time I was asked to be dean that there were probably, out of a faculty of nine hundred, only four or five who were at a stage in their career, who had the right inclinations or abilities to be asked, and I'd probably never be asked again. So here was a chance where I was one out of nine hundred where I had a chance to do this and see whether I could do it or not. Every job I've taken on like that has been an experiment in the sense that if I was absolutely sure I could do it, I wouldn't do it because it would be boring. This was a chance to try one's wings on something else.

Hughes: What happened next?

Park: Well, after I became dean of Letters & Science, I suddenly had responsibility for a much larger range of programs, their directions, the kind of support that they had. There was a statement made to me by Lincoln Constance, who's still alive, a former dean of the college. When I was asked to be dean, I said, "Lincoln, what's important about being a dean?" He said to me, "Well, if you do the people part right, it doesn't matter too much what else you do that's wrong. But if you do the people part wrong, it doesn't matter what else you do right."

Deteriorated State of the Life Sciences Building

Park: We certainly worked hard on the people part, but on the other hand, we had some really aging facilities. The pipes were always bursting in the Life Sciences Building. It was designed in the twenties, and the electrical bus bars, the transformers, and ventilation control, and everything were totally inadequate for modern biology, and the building kept getting patched over and over.

It had a large central courtyard, and people needed hoods, and they would run these big sheet metal hoods all the way up to the top of the building from the first floor. The courtyard, which was supposed to be a beautiful spot, began to look like one of those alleys in Harlem you can see from the train. It was getting to be a real shambles. And there were constant breakdowns, plumbing problems, hollow tile walls with roaches, and electrical

outlets that you would have to put on an oscilloscope to find out what they were, and they'd be delivering something like rectified 240 volt current that would burn out anything that you plugged in. No one knew anything about the utilities; there had been no wiring map kept, and it was just a very bad place to do modern research.

Hughes: Your lab was in LSB?

Park: Yes, I was down in the basement. I had moved down there full time in 1964.

So there were lots of problems both with academic organization and with facilities. One of the ways we discussed these problems, besides during our routine work, was at the executive committee of the College of Letters & Science, which was a committee of faculty who were nominated and appointed as an Academic Senate committee by the Committee on Committees of the College. Marian Koshland was on that committee--I don't know just what years, but it was about 1973-'75; maybe '72-'74.

The Meeting which Sparked the Reorganization of Biology

Park: I described, I think, in the chapter that after one of our executive committee meetings, which occurred once a month, we went over late in the afternoon to have a drink at the [Men's] Faculty Club, and Dan Koshland joined us. That's when we had our very first discussion about the direction of biology. Were we as a campus equipped to take advantage of all of these new developments in molecular biology and biology?

Of course, the answer was we were not. We were going to get left in the dust by institutions such as MIT and Stanford and others which were taking a more aggressive approach. What does that mean, to get left in the dust? To get left in the dust means that there would be money appropriated through NSF [National Science Foundation] and NIH [National Institutes of Health], where we would have less of a chance of getting it because of the way we were organized and because of our facilities. We would no longer get the best graduate students in these new fields. We might still get the best graduate students and faculty in our traditional fields, but that's not where biology was going to go in terms of getting the best intellect into the university which was necessary to retain the top rankings of our departments.

So [the Koshlands and I] had that discussion and talked about how we needed to replace LSB and we needed new facilities of various kinds. The amount of money was just appalling to me. We worked very hard to get a few million dollars to fix this or that, and this was going to be hundreds of millions. There was a total disconnect between what was being said and what I felt was possible.

There was not only a financial disconnect, there was also a faculty disconnect. As I suggested earlier, of the several hundred biologists, except for the ten or so who wanted to do something different and taught Biology 1, there were a lot of people who were very happy with their present circumstances. And how were you going to change that?

Inventory of Campus Biologists

Park: I sat down and talked to Fred Carpenter, who was a professor of biochemistry and divisional dean of biology, about what we might do. We jointly developed a strategy, which was to do an inventory of biology faculty at Berkeley that we would organize by research field rather than by present departmental alignment. And we would then ask faculty whether we had it right. If they were working in neurobiology, we'd have all those people together. So we would pull the physiologists together and the biochemists together and the taxonomists together and various others, to find out how many people we had working in various areas of biology. I don't think that was really known. We knew at the time that every department wanted to have its own geneticist, its own biochemist. Every department wanted to create sort of a mini-university within a university, and that was obviously a bad way to be going. So we pulled all of these people together, and we went out several times with this inventory to get the faculty to respond to whether they were put in the right category or not.

Ordinarily, faculty don't like to respond to white papers that appear in their mailbox. They just come and say, "It snowed again." But when it touches as closely to home as--what kind of physiologist are you? What group of people do you fit in with? How do you look in this spectrum?--they are very inclined to respond. We got this material back, and that really formed the basis for deciding what we were good at and not so good at in terms of the subsequent reviews, the external reviews and others.

Hughes: When was this?

Park: We started this in '74, and I don't think we finished it for a couple of years. And then we got the external review committee, which was 1980 or something?

Hughes: The report came out in 1981.³

Park: Yes, 1981. I think it was about '78 that we finished the inventory and put it together, and of course we got the recommendation for an external committee to advise us on scenarios for academic development.

³"The Biological Sciences," University of California, Berkeley, Report of the External Review Committee, April 1981. (Office of Planning & Analysis, Vice Chancellor--Resource Planning & Budget, University Hall, University of California, Berkeley. Hereafter, University Hall documents.)

What our internal committee recommended for facility development seemed financially impossible. I knew there was no sense in talking about dollars until we had a program lined up, because money in search of a program is always a disaster. We needed a program in search of money. So this was really a way of starting, starting with the committees, which then looked at the number of facilities we'd have and what we'd have to do, and then we got reinforced with the external committee.

Asking the California Legislature for Funds

Park: That led to the proposal for a new and simplified organization that came from Dan's committee. They proposed three major units based on level of biological organization from molecular biology to ecology. It's a chicken and egg argument. The reorganization was not going to fly unless we had the money to construct the new facilities. In order to implement their report, we had to be able to go to Sacramento and get the money. As I pointed out, that was a long and drawn out argument--first with the people who represented capital planning out of Sacramento, such as Jerry Beavers from the legislative analyst's office. Jerry was a knowledgeable guy but felt that his job was to protect the legislature from these hedonistic, irresponsible, avaricious, non-understanding people [at Berkeley] who didn't understand the fiscal realities of Sacramento.

I remember one day in the late seventies or so when we were touring the Life Sciences Building, showing Jerry the inadequacies. We took him into the Museum of Vertebrate Zoology. They have big tables for spreading out skins, and there on the table was a sole beaver skin. Bill Baker looked at him and said, "See, Jerry, that's what we do to beavers around here." [laughter]

Hughes: Did his ears perk up? [laughter]

Park: So we had a problem: we could never convince Jerry Beavers that we should have these facilities. It was going to be a political decision that went over his head.

The Animal Rights Issue

Park: As I said in the book, it came down finally to a decision of the legislature based, not on the merits of our proposal, but on whether we had satisfactorily met the latest criticisms leveled at us at the university, and some of them properly so, by the animal rights people. Animal rights had become a lively new furry issue in Sacramento that they were all excited about. It was also bringing a certain amount of money into politics because animal rights supporters are a well-heeled group.

The problem with respect to the animals started because we had received criticism. It continued when George Maslach,⁴ who was the provost of the professional schools and colleges at the time, had--I think foolishly--asked the American Association for Laboratory Animal Care [AALAC] to come and see whether they could accredit us. Well, there's a rule that if you don't want the answer, don't ask the question. AALAC came, and they were kind of appalled by what they saw and put us on probation.

Hughes: This is mid-eighties?

Park: Probably '82, '83. It became evident we needed to construct all new [animal] facilities; we needed to train the faculty. We were dealing with a faculty that had been doing things the same way for fifty years. They kept all sorts of mixed species and animals in their labs. They did all kinds of things which were absolutely improper and kept improper records. They didn't have proper veterinary care or proper sanitation. The animal techs were not uniformly trained; the graduate students weren't properly trained. The faculty members said, "This isn't regents' space; this is my space. You can't come in here and tell me what to do." Again, there is only one argument for doing something; the rest are arguments for doing nothing.

But in order to get the money from the legislature, we had to change all that. We got an outside consultant named Al Edward who came in and started working with us. He developed plans for new animal facilities and redid our training, and got this long process in motion. But reorganizing animal care has many, many arms and legs in terms of what you have to do.

Hughes: Was that the genesis of a central animal care facility?

Park: Yes, that was the genesis of central animal care facilities. One of the reasons we told the legislature we needed these buildings was we had to construct a centralized animal care facility on the top of the annex building on LSB. There's another one for invertebrates in the basement. We also needed to construct a new facility over in the northwest corner, the Northwest Animal Facility. We could not get accreditation until they were all finished, but at least we might get provisional accreditation by showing that we were making progress.

⁴See the ROHO oral history with George J. Maslach: *Aeronautical Engineer, Professor, Dean of the College of Engineering, Provost for Professional Schools and Colleges, Vice Chancellor of Research and Academic Affairs, University of California, Berkeley, 1949-1983, 2000.*

The Chancellor's Role

Park: Mike [Ira Michael] Heyman had to convince [John] Vasconcellos and the other legislators that we were making progress, and he did. He went up to Sacramento and he gave the legislative budget committees a good talk on all the things we'd changed and the things we'd done and what our commitment was, and they finally believed us. As I said, in the spring of '84 the legislature finally approved the funding for the first building, the [Life Sciences] Annex. And that was the turning point.

Earlier that spring [1984] I had started a discussion, speaking with the biology faculty in the Faculty Club. They were very diffident about whether our planning for reorganization should be done or not. Why are we doing all of this planning; it was dependent on new buildings which might never happen. Construction funds were finally approved and the money was to be appropriated July 1. There was an arrangement whereby the contractors could start a little bit before that. In May the construction fences went up behind old LSB, the parking spaces disappeared, the trees started coming down, the utility work started going on, and suddenly people realized the reorganization was going to happen. Suddenly diffidence disappeared.

Then the fact that we were going to occupy these new buildings not by traditional departments but through intellectual affinity groups, which we had been working on since '74, led to a faculty scramble for position that was almost shameless. [laughter] Faculty members in the abstract like to regard themselves as being totally rational human beings who are above coarse, base, political, and financial incentives. All one has to do is dangle out that kind of facility opportunity to observe how rapidly people will follow when they see it's greatly to their advantage to leave the past. But without those buildings, that reorganization never would have occurred.

The External Biology Review Committee

Hughes: Well, let's go back: whose idea was it to appoint an outside review committee, and who selected the people and for what reasons?

Park: We had the Chancellor's Advisory Committee on Biology. I'm really not too clear on this; it has been so long ago. The Chancellor's Advisory Committee came up with the names for an outside committee to review our plans. Dan Koshland and I had discussed it.

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Park: People aren't really going to take a plan seriously until there is an outside review. That's absolutely imperative. Outside review has to do with your reputation in a much broader

arena than just that on the Berkeley campus. Faculty egos are like any professional egos. Faculty want to be seen as doing meritorious things by the outside [world], and they want to be supported by the outside. So it's essential to get an outside committee; that's part of the pageant.

The names were nominated by the advisory committee. They were brought to me for a laying on of hands. I wasn't going to make any changes unless there was something obviously wrong. The names that came forward looked just fine to me. This was the group that was invited, and Louise [Taylor] was very much involved with staffing that external committee.

Hughes: Were the members to provide a range of scientific perspectives?

Park: Yes, the idea was to include the major biological disciplines. So the review covered everything from microbiology, genetics, physiology, and molecular biology on through taxonomy, organismic research, and ecology. It covered the whole spectrum. So it was sort of a biosphere review.

An unexpected benefit of those interactions with the external committee, coming once [1981] and then coming back again [1986], was the fact that a number of them actually became interested in coming to Berkeley. I think a couple of them ended up on the faculty. But they also were very good ambassadors for us in terms of recruiting new faculty because they went back to their institutions and talked about all the wonderful things that were happening at Berkeley and how we were really addressing issues. It led to a lot of discomfort [for] administrators on other campuses. ‘

Dan has been asked to go many times [to other universities] to talk about what happened at Berkeley. I've gone several times to talk about what happened at Berkeley, what the essential parameters are for bringing about change. Administrators look for simple answers. I told you about the tripod talk. They want something that can be digested in thirty minutes. First you do A and then you do B and then you do C. And it isn't like that. A reorganization has to be specifically adapted to every institution, its history and its motivations, and the right people have to emerge and get excited about it. They may have animal issues; they may not, which may interplay. They may have strong private fundraising or not, which may interplay. They may have legislative oversight or not, which interplays. There's no set rule for it. It's a matter of developing a strategy, given the circumstances of a given institution.

Hughes: Reorganization of biology at Berkeley, within those very wide parameters, has been used as a model for other universities. But what about you? Where did all these ideas come from? Were there models out there that you could take pieces of?

Park: No, we didn't. I think we pretty much developed a strategy that we knew would fit for the Berkeley campus. I don't take management courses, and I didn't read any management books. I just did what was reasonable, understanding some of the deepest motivations and

fears of faculty, of the legislature, of the students, of the staff. The staff was an important part. You had to say in this reorganization that you might not end up with the same organization. But you had to assure every staff member there would be a job and nobody would be downgraded, though certain people might be red-flagged for a while if they were in another position without as much responsibility until the salary caught up with the job.

There were all sorts of things that had to be done. We didn't need a union fight on top of the other tasks. But all of these factors--understanding the motivations and fears, as I said, of the students, the staff, the faculty, the legislature, the regents--all of this enters into it. And if you don't understand their lives and understand what concerns them, you're going to step into a big trap somewhere along the line. There are different traps in different institutions.

More on the Chancellor's Role

Hughes: Top administrators at Berkeley have a certain understanding of Sacramento, but I'm suspecting that this particular problem required you to acquire even more understanding. Or did you hand that aspect over to the chancellor?

Park: The chancellor did a lot of it, and he and I taught each other a great deal. He and I had a relationship similar to the one that I had with Dan. He was the one who was at the regents' meetings, though I used to go occasionally and represent him. He was the one who testified in Sacramento, though he and I would have long discussions ahead of time about the strategy and how to put all this together.

Hughes: Did Heyman have rapport with Sacramento?

Park: Yes, very good. He's a Democrat. It was a Democratic legislature. He's a very powerful speaker in front of a political group. As a matter of fact, when he was testifying, the members of the legislature in Sacramento, who each had a little squawk box in their office so they can hear what's going on on the floor, used to come down to listen to [Heyman], just because they liked to watch him work and answer questions. If you like to watch people do things well, as I do--I don't care whether they're cracking a safe or whatever they're doing (moral considerations aside)--if they're really expert at it, it's fascinating to watch them work--whether it's a [Slobodan] Milosevic or whatever it is--to see how they go for public opinion and support. And Heyman just was superb at that.

I'll tell you a little story about Mike. After I was on the Muscatine committee which wrote "Education at Berkeley," there was a new senate committee set up in the mid-sixties called the Senate Policy Committee. The Senate Policy Committee was to take up issues of policy which weren't covered by standing senate committees. Because everything seemed to be changing so rapidly, some things fell in between the cracks. We suggested appointing a

commission to look at a whole variety of problems facing the university, and Mike came in to argue for this new commission in old Wheeler Auditorium in front of the Academic Senate.

We had an English postdoctoral fellow in [Melvin] Calvin's lab at the time named Vivian Moses. He and I got to the meeting about ten minutes late, and Mike had already started. We were standing in the back. Mike went on for about ten minutes or so, addressing the faculty, and Vivian had no idea what was going on or who he was. He looked over at me and said, "Who is that man? He could be dangerous!" So Mike was very, very important in selling the biology project. Mike also really got us into big-time fundraising.

UC President David Gardner and a Fundraising Campaign

Hughes: Tell the story of going to see [David] Gardner.

Park: Okay. When we realized the reorganization was coming along in the early eighties, we were going to have to raise money, but we had to get some from the state, too. It was obviously going to have to be a joint effort. We hoped we could get a lot from the state. Things were finally easing up twenty years after the FSM, and the economy was doing a little better than it had in the late seventies. We went down to see David Gardner at University Hall. We met--

Hughes: "We" is you and--

Park: Mike and myself and David, and I think Bill Baker may have been there. We described what was going on, what the reorganization was, how this was essential to the academic future of Berkeley because biology at that time was really in the place that physics had been back in the twenties; there were some very exciting emerging disciplines, with huge implications. And if we didn't get the right people and the right programs, we were going to be left in the starting gate. And to get the right people and the right programs, we had to have the right support and the right buildings and facilities, and we had to have approved animal care. We had to have a whole series of things. We went through this whole argument. It probably took us fifteen minutes or so.

Gardner said, "We don't have that kind of money; there's not that kind of money in the state. Even if we took the entire capital budget of the university, it wouldn't do what you want to do." We said, "Well, could we do part of it? Maybe we could fund raise for part of it." And Gardner said, "Oh, no, we can't do that. If you were to show that you could raise significant private support for this building, that would generate a whole range of problems. First of all, the legislature would then expect Riverside and Santa Cruz [campuses] to raise money for their buildings. They would be unable to do it, and we couldn't do that. This is an obligation of the state. We can't let the state off the hook."

Gardner said, "Also, Stanford would be very upset if we went in and started raising that kind of private money." I think Mike used the argument that if you looked at the total budget of a federally assisted university [such as Stanford], they weren't a private university any more than Berkeley was. They just got their money from a different source. I think we said that as long as we raised not more than one-third as much money as Stanford, we probably wouldn't get into trouble. [laughing] Gardner finally agreed that he would try to get all the money for the first building, and then we'd have to raise half the money for each building after that. We were going to have to raise over a hundred million--a hundred and twenty million dollars.

I think I explained that we already had been told by people from private institutions with very mature fundraising programs that a university just starting out fundraising could not say they wanted money for something and expect to get it. They'd have to have a variety of things on the shelf, and people might give it to them. But only the maturest programs would say, "These are the priorities of the university," and people would donate to the university priorities.

When Heyman and I [chuckling] walked out, got in the elevator and walked back up the campus, we looked at each other and said, 'Well, we've done it now.' [laughing] "There's no turning back at this point."

Hughes: This was the beginning of the campus looking towards private funding sources in a much heavier way.

Park: That's true. It used to be that money in small amounts would come over the transom to the regents, scholarships and various other things. When Albert Bowker became chancellor, he started putting a little money into fundraising and getting a program going. When Bowker came in, there was one half of one FTE, a man named Joe Mixer, involved in fundraising. Bowker had Dick Ericson for a while as head of the development office. And then it was time for a person with a greater capacity--well, I guess that's when Heyman and I came in in the early eighties. We recruited Bob Curley, who had been the former vice chancellor for administration, to take development to a higher level. After Bob left, Heyman got Curt Simic to head the office, and it expanded again to undertake the "Keeping the Promise" campaign. He was our first truly professional fundraiser. Then Simic left us right in the middle of the biology campaign. I was really annoyed. He was a graduate of Indiana, and Indiana offered him the chief fundraising job back there. Going back home to Indiana--isn't that the way the song goes?

Hughes: Yes.

Park: He went back home to Indiana, and I remember Heyman and I sitting down. (We had been at a regents meeting.) We were walking back from the convention center in Los Angeles to the hotel, probably ten blocks or so. We went into a bar to have a beer, and said, "What the hell are we going to do? Simic is leaving."

I said, "Well, I know one person that you can put in to finish this campaign. It's going to cause you a lot of trouble, but at least we'd get through it, because he loves the social flow; he loves dealing in the halls of power; and he has the genealogical memory of a Mormon. That's Mac Laetsch." And so Laetsch was very much involved in the successful conclusion of the campaign. Then he was replaced by [Chancellor Chang-Lin] Tien. [tape interruption]

The total fundraising campaign, of which biology was part, started out as having a goal of raising \$360 million. Because it finally went so well, we achieved over \$400 million, which was for that time very large for a public university. We had a number of goals aside from biology. We had other buildings and endowed professorships. Mike was trying to get ten per year, with the notion of that being important for keeping star faculty members here.

We had one component that I contributed to; I've always been involved in investment and interested in it. Mike had a whole series of needs that didn't sound, in his view, very sexy-- graduate student support and departmental support and a whole set of smaller items, and why would people give money to that? I said, "Why don't you put it all together and call it Cal Futures?"--with the idea of future investment, with hopefully a large return. That was a name that stuck and it's still being used.

Biology was the centerpiece. What really made it go in the end were two major gifts-- one from the Valley family, which was \$15 million. They had been involved as one of the early owners of the Oakland Raiders. And Gordon and Ann Getty put in \$15 million during the last spring that Mike was chancellor. That really put it over the top, those two big gifts.

Hughes: Were they specifically approached?

Park: Laetsch was involved in approaching the Valleys. The idea was to name it the Valley Life Sciences Building. Mike knew the Gettys and had spent time with them. They felt it appropriate to do something for the university, only they didn't want their name on it. They wanted to take a central courtyard of LSB and name it after Sherry Washburn because Gordon Getty was so interested in hominid evolution. Sherry Washburn had been a major figure in that continuing study, and so they wanted to honor Sherry Washburn. So I think that's the way it finally worked out.

Hughes: That was the carrot?

Park: Well, to do any fundraising, you need those big gifts at the bottom of the triangle that the fundraisers always draw; the little gifts at the top. We started filling in the top, and we didn't have the base. [laughing]

Hughes: When I read your chapter, I thought, that was late in the game to start fundraising! You must have been worried--

Park: The whole business was risky. On the other hand, you don't achieve great things without a degree of risk. It turned out well. If we hadn't had the problem and hadn't made the effort, we wouldn't have gotten those gifts.

Creating Three Mega-departments

Hughes: One of the major points that the external committee made in their first report was that it was really important very quickly to begin changing the administrative structure so that it reflected changes in science.

Park: Well, it got changed at the departmental level very significantly.

Hughes: But not immediately, right? The first meeting of the biology faculty in the Faculty Club was not until 1984. So what happened between the late 1970s and 1984?

Park: There was no administrative change until people occupied the new buildings. The occupation of the first building was about '87, '88--something like that.⁵ It would have been very inconvenient until people were all together in one place to have an administrative change. Louise could give you the exact dates, but my recollection is it was the late eighties when the administrative change followed the people into the new structures.

Hughes: What do you mean by the administrative change?

Park: Departmental changes. Converting those thirteen departments into about three.

Hughes: Was that essentially it?

Park: That was really it.

Exclusion of the College of Natural Resources

Park: I had wanted more than that. I had wanted to see the College of Natural Resources included in the administrative changes. That obviously was politically not going to happen.

Hughes: Why?

⁵See document in appendix dated July 23, 1989 which signals completion of the reorganization of the biological sciences. Faculty had of course moved into the new buildings prior to this date.

Park: When we were doing the affinity groups, we had gotten very little cooperation from the faculty of the College of Natural Resources, except for genetics and the plant biologists. The geneticists were important, and they came across. Our plant biologists out of botany and others joined their CNR plant biologists in a new building, but under their leadership. But the rest of CNR was as conservative and self-protecting as agriculture itself is, for some very self-serving reasons. Those faculty were all on eleven-month appointments, not nine-month appointments. They saw themselves as losing significantly.

They had also assured staff support, research support from Hatch funds and various others. They had staff research assistants, SRAs, whom they didn't have to justify from year to year. In other words, a lot of the resistance came because they were in a very preferred position in the university. They saw themselves as losers in the reorganization, with a very competitive faculty [in the College of Letters & Science] who were much more successful in getting grants than they were. And so finally I said, "Well, we'll just treat this as a pig does trichinosis. We'll just wall them off, and eventually it will come down and the thing will get resolved." [chuckling]

In fact, much of that has happened. The [current] dean, Gordon Rausser has been there five years now. (I'm his chief fundraiser right now, and the reason I'm working with him is our whole department, the botany department, at least the physiological part, became part of plant biology and has been very strong.) Gordon has ended all those special CNR funding deals, and a lot of the very conservative people have retired. Resources are now put out on a competitive basis.

The College of Natural Resources' Alliance with Novartis

Park: Gordon Rausser has put together this deal with Novartis, which I think is a very good one. I was very active in lobbying for it with the chancellor and others. It doesn't sit well with the liberal mind, which is under the delusion that the university is somehow free from political and financial pressure. [tape interruption]

I really believe that the future of research funding is going to be more with the private sector, the way the whole world is moving from government at the commanding heights to the market of commanding heights. As long as academic freedom assurances are there, as they are in the Novartis deal, and the money is distributed by a committee which is dominated by Berkeley faculty, the program will work. There is faculty review of the program, in which Novartis only gets first rights to negotiate on inventions, and only a portion of them, not all of them. There are many assurances which I think make this an excellent model agreement.

Initially, the chancellor, Robert Berdahl, was very suspicious of this agreement and how it would fly with the regents and what his problems were going to be. I know Berdahl

quite well because when I was at [University of] Colorado [at Boulder, 1994-1997], he was at Texas. With the people in the Big Twelve, the student athletic conference, the two of us really put the heat on Nebraska to stop taking so many nonqualifiers as athletes. We finally appealed to nine the other [university] presidents, and they went along with us. We voted Nebraska down, so we're not loved there.

I worked with Berdahl in some difficult situations and I think explained to him what I really believed, that to make his mark at Berkeley as chancellor for more than dealing with the problems of earthquake-prone buildings--he really wanted to do something in higher education--he should take a very close look at this agreement, because I thought it was the future of what would have to happen in a lot of research. He'd put a feather in his cap by having the first one of these agreements that really worked. And so I think he really became convinced. He certainly speaks that way now. And he sees the corporate alliance as an accomplishment, not as a threat. Again, there's only one reason for doing something; the rest are reasons for doing nothing. It was an example of leadership.

Hughes: What was the basis for his initial hesitation?

Park: I think he saw political retribution [from] the legislature and others because they would perceive that we had given away a public resource to the avaricious interests of private industry. And that's the way it would spin politically, unless he was given a lot of ammunition. There was a lot of good P.R. work done to show that was not the case.

Hughes: And there was criticism.

Park: Oh, of course. There's always going to be; you know that before you start. The question is, are you in a defensible position? One of the rules is, never defend an indefensible position; retreat to where it's defensible. [laughter] It's just one of those administrative rules.

Anyway, we were talking about the College of Natural Resources and the fact that Rausser has made many changes, and I believe they are good changes. I think if the biology reorganization were to come along today that the college faculty would be very much involved in it.

More on Affinity Groups

Hughes: Well, getting back to the reorganization, we talked about the external biology review committee. But there was, of course, an internal biology review committee. Was it building on the work that you described with Carpenter, the inventory concerning where biologists were and what their affinity groups might be?

Park: I, of course, was not at their meetings. Dan could tell you a lot more than I could.

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Park: I think Fred and I pretty much came up with the intellectual affinity group, trying to find a way to describe what we were doing in as few words as possible.

Hughes: It must have been hard to define.

Park: I think it is hard to define. You strip it all away and say, "What are we really trying to do here? Does our present administrative organization reflect what our faculty is interested in?"

One of the things that lead to the affinity group idea was that in the early seventies I would go up to have a drink after work at the bar of the Faculty Club and talk to various faculty members. There would be groupings of younger faculty--Beth Burnside was one of them--who were having their cell biology seminar up at the Faculty Club that evening. Now, this was not part of any administrative structure. The younger faculty were deserting the departments and forming their own groups. When I looked at what they were doing, I realized these were intellectual affinity groups. And that's exactly what we were talking about. There was something going on in parallel that was outside the departmental seminar. Sure, that was fine. But they were much more interested in what people were doing in other departments. It [the term affinity group] was naming something that was already happening with the younger faculty members.

Hughes: You spoke of how each of the formal, long-standing departments wanted a geneticist, a biochemist, etc. Because biology is increasingly interdisciplinary, I would think that it would be difficult to decide whether any given professor should go into biochemistry, cell biology, genetics, whatever.

Park: When you only have three groupings, it's much easier to fit people in. Organismal biology, ecological biology--that's where all the taxonomists would fit. Cell biology is for people who work at that level, like Beth Burnside. If you have molecular biology, that's where any geneticist would fit, unless they were population geneticists, and then they might be way over at the other organismal end.

I don't think anyone was put where they didn't want to be. I think Dan really worked that very skillfully. There were people who maybe didn't like the reorganization, but there wasn't much they could do about it. Probably out of two or three hundred faculty members, there are always going to be five or ten who are pretty upset about things, but you can't allow that to stop what you're doing. You just try to work things out the best you can for them.

Dealing with the Academic Senate

Hughes: Have we sufficiently covered the bureaucratic requirements to go ahead with this plan? What did the chancellor or any of you administrators have to do?

Park: There was a main one we haven't covered, and that is that at Berkeley there is a tradition, from the days of President Benjamin Ide Wheeler, of shared governance. Particularly in academic matters, one is supposed to--and I think we do--consult the Academic Senate. They would normally send a proposed reorganization to their Committee on Educational Policy [CEP]. CEP would write up a report and send it to other committees and to the chancellor, who then would send it on to other appropriate people for response.

I consciously did not ask the advice of the Academic Senate in this matter. I sent them all the material we generated so that if they wanted to respond, they could. But I didn't specifically ask the question, because the senate tends to be dominated by people who want to preserve the traditions of the institution, and that's only right. As a matter of fact, back in the sixties that was very fortunate because if all the changes that had been demanded occurred, the place would have been in a hell of a mess. It's just lucky, at that point, that it did take a long time for things to filter out [of the senate].

So we really didn't ever ask the advice of Academic Senate until the reorganization was about to happen, the buildings were going up, all the rest was going on. And I knew that the senate could not tell three hundred faculty members they couldn't do what they wanted to do. But if I had asked that question earlier--because I was the person who was supposed to contact the senate on this--they could have stopped the reorganization in its tracks and said that this whole development--

Well, what would I have said? Let's assume that I am a senate member, sitting on the CEP, and a vice chancellor comes to me with this proposal to take all this money and spend it on the biologists. I'd say, "Look, there are people in this university besides biologists. What about all the needs in the music library? What about the need to get humanists here? What about all the rest of us? You're absolutely distorting the priorities of this university. We're all important, too. You're being much too precious about biology, and I'm absolutely opposed to this. We need a total reconsideration of what we're raising money for, and it has to be more inclusive." That's what I would have said.

As I said, to make these things work, you really have to understand the deepest fears and motivations and desires of faculty members. So this is just another trap you could fall into. You could stop the thing dead in its tracks if you didn't play the senate card correctly.

Hughes: There wasn't a mechanism to force you to deal with the senate?

Park: Well, they could have forced it, sure. They could have said, "We're really upset about what you're doing, and we want you to come over and talk to the committee, and we want a

chance to find out exactly what's going on and a chance to respond." They never asked me for that, even though they were totally informed about what we were doing.

Hughes: Interesting. It was not only the funding issue, which was totally focused on biology, but there were also questions of dismembering departments and tenure issues and probably many, many more issues that I'm not even mentioning that were the rightful area of the Academic Senate.

Park: That's true, but I tried to make sure that none of those really sensitive things would be issues; they'd all be taken care of. I guess in retrospect it's rather amazing that we were able to put all those pieces together in such a way that allowed this whole situation to happen because there were so many points where you could have made a mistake, almost an irretrievable mistake. One can handle small mistakes and get out of them. But if you make a big mistake, it can become almost irretrievable.

Hughes: And the Academic Senate could have--

Park: They could have stopped it.

Hughes: There's some interesting correspondence from Sheldon Zedeck, written in February 1985, in which the Senate Committee on Educational Policy criticizes the reorganization plan on several counts. Some of the points that he made as chair of this committee were, and I quote: "The plan seems to offer the potentiality of a massive disruption of education programs in exchange for a quite unknown future." And then another point: "This plan with its large-scale realignment of faculty includes no discussion about the implications/advantages/disadvantages with respect to undergraduate education. No course plan is presented..."⁶

Park: Oh, we responded to him. I can't remember what we said.

Hughes: I don't expect you to. How did you keep the continuity of courses?

Park: A catalog has to be put together a year and a half ahead of time, and we had to get all the courses approved and in the catalog and through the course committee, prior to the official reorganization and everything else, and the two didn't coordinate properly. And then we got the senate, with a certain amount of grumbling, to go along and approve all of this because they realized they couldn't stop it if they wanted to. But it took a little bit of--I suppose it would be seen as administrative hardball to get all this stuff through, without leaving the senate too ruffled. And I think they're quite satisfied with the way it turned out.

⁶Sheldon Zedeck to Vice Chancellor Roderic B. Park, February 13, 1985. (Black binder: "Biology Reorganization," to be donated by Louise Taylor to The Bancroft Library archives. Hereafter, "Biology Reorganization" binder.)

I was once told--and this is on a different subject, but it's about pushing--that you should never lie unless it's really, really important. Well, I don't think you should push the senate unless it's really, really important. I was playing some brinkmanship with the senate because otherwise we could never get this thing through. It would all be held up on technical matters.

Park's Commitment to the Reorganization

Park: One other thing I'd like to add here: though I never have and certainly didn't then regard myself as a professional administrator, I ended up doing a lot of administration. I started getting a lot of offers to go to other places--[university] presidencies. There were two reasons I didn't go. Probably the less important one was that I like living in northern California, though I of course did go to Colorado. But the real reason I didn't go was I felt if I left, the whole biology thing would collapse because of these senate pressures and other pressures, and I really had to stay long enough to make it work.

Here was an idea that I had been working on for my whole career. It had started back, let's say in '64, and then by '74 I was dean and I had it going then, and here we were in '84, so we were already into it twenty years. We were into it for really twenty-five years from the very beginnings to having it actually happen. You put that much effort into improving the institution--not that everyone would agree--but I think it did, and you want to finish the effort. Improving the institution, that was something really significant and important. Who was president of the University of Houston for five years doesn't make a damn in the bigger scheme of things. Presidencies have become pretty trivialized; they come and go. But big changes don't. So I stayed. After this was over, I took my shot at what it was like to be a chancellor and in retrospect, I made the right choice [laughing] staying at Berkeley to finish the biology reorganization.

Faculty Meetings on Reorganization

Hughes: I'd like to capture some of the color of those meetings in the Faculty Club.

Park: When the first report of Dan's biology committee [Chancellor's Advisory Committee on Biology, CACB] came out, we set up a meeting of all the interested parties in the Great Hall at the Faculty Club. It must have been in January 1984. There were probably over a hundred faculty members there.

Hughes: All from biology, right?

Park: Well, anybody can come to meetings, but all the biologists were there. I was a little bit nervous because I could see the alignments of people sitting together, and you start reading the audience in terms of what groups were going to say what. I made a few introductory remarks about the history, where we were, and the external committee, and the internal committee, and the proposal for change. This [current meeting] was a chance to discuss it.

I think Ralph Smith and some other older faculty members came up, saying that they thought it was fine to have these discussions, but remember something: we are the most distinguished program in biology in the United States, seen by the last reports out of Washington, and that nothing is broken here so we shouldn't be fixing it; things are just fine. It's fine to talk about these things, but there is no money and this is all fluff. Don't really understand why we are here. I think Ralph and somebody else spoke.

And then some younger people got up--Paul Licht and some others. They didn't dismiss the reorganization; they thought there were some things happening in biology that merited a very close look, and they felt that maybe there were some things that should be improved about these organizations. Maybe there should be some of the rough edges cut off, such as such-and-such and such-and-such. Anyway, this discussion went on for an hour or so. I sat up there, and I think I said [in the chapter] that I felt that I was slowly sinking in a vast viscous bowl of academic oatmeal.

Hughes: Yes, you did. [chuckling]

Park: [laughing] About to drown! I just hoped the damn meeting would get over without further damage. It's the kind of thing that drives you into the bar at the Faculty Club afterwards. So that meeting ended, and it was neutral, I think, in terms of the total feeling that came out of it.

The next meeting was held in the library of the Faculty Club, which is now the Glenn Seaborg Room upstairs. That had almost as many people. I began, people got up and spoke, and I think members of the [CACB] committee spoke. Dan may have spoken. The second report was out. In general, the feeling was somewhat more favorable than at the first meeting. But there was still the argument, there was no money. And so what was all this about, anyway? Why are we wasting our time? Where is the money? You're just causing a huge uproar here and upsetting everything. It was the chicken and the egg thing. Without the program, I couldn't get the money; without the money, I couldn't get the program at that point.

Hughes: Didn't that play on your own fears? You didn't have the money yet.

Park: Of course, I didn't. We didn't have the money at the time, and we weren't sure we were going to get it because we were arguing with the state legislature on the whole animal rights issue and Elliot Katz.

Did I describe in the chapter the interview at KQED?

Hughes: Yes.

Park: That almost led Dan to an entire withdrawal of Jewish support from KQED. He was really mad. He spoke to a bunch of his friends. I don't know what happened. [chuckling] It could have been the death knell for KQED.

Anyway, that was the last formal meeting we had [with the faculty]. By May 1994, the legislature came up with the money. After that, there really wasn't a need for a meeting. We were going ahead, and we were going to occupy these buildings by affinity groups and make the assignments, and the facilities were all set up for specific people.

Over those four or five months, I followed it from a program that I felt would never get off the ground to one I couldn't have stopped if I had wanted to. People would have run right over me. [laughing] I found that very often projects were that way. You say, "My God, how are we going to get this to happen?" And suddenly you realize, as it gathers momentum and the resources come together, that you are inconsequential; you couldn't stop it if you wanted to. People wouldn't let you.

More on Animal Rights

Hughes: You said in your draft chapter that you regard solution of the animal rights problem as the kicker that moved you into the final phase. You raised three stumbling blocks that by then had been solved. One of them was the innate inertia of the faculty or any body that is more or less happy with the status quo. And then, of course, the money problem and the animal rights problem.

Park: The animal rights came before the money. We had to solve that, and then we could get the money, and then the whole thing would go.

Hughes: The animal rights issue came out of left field, didn't it?

Park: As I said earlier, George Maslach asked for AALAC accreditation when we were nowhere close to it, and suddenly we were put on probation. [laughs] That allowed all the animal rights people--Elliot Katz and Gladys Sergeant, the flea and tick powder heir, and all those people--to go up to Sacramento and say, "You should never give any money to Berkeley." It was a wonderful political platform for them because, being about Berkeley, it got a lot of press.

There is a truism that if you are in any kind of social protest movement and you have limited resources, you don't go to Wichita State to make your case. There are only a few

universities you go to if you're going to get on the evening news: there's Berkeley and there's Harvard--a few places. But this was an ideal setup if you're an animal rights person, and here's Berkeley in trouble, and I can get on the steps of Sproul Hall, and I can go to Sacramento. I can get all kinds of lines in the press, which I couldn't get if this was going on at Sacramento State. So Berkeley is a wonderful political opportunity for any protest movement, and I've been through enough of them so I could have run one very well. I know where all those switches are. I see the kids--young people--on television now, with ethnic studies and cutting it back and Third World College and all the rest. I've been through all those arguments. I know all the strategies. I know what's going to happen.

The UCB Apartheid Protests, 1986

Park: I named this unpublished book *It's Only the Janitor*. That came from the apartheid protests in 1986, in which the shanties were put up. A group was organized in Berkeley to put up the shanties. Our police intelligence wasn't very good. We knew that they were building prefab shanties all over Berkeley. One afternoon they came from all four corners of campus, carrying this stuff in, and nailed them up in front of California Hall and started their protest.

It was a question of what to do. Heyman wasn't around. It was sort of like the Gulf War. You have to do two things. First of all, you have to get a declaration that this is improper and inappropriate. The shanties were declared a fire hazard by the fire marshal. Within twelve hours or so, you have to bring the thing to resolution with overwhelming numbers of police in this case, or people are going to get hurt. Otherwise, they'll be there for months. The ACLU [American Civil Liberties Union], the legislature, the courts, the alumni--everybody will get in on it and prevent any action to remove the shanties.

What happened was we didn't have sufficient police to control the situation. People started throwing rocks through the windows of California Hall. I was there through the night as the chief administrative person. I put on my sailing clothes because I knew it was going to be cold and I was going to be up all night. About one o'clock the campus police had to stop arresting people because they couldn't both hold the lines and arrest people, and get them out of there into Santa Rita [Prison]. So they failed in a sense. Terry Bowles was chief of police. He called the Oakland Motorcycle Corps. They couldn't be there for two hours. Well, of course, that's a pretty fearsome group. Students at Berkeley have no idea what police are like until the Oakland Motors show up. Well, they finally showed up, and they restarted the arrests.

Chancellor David Gardner wanted to know early in the morning, before the reporters called him, what was happening. They let me into the building [California Hall] through a side door about five thirty in the morning, and I called up David Gardner and told him all about our sorry predicament. He's a very matter of fact person; just keep him informed. I went to get out of the building, and they had locked all the doors because people were

surging in against the building and trying to climb through the windows. There was broken glass everywhere. So they [police] said, "We'll let you out through the front." Well, out through the front they had these big spotlights [shining] down, giving the appearance of a procenium arch. All the shanties were there. They had arrested some of the people, but they were all gathered right in front of the building. The police opened the door, let me through, the cry went out, "They're coming through the back! They're coming through the back!" And I walked out through the shanties, and they looked at the way I was dressed and said, "It's only the janitor! It's only the janitor!" [laughter]

That sort of became my job--taking one administrative mess after another and trying to deal with it and work with it and get things back on track, and so it really described my role.

Hughes: Just call in the janitor.

Park: Yes! The other thing that was funny about that was that it came from this bastion of Berkeley political correctness which didn't seem to realize that such individuals had been called custodians for years. [chuckling]

Building Construction

Hughes: Getting back to reorganization, we haven't talked about construction per se.

Park: That's another whole story. The construction is very interesting. We started with the Kaplan, McLoughlin, Diaz study back in '78 or so. We got money for that from Bill Baker. Jerry Beavers from the legislative analyst's office was really upset because we had gone ahead, outside of state regulations, and done a study without any kind of approval from capital committees or anyone else. Nobody else raised a question, but technically we hadn't done the right thing.

Then, when we decided we were really getting serious about this in the early eighties, I did something else that was outside state fiscal rules. I went ahead and hired Elmo Morgan and Dean Gustafson from Utah as project managers to start putting a project scheme together for how this would go in terms of architect interviews, hiring--all the things that you have to do.

Now, Elmo was a former vice president of the university for capital projects. He knew Gardner very well. And Dean Gustafson was a Mormon, knows Gardner very well, and did a lot of projects for him in Utah. I never went out to bid or anything else. I hired Elmo and Dean as consultants and then kind of moved them on in. I heard very little about it. Again, you don't break the rules unless it's very, very important. And I needed people who had the confidence of David Gardner. So that, again, was a time when you bend the

rules a little bit because otherwise it's not going to work. Then they drew up elaborate project management schemes, which Louise can show you, of all the buildings and the time courses to get things done.

Hughes: Louise told me that there was a problem once the buildings were up.

Park: Oh, yes. That was another matter. It's rather typical. Louise is very sensitive to that. You put in all kinds of specifications, and the buildings don't turn out that way, and the [fume exhaust] hoods don't work just right, and various other things happen. Gunther Stent claimed the building was terribly noisy, and it turned out to be his hearing aid. [laughter] We went through all this trauma all the time.

Integrating the Molecular and Organismic Approaches ##

Hughes: There was apparently some tension over the fact that the two new science buildings--Valley and Koshland--were described by I don't know whom as being for high-tech biology. Also, the original composition of the CACB was pretty molecularly oriented. Was there an implicit message that high-tech molecular biology--the newer genetic, cellular and molecularly based sciences--was the science that counted?

Park: Certainly, from my point of view, that's not the case. What I always expressed was that I wanted biology at Berkeley, which was a large place and could afford to have a very encompassing program, to run all the way from what you see outside the window to the details of chemistry at the laboratory bench and everything in between, and we should not forget that biology is ultimately about the biosphere. It's about what goes on outside the window.

Now, there are places that are much smaller and can't do it all. They have resorted to doing much more molecular biology, for the most part, and much less with the biosphere. Stanford gave away its botanical collections; all kinds of places are doing that. I think California Academy of Sciences has them now. I've always said that I think that's a terrible mistake because if you emphasize only the technical part and don't relate it to the larger questions of biology and the biosphere, you become like an amateur radio operator who has tens of thousands of dollars' worth of equipment and absolutely nothing to say.

If you go down to the basement of LSB and look at the University Herbarium and the Jepson Herbarium. They are beautiful facilities. If you look at the MVZ [Museum of Vertebrate Zoology], you'll find that all the biospheric and evolutionary work is still going on, but a lot of it now has the ability to characterize organisms in molecular as well as morphological terms. For example, Bruce Baldwin at the Jepson is doing molecular biology to study relationships among tarweeds. In other words, there's a lot more information in a dried plant than just what it looks like.

So I think we're seeing some classical biology done, some of it in the same way, but some of it using more modern tools. The tension, I think, came from the classical biologists, who were frightened by the developments [in the molecular approach]. If you spoke to them now, I don't think you'd find their fears were justified. But I think that Martin [Trow] is probably right in that people are threatened by change.⁷ Change is uncomfortable. It always is.

The Park-Koshland Team

Hughes: We talked off tape about the role of personality, particularly in terms of how you and Dan worked together. Would you comment a bit more?

Park: Yes, sure. The partnership that Dan and I had was absolutely essential to making this reorganization happen. Dan, from my view as dean, had over the years eschewed administrative assignments. He was primarily a very fine faculty member and teacher and researcher and I think was pretty impatient with the Academic Senate and the fact that it took forever to get things done. The senate didn't seem to be very incisive, and it was very gloomy. Dan is an impatient person; he just didn't like dealing with the senate.

The first time I ever saw him step forward--and I never expected he would do it--was in this role of heading up the Chancellor's Advisory Committee on Biology. It was an extraordinary job that he did, with huge energy and commitment and passion. There was probably no one who could have done it as well. He's regarded as a brash scientific bad boy by some of the more classical biologists. On the other hand, he heard them out. He was patient. Things I hadn't seen in Dan before. And he carried the prestige that allowed him to deal with the Gunther Stents and other angular people.

So Dan stepped up in a rather remarkable way to fill a job that I wouldn't have expected he would have wanted to do or would have done. But he obviously saw it as very important and worth giving a good part of his life to at that point, whereas obviously, in a number of prior more trivial activities, he hadn't seen it as worthwhile. [chuckling] So that was a remarkable, generous commitment on his part, where he played a very essential role.

I played a role of interfacing in a delicate way with the Academic Senate, with student groups that were interested, certainly with the employees. I had to work on that, and with and through Heyman, with the regents, with the legislature, and with the fundraising.

Hughes: Were you spending most of your time on reorganization?

⁷Martin Trow, "Leadership and Organization: The Case of Biology at Berkeley," in *Higher Education Organization*, Stockholm: Almqvist & Wiksell International, 1984.

Park: No, there were many other demands. Biology was a major change commitment. I don't know the percent time. I suppose I was spending 25 percent to 30 percent of my time on this, something like that.

I think there was another part besides Dan and myself. That other part was Heyman, who dealt with the heavy hitters and the fundraising, and in a more direct way with the legislature. He and I stayed in good contact, but he had more of the day-to-day outside contact. So there are really three musketeers!

Very often, when a situation becomes critical and people think it's important, strong people will come forward and begin to take it over and do it. Then you look back on it and say, "Where are those people now? Those were the days when giants walked the earth." But the fact is, if a similar situation came up now, I think people would come [forward] again, assume leadership roles, and do the same things.

Results of the Reorganization

Hughes: Did the reorganization do what you had hoped?

Park: Yes, I think it has. I haven't kept up on it in recent years. You ask about the quality of the graduate students who are applying for these programs, and you look at the amount of money that's coming in, and you look at undergraduate enrollment (25 percent of undergraduate majors, and you look at the quality of faculty we're getting. I think all of those parameters show considerable improvement.

Hughes: What about ranking of departments?

Park: I don't know of any recent national rankings, but I'm sure that the university ranks very highly. From other universities I go to, Berkeley is highly thought of. What really tells, though, is what you recruit in terms of faculty in competition with other institutions--can you meet salaries--and what you get in terms of graduate students and graduate student support. What kinds of jobs the graduate students get. Those are very telling, and we are doing well.

I'm convinced the reorganization was the right thing to do. Maybe we were a little late; we should have started earlier. It was something that I started, I finished, and in retrospect, I'm very proud of it.

Hughes: Is there anything more you want to say?

Park: Not now!

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REORGANIZATION OF BIOLOGY
AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, 1980s-1990s

An Interview with
Louise Taylor

Interviews Conducted by
Sally Smith Hughes
in 1999



Louise Taylor

INTERVIEW WITH LOUISE TAYLOR

THE REORGANIZATION OF BIOLOGY AT THE UNIVERSITY OF CALIFORNIA, BERKELEY¹[Interview 1: January 7, 1999] ##²**Taylor's Roles in Reorganization****Creating a Program Planning Guide, Networking, and Serving on Building and Planning Committees**

Hughes: The first thing to establish is your relationship to the reorganization of biology on the University of California, Berkeley, campus.

Taylor: In 1980, Provost [George] Maslach asked that I do some analysis for a Project Planning Guide, as it was called, so that we could go forward to the state for capital projects funding for some new biology buildings. My office and I did the analysis, and then he asked me to talk to Alex Glazer, who was very much involved with Dan Koshland in the reorganization.

Alex got me involved with Tom Koster, who is now director of the capital projects program for UCB. Alex asked Tom and me to work with him in rewriting a draft consultant's report on the Life Sciences Building [LSB]. I thought that was the end of my role in biology.

However, [The Vice Chancellor] Rod Park then asked me to be staff to a building committee; Tom Koster and I would both staff it. I did that. The building committee was formed in probably 1982. We started planning for the first building, Project 1, as we called it, and that became the Life Sciences Addition, LSA, and we did preliminary planning on Project Two, the Genetics & Plant Biology Building.

Hughes: What exactly was Project Two?

¹Louise Taylor has kindly donated to The Bancroft Library a reference reference binder containing documents on reorganization of biology at Berkeley. Additional documents referred to in this oral history are also on deposit in The Bancroft Library but as of 2003 have not yet been processed.

²## This symbol indicates that a tape or tape segment has begun or ended. A guide to the tapes follows the transcript.

Taylor: Project Two includes Koshland Hall, the garage, teaching facility, and the Pat Brown Grill. When planning got to be too intricate for the building committee alone to do it--the building committee was basically a policy committee--we set up program planning committees for each building. Rod Park also asked me to staff those program planning committees, but I needed help so my staff became involved³.

Chancellor Mike [Ira Michael] Heyman, as you probably will read in Martin's report,⁴ went to [UC] Systemwide and requested funding. It was based on the budget that I put together for all three buildings. We had some negotiations with the systemwide budget office and Bill Baker, who was the Vice President for Budget and Capital Projects at that time. This was probably 1980 or 1981.

Then Mike Heyman was asked to testify at budget hearings in Sacramento and brought along, at Bill Baker's suggestion, Dan, Alex [Alexander N.] Glazer, and possibly Bob [Robert M.] Glaeser, who was dean at the time. There were two or three or four faculty who testified in Sacramento as to where biology was on the Berkeley campus and the great need it had for new buildings in order to do modern biology.

I had been very active on the program planning committees for the first project, LSA and for Koshland Hall, and on the building committee. We were beginning to form a program planning committee for the Valley Life Sciences Building in 1985, when I understand Horace Barker suggested my name to Dan as someone who could work more closely with Dan and the faculty. The Chancellor's Advisory Council [on Biology--CACB] had been formed in around 1982 with Dan Koshland as its first chair.⁵ When he and other council members saw that there really would be new construction (LSA, 1984), they recognized this would provide a unique opportunity to reorganize the faculty. They set about, on a very confidential basis, drafting a reorganization plan, which they presented to The Vice Chancellor⁶ in December 1984. This first draft was not well received by the biologists and The Vice Chancellor and Dan emphasized it was only a draft and there would be a new draft created in a more democratic fashion by the council. It was obvious CACB would have a lot more work to do and staff support would be helpful.

Dan evidently went to Mike Heyman and asked for staff support for the Chancellor's Advisory Council on Biology and specifically asked for me. So I put that hat on and started working with the advisory council. Even now, after I've retired and come back to the university as a consultant to the vice chancellor on biology, until they can find a replacement for me, I'm going to CACB meetings as liaison between the administration and the

³Loris Davanzo worked with the faculty on Project 2--the Genetics and Plant Biology Building, later named Koshland Hall, and on the Northwest Animal Facility; Sandy Ellison and Charles Upshaw worked with Project 3--the Life Sciences Building, and I continued with Project One--Life Sciences Addition.

⁴Martin Trow, "Leadership and Organization: The Case of Biology at Berkeley," *Higher Education Organization*, Rune Premfors, ed., Stockholm: Almqvist & Wiksell International, 1984, pp. 148-178.

⁵See the reference binder for a "History of CACB Membership" from 1981-82 through 1999-2000.

⁶Roderic B. Park

biologists. I worked with CACB on the second, third and fourth drafts and on the return visit by the external review committee.⁷

In 1986 or so, Rod Park appointed me as special assistant to the vice chancellor for biology; he was vice chancellor at the time. As I saw my role, it was to try and remove bureaucratic roadblocks and to make things happen from the administrative side. I was also director of academic planning. When we got into the reorganization, it was very much a role for our office because I believe that academic planning goes from the ground up here at Berkeley. The administration can't impose a plan on the faculty for the academic disciplines.

I was very active through the reorganization of biology as staff to CACB and as staff to the vice chancellor. I developed a lot of the plans and policies for the actual process for reorganization of biology, once the academic component was developed--how do you transfer staff, how do you transfer budget--all these kinds of things. Reorganization took place July 1, 1989, as I recall.

Hughes: Do you mean when Berkeley went to the new system?

Taylor: Yes, the new organization for biology, the disestablishment of twelve departments and establishment of three new departments. Rod Park and my office got that through the campus review, Berkeley's Academic Senate review, the UC Systemwide review, and we got the required approvals from the president, the regents and the state. And that, of course, was in my bailiwick of academic planning.

Hughes: Which is your background, right?

Taylor: I started in the chancellor's office on the budget side and then moved to planning after about eight years, and have been in planning ever since. With my budget and planning background--

Hughes: You were perfect for the job.

Capital Planning

Taylor: Yes. And then I got into capital projects and in capital planning in '87 or '88 when Rod Park came in one day, and he said that one of our offices, Physical Resources (now called Planning, Design & Construction), had put a proposal to Systemwide for the "decanting" of

⁷Membership included John Abelson, California Institute of Technology, Robert Barker, Cornell University, David Botstein, Massachusetts Institute of Technology, Guy Bush, Michigan State University, William Dawson, University of Michigan, Mary Jane Osborn, University of Vermont, George Palade, Yale University, Keith Thomson, Yale University, Seymour Van Gundy, UC Riverside; February 26, 27, and 28, 1986.

LSB, as we called it, the moving out of people and then moving them back in. The proposal was for \$10 million, and UC Systemwide had sort of laughed in his face and thrown it back at him and said, "If you can come up with something reasonable in three weeks, we'll consider it."⁸ And so he said, "Louise, I want you to develop a reasonable plan that Systemwide can accept."

Fortunately, I had the cooperation of so many people in the biology community, and we pulled together a team, and for three days we sat in the chancellor's conference room. The deans for [College of] Letters and Science, Division of Biology⁹ and [College of] Natural Resources¹⁰ were there, and all the people that had the authority to make decisions on behalf of the faculty, teaching facilities, museums, etc., and our physical plant people. We decided how we would go about decanting LSB. We would send our staff out to measure, for instance, the Wellman parking lot to see if we could put trailers in there. That would allow us to have the Biology 1A, B class labs served there, along with some of the other functions (e.g., Biology's rare book collection and specialized lab facilities). We just made all the decisions in those three days. There were about thirty of us around the table for three days. We came up with a proposal for \$4 million, and Systemwide approved it and helped us fund it.

Decanting the Life Sciences Building during Renovation

Taylor: Following this, I was supposed to be moving away from capital planning, but people weren't doing much on the capital side to implement the decanting¹¹ of LSB. So I got charged with making sure that the move out of LSB and the move back in functioned effectively. People were scattered all over campus. We had a directory out in front of LSB, showing which departments had moved where and how to reach people, because they were scattered all over. We were thinking of putting classrooms in the movie theaters in downtown Berkeley for a while. We did put classrooms in the International House auditorium. It was just amazing. Any place that was reasonable. We were looking at the Rec[reational] Sports Facility garage for housing the museums, but the ceiling was too low to stack museum cases. Just on and on. We looked at everything. We came up with a plan, we managed to do it, we stayed within budget, and we completed it within the scheduled time.

Hughes: How long was that time?

⁸Trudis Heinicke, Director of Capital Planning

⁹Beth Burnside, now Vice Chancellor Research

¹⁰Wilford Gardner

¹¹The LSB Decanting Committee, chaired by Roy Caldwell, included Jim Allison, Lewis Feldman, Anne Good, Bob Goodenow, Carole Hickman, Paul Licht, William Lidicker, Steve Lindow, Taso Melis, Jim Patton, Beth Weil, and Eleanor Crump, and reported June 15, 1988. This committee established the policies by which faculty would be moved out of LSB (i.e., "decanted") and the assignment of faculty to various buildings around the campus.

Taylor: Well, it kept getting extended. The construction of the Valley Life Sciences Building [VLSB] was supposed to start in '88. It got pushed back to '89. I don't think it actually started until '91 or '92. Finally, we were told we had four months to complete the move-out. By that time, we had moved people into LSA and into Koshland [Hall], and so it was just the people who were moving out of LSB who would be coming back into LSB, for the most part.

Hughes: Who were they?

Taylor: They were the University and Jepson Herbaria, the Biology Library, teaching labs and three major classrooms, and the faculty and their labs, everything except for MVZ [Museum of Vertebrate Zoology], which stayed in LSB during the construction and then only moved once into new quarters. They had so much to move and the specimens were so delicate that we decided (with Director David Wake's permission) it would save us money--and it would protect them--if they could stay and then just move into the finished facility. MVZ was the first to be moved back in and then the west end of LSB where they were housed was renovated in Phase II of the construction. That area primarily became home for the new biology library.

Other groups that moved out included the Electron Microscope Lab, which did not return, in part, due to the cost of renovating decent space and moving them; and the Scientific Photo[graphy] Lab which was disestablished. The Museum of Paleontology was in the Earth Sciences Building originally, but as part of the reorganization moved into new facilities in VLSB, along with the Paleontology faculty.

Handling Construction Problems in LSB

Taylor: Once VLSB was completed, there were a lot of problems with the building, the contractor and the sub[contractor]s; the faculty were very unhappy, so I got called back in to try and help resolve those problems.

Hughes: What sorts of problems?

Taylor: Construction-type problems. The floor tiles kept popping up in the hallway--not all of them: one there, one over there, six along there. We had to discover what was causing them to pop up, and there were some places in front of the library, for instance, where most of them were loose. It turned out they had been installed before the cement underneath was dry, and the moisture that was in the cement caused the tiles to pop up.

There were problems with leaks, on the fifth floor, primarily. The roof would leak into new faculty labs, drip on their expensive equipment, and all of that. We had four windows blow out, and we were afraid for awhile that all the windows might blow out in the building, until they were tested and found to be safe. In that process, they learned that portions of the windows had not been built or installed in accordance with design

specifications. My role involved working with Planning, Design and Construction¹²; the faculty¹³, special consultants and engineers, our Academic Facilities Office¹⁴, and the lawyers that were involved with the legalities of the whole thing, in order to resolve those problems. The biggest problem was getting people to do what they promised to do to the satisfaction of our faculty. Some of our faculty (e.g., Roy Caldwell) became technical experts in the problem-solving process.

Retirement and Current Position as Consultant

Taylor: And then I retired! So I'm back now, doing my biology work for [Vice Chancellor] Carol Christ, until she hires a replacement for me.

Hughes: "Biology work"?

Taylor: Anything for biology. At this time, Biology is our most innovative discipline on this campus and the most complex. The discipline is spilling over into other disciplines. I'm sitting here in a new bioengineering department, and the kinds of things that they are thinking of doing are just fabulous. Biology is making joint appointments with physics. They already have a strong relationship in structural biology between the Dan Koshland group in Stanley Hall and chemistry. They are becoming much closer now to public health because of the appointment of Ed Penhoet as dean.

The College of Natural Resources [CNR] and the Department of Botany

Hughes: How does the College of Natural Resources fit in?

Taylor: The College of Natural Resources-L&S [College of Letters and Science] Biology relationships have not always been really good. CNR didn't participate in the reorganization of Biology to the same extent that the L&S portion did.

Hughes: Why was that?

Taylor: Probably in part political. They didn't want things to change. There were old guard in there who were concerned the relationships and resources with the Agriculture Experiment Station would be impacted. They worked very hard to make an appearance that they were cooperating when a few faculty were doing everything possible behind the scenes to see that CNR would not be affected.

¹²Represented by Rob Gayle.

¹³Represented by Roy Caldwell.

¹⁴Headed by Roy Pickrell.

Hughes: Were they hurt as a result?

Taylor: Yes, I think so. Let me give you an example.

In 1987-88 a new dean, Wilford Gardner, was brought in. His charge was to develop an academic plan for the College of Natural Resources, and I was asked to work with him and his faculty to develop an academic plan for them. They should have been doing it when L&S Biology was reorganizing and trying to involve them. Only one department in CNR came out of that original reorganization--Plant Biology. The botanists transferred from L&S to CNR and merged with some plant microbiology people in Natural Resources. They were the only ones to transfer from L&S. A handful of faculty from CNR transferred into L&S.

That was an interesting thing because Botany had been asked by the [Academic] Senate to do an academic plan in the mid-80's. They kept proposing things that the senate couldn't accept as academic planning. The senate said, "That's not an academic plan. Go back and do it again." So--I don't know, '85? '86?--our office was called in to help Botany develop an academic plan. No, it had to be before '85 because it was before the reorganization. Botany had heard that they were going to be involved in this reorganization, that there probably wouldn't be a department of botany anymore. Botany was one of the original departments at Berkeley and the faculty couldn't imagine being disestablished or merged. They felt that programs and research would be devastated by such a move.

Loris Davanzo, in my office, and I met with Professor Russell Jones, Department Chair, and some faculty and asked them to tell us what their academic plan should be, and we'd help them write it. We'd be their staff to do this. Botany told us that they couldn't be a part of the reorganization; they had to maintain a department of botany, and they gave us their reasoning. So we wrote an academic plan around that and then asked them a bunch of questions, in order to fill in the blanks and to justify their maintaining the department.

Botany met and talked about the draft plan. They came back to us and said, "Hmm, this plan won't do. We see the need to merge with plant biologists in CNR and reorganize." They wrote an academic plan that fit that whole notion. In other words, once they thought seriously about their programs and future directions they realized that just staying Botany wasn't good enough. Unfortunately, this perspective was not unanimous and one or two faculty felt the reorganization ruined their research. On the whole, however, I believe the new department [Plant and Microbial Biology] has been very successful.

Initial Impetus for Reorganization

Surveying Campus Biologists in the 1970s

Hughes: What stimulated the reorganization?

Taylor: Around 1979, as I understand it, the provosts (George Maslach & Rod Park), Chancellor Bowker and the vice chancellor Mike Heyman were talking one day about the most significant academic problem at Berkeley. They felt it was Biology, and wanted to do something about it. They hired a student to go through the bio-bibliographies of all the faculty the next summer, and list every faculty name who appeared to have a grant that pertained to biology. There were something like 350 names on the list, where we only had roughly 225-250 recognized biologists on campus. There were biologists in Electrical Engineering, Chemistry, Optometry, Natural Resources, Public Health, Statistics, etc.

For example, Ted Lewis, Professor Electrical Engineering and Computer Sciences, was very much involved with sound and vibrations, and was one of the founding fathers of bioengineering in the sixties or early seventies. He was doing biology, as well as his engineering. Optometry had several biologists that were very important to biology. At the same time, the biologists were complaining to the administration about their space.

A Deteriorating Life Sciences Building

Hughes: I wanted to ask you specifically about some documents that are mentioned in Trow's chapter. Remember that it was published in 1984, before the reorganization had been completed. There was an internal assessment of Biology, and there was an external assessment, and he doesn't say by whom. They were damning.

Taylor: This is where Martin didn't have all the background, and he started his history late. My perspective on it goes back to '79, the period I just described. Simultaneously, the biologists were saying to the administration and to the BCDC, (the Buildings and Campus Development Committee, which was part-Senate, part-administration, that ruled on space at that time), that the facilities in LSB were just horrendous. They couldn't do modern biology in that building. It had code violations; it had leaks--it really was a disaster.

It was the largest building constructed west of the Mississippi for any academic program in 1932, but that was when biologists used microscopes on the top of desks and tables. We had all of these exhausts pipes going through the building and up the outside of the courtyard for fume hoods and for other kinds of instrumentation. They had brought in electricity for modern equipment until the electrical capacity of the building was overloaded. When something flooded upstairs, it leaked down onto expensive equipment two or three floors below. The electricity would be shorted and the whole building would black out.

We had a scholar here from someplace in eastern Europe. She had won a very prestigious scholarship to come here and study at Berkeley, and we either boiled her fish or fried her rats; I've forgotten just which. Six months passed, and a Ph.D. student, who had worked for years and was completing his research to get his Ph.D., lost his experiment because of building problems. I mean the building was a real disaster.

I wasn't involved at this early stage, but I understand they decided to form four committees.

Hughes: Who's "they?"

Taylor: The biologists working with BCDC, the Buildings and Campus Development Committee.

Hughes: The chairman of Biology?

Taylor: Well, it probably was more like individual biologists, the dean, and the chancellor's office.

An Internal Campus Review Report, 1981

Taylor: So however it happened, there were four committees¹⁵ formed that were part of an internal review committee. The committee consisted of four senior people who were well respected on this campus by their peers and who were not seen as people who were out to gain something for themselves.

Hughes: Do you remember who they were?

Taylor: It was Dan Koshland; it was Alex Glazer; it was Milt Schroth from the College of Natural Resources. [interruption] The fourth person was David Wake. At any rate, these four senior people were respected by their colleagues and by the associate and assistant professors, and each chaired a Review Committee.¹⁶

Hughes: Were they self-appointed?

¹⁵ The four committees were: Biochemistry and Molecular Biology (A.N. Glazer, chair); Cellular Biology (D.E. Koshland, chair); Organismal Biology (M.N. Schroth, chair); Ecology & Evolution (D. Wake, chair).

¹⁶ The Biochemistry and Molecular Biology Review committee, chaired by A.N. Glazer, included M.R. Botchan, K.J. Carpenter, S. H. Kim, N. J. Panopoulos, E. E. Penhoet, and W. Taylor. The Cellular Biology Review Committee, chaired by D.E. Koshland, included B. Buchanan, M.B. Burnside, W. Z. Cande, J. Gerhart, J. W. Fristrom, and F. Wilt. The Organismal Biology Review Committee, chaired by M .N. Schroth, included L. Feldman, P. Licht, S. H. Madin, R. L. Pipa, G. S. Stent, and G. Westheimer. The Ecology & Evolution Review Committee, chaired by D. Wake, included G. W. Barlow, H. W. Daley, D. Erman, C. Hickman. G. Robichaux. and J. L. Hardy.

Taylor: Well, I don't think so, but I wasn't really close to that. They may have been appointed by the chancellor. This is 1979, 1980, right around in there. Each of the four chairs had six or seven young faculty on their committee--(young Turks, as we called them) young, brilliant people who we wanted to have here at Berkeley twenty years hence. They were told, "You're important to us. We know the biology facilities are falling apart. What do you think you need at Berkeley to stay here? What will keep you here?"

These four groups, with these young people and these senior leaders at the head of the four groups, certainly realized that the facilities were terrible. The four groups represented all of biology. They went out and surveyed their colleagues at other institutions, where they thought there were good programs and good facilities. They even got to the point of saying, "We need twenty-five feet of sit-down bench space for these items of equipment. We need these kinds of specialized rooms." They were able to gather and compare all this detail and synthesize it into a very compelling report.

Hughes: And that was put into the report?

Taylor: And that was put into the report.¹⁷

Hughes: That's the internal report.

Taylor: That's the internal review committee report. I think they also basically said in that report, "Biology is in serious trouble not just because of the physical facilities but there need to be changes in the academic program as well. The only way that we can see to have success with the academic program is to reorganize the departments in some manner. But we can't do that unless we have physical facilities that are modern, that we can move people into. And then, when we move them in, we can reorganize."

They signed the report, which was something like August of '81, if my memory serves me correctly. Remember that Alex Glazer and Dan Koshland and several other people were talking to Rod Park, who had been appointed the vice chancellor at that point in time. In fact, Dan talks about the time he, Bunny (Marian Koshland), and Rod Park were talking about the problems in Biology and the need for improvements. I believe he marks this as the "birth" of the idea to reorganize. I don't know when this meeting occurred.

¹⁷ "Report of Biology Review Committee," written in 1980 or early spring 1981, but not formally signed until August 4, 1981.

Vice Chancellor Roderic Park

Hughes: What was Park's background?

Taylor: He was a botanist, so he understood biology, understood the issues and his colleagues. He had been dean of the College of Letters and Science, and then, when Mike Heyman was made chancellor in '80, Rod became the vice chancellor under Heyman. When the provosts, vice chancellor, and chancellor were talking about Berkeley's worst academic problem (as I understand it), Rod Park was provost of Letters and Science, not vice chancellor. Heyman was vice chancellor.

First External Review Committee Report, April 1981

Taylor: I believe that they suggested--whoever "they" are: Dan, certainly; Alex, probably--that you couldn't just stand on this internal review committee report alone. People could criticize it. It needed a stamp of approval from some outside agency. And they recommended that the stars in biology across the nation--eight or nine people--form the committee.

Hughes: Do you remember any names?

Taylor: They are all listed in the external review committee report. Peter Raven from Missouri Botanical Garden was one, so he represented the plant biology field. Just outstanding people, highly regarded. Fortunately, they all were willing to serve on the external review committee.

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Taylor: --and then they wrote their report. That, too, was dated in April 1981, I believe.

The Chancellor's Advisory Council on Biology [CACB]

Formation

Taylor: The external review committee verified the findings of the internal review committee and they recommended that a council, or umbrella organization, be formed immediately to bridge the College of Letters and Science and the College of Natural Resources, and that a College of Biology be formed from the L&S biologists and the CNR biologists. I believe that the idea of a College of Biology that would encompass the College of Natural Resources was threatening to the College of Natural Resources, and it was threatening to the College of L&S, or at least to the dean's office. Therefore, the college idea was quickly dropped, but

the chancellor's advisory council was established, initially with representatives from L&S, CNR, and Chemistry. The deans were also included, ex officio, Beth Burnside (L&S, Biology) and David Schlegal (CNR).

The CACB was intended to form the umbrella between all aspects of biology at Berkeley. Its membership was chosen from senior faculty who could represent broad fields of knowledge and who were well respected in their own field. Department chairs were not allowed to serve because of the failure of an earlier biology council of department chairs who had argued only for their own department's special interests. Deans are invited to attend meetings, respond to questions, and convey information, but they do not enter into the decision making process and do not participate on subcommittees of CACB.

Hughes: Who made those determinations?

Taylor: The external review committee made the general recommendation, and then Rod Park developed the charge to the chancellor's advisory council and made the first appointments, probably with advice from some of the Biology leadership.

Hughes: Who, Park or the council?

Taylor: Well, actually, I think it was Heyman who made the appointments because it's the chancellor's advisory council, and it was always important to Dan and Alex that they be able to speak directly to the chancellor.

Faculty Resistance to CACB

Taylor: Unfortunately, this organization was outside the norm. It was neither a committee of the Academic Senate, carrying with it the rules and regulations and checks and balances of the senate, nor was it an administrative committee. Although it was appointed by the administration, its membership was wholly faculty. And it was not a creation or arm of the dean's office, nor of a department. Therefore, some worried that it would become politicized, it would have no checks and balances, and it would get its way on everything because only the chancellor had authority over it.

The first draft it developed had been done in complete secrecy and was devastating to parts of the Biology community. Had that draft become the final plan, it would have proved that those who opposed CACB were right. However, wisdom prevailed and the first draft was followed by three others.

Hughes: The resistance was coming from the faculty?

Taylor: Yes, the faculty resisted it.

But at any rate, Rod Park and the chancellor stuck to it--that the CACB was necessary, an umbrella organization over the colleges was necessary until the campus decided whether or not to have a College of Biology.

Dan was appointed the first chair of the CACB. He served until '88 or '89, when Bob Tjian then became the chair, and he's chair now. Maybe Dan served into the nineties. When did Dan retire from *Science* magazine?

Hughes: Nineteen ninety-five.

Taylor: Okay, then Dan stepped down from CACB in Spring '93, and that's when Tj went on as chair.

Faculty Recruitment Policy

Taylor: The chancellor's advisory council started meeting in about '82. They were charged with all aspects of biology, but their attention was to be paid particularly to curriculum and areas of recruitment. CACB did a very radical thing. They insisted that whenever there was a faculty recruitment in biology on campus, the search committee would be composed of some faculty from the recruiting department and some faculty regardless of departmental affiliation who were knowledgeable in the field being searched. CACB was only advisory to the deans, but their recommendations were followed. This has led to much broader knowledge on the part of faculty about other departments and to more joint searches and joint appointments among two or more departments. Departments appear today to be more cooperative with one another and less insular with respect to appointments. At the time, however, there were a lot of departments that didn't like the idea.

Hughes: I can imagine.

Taylor: Nonetheless, it has worked very well, and it has brought a lot of good talent here. It has brought a much better understanding of the interdisciplinary nature of biology.

So over time, this has worked very nicely because it has become a group of biologists coming together to say, this is the best person that we recommend for this position. There has been a lot of cooperation and the diverse perspectives on a search committee have served the campus well.

Hughes: Members of the search committee were chosen because of their interest in the discipline of the person being recruited?

Taylor: Oh, definitely. Interest and knowledge. Further, CACB's influence has resulted in departments not replacing separations in kind. Instead, they consider where the field is going and where the campus should build strength. The chancellor's advisory council's advice is also sought on departments' recruitment plans and areas in which they wish to build strength.

Membership

- Hughes: Originally, according to Trow, the advisory council was perceived as being slanted toward molecular biology.
- Taylor: Oh, yes.
- Hughes: And then a population biologist and an ecologist were appointed.
- Taylor: It was perceived that way. In part, I think it was because of the advisory council's first draft plan which favored molecular biology and because many of their early members were molecular biologists. Also, as I understand it, molecular biology was considered the hot field during that period. So it was natural to expect molecular biology to be favored. However, I think bringing in so many other non-molecular biology faculty to participate on subcommittees that developed subsequent drafts, and the changes among the draft plans that emphasized the importance of other areas, helped to still many of these early concerns.

During the early formation of CACB, they also worked on curriculum and space. They advised Vice Chancellor Park to some extent on capital construction processes, particularly as these related to faculty involvement. Their advice was sound and certainly resulted in improvements to the new buildings.

State of California Building Funds

- Hughes: Would you speak a bit about the building program?
- Taylor: In 1985, following at least five years of severe drought, the sky suddenly opened up, and the university received state support to start construction on a few buildings, including the Life Sciences Addition. When that happened, unlike other campuses, Berkeley was prepared. The biologists had been planning for this, working on it for three or four years without any expectation of receiving needed funds. When the internal review committee, external review committee, and the building committee started in the early eighties, the entire university budget for capital programs was \$5 million for all nine campuses. There was no money going to Berkeley for new programs.

In the late seventies and the early eighties, all new construction money went to the new campuses: Santa Cruz, Santa Barbara, Irvine, San Diego and the growing campuses: Davis, Riverside, and to some extent UCLA. In 1982-83, Heyman convinced the president and Bill Baker, with Rod Park's and the faculty's support, that Systemwide had neglected Berkeley. I can remember pulling out data showing Berkeley had not received any kind of state support for construction since the sixties. Berkeley was starting to fall apart; it was time that we got some help.

- Hughes: Plus molecular science was exploding.

Taylor: Yes, exactly. But they [in the President's Office] didn't care about that because all other campuses were saying that their disciplines were exploding, too, and needed the help.

The First CACB Reorganization Plan

Dissension between Molecular and Holistic Biologists

Taylor: At any rate, the CACB said, "My God, it looks like we're going to have a new building. It looks like we are going to have an opportunity to reorganize. We don't have to keep the same organization. We better come up with a plan."

Beth Burnside, who was dean at that point in time--

Hughes: Dean of Letters and Science Biology?

Taylor: Yes, L&S Biology. CACB worked in secret because they didn't want to have anybody telling them and pulling them politically. They wanted to come up with what they thought was the very best plan for the discipline for Berkeley. Well, unfortunately, they weren't real sensitive to all the different views on campus. The report actually said that there are the high-tech and the low-tech biologists here, and the high-tech are the molecular biologists. This is where biology is; this is where biology is going. And then there's the low-tech science over there in zoology, paleontology, botany, places like that.

Well, Rod Park had come from Botany. He was a biologist, and he understood, as I recall him describing it to me, that the molecular biologists would develop these techniques and all, but eventually if it was going to work, it had to be applied to the whole organism. So the low techs would be needed in the future.

The quote, "low-techs" were beginning to see the need for modern biology in the work that they were doing, and they were beginning to try and catch up. And you know what's going on now with paleontology and everything else in terms of DNA and RNA and all the various exciting things they're able to do.

Their proposed plan came out in December of '84. It was highly slanted to the molecular side, and that's where Martin gets his notion that CACB was initially slanted towards molecular biology. And it may be because CACB was primarily on that side of the fence; it may be that those people truly believed that this is the direction biology was headed in. I don't know because I wasn't involved in those conversations.

At any rate, Rod received the report, and I drafted a letter to circulate it to all the faculty. Every faculty member got their own individual copy. And because Rod and Dan knew it was going to be an explosive thing--CACB had proposed, in that first iteration, that

people go to new departments, as I recall; they certainly described what was going to happen to the existing departments [i.e. dissolution].

Seeking Faculty Input

Taylor: They sent it out and advised that they were going to hold four faculty meetings up at the Faculty Club four Tuesdays in a row in January and February--and that they would be town meetings where everybody could come and express their opinion. The first meeting was held in the Great Hall. It was very heavily attended. That hall was filled. People were yelling at one another, and they were yelling at Dan, and they were yelling at Rod. [demonstrating] "Just calm down. I'll respond." You know, this kind of thing.

I remember Dan saying a number of times how pleased he was with Rod's behavior and his actions at that time and how supportive he was. And they [Rod and Dan] kept saying, "This is only a draft. You all will have a chance to comment on it." But the faculty didn't believe them. The organismal biologists felt that they had been so poorly treated, they were just positively insulted by the whole thing, so there were very hard feelings.

The second meeting was held up in the library of the Faculty Club. Forty, fifty people crowded in there. And they had different tables taking up different topics, and people were loud or excited in making their comments and pounding the table and all that. And then they had two more meetings that I vaguely remember. There were fewer and fewer people attending each one.

Hughes: So people went back to their own business?

Taylor: No, they finally, I guess, began to recognize that everybody wanted to hear what their comments were, that they would be listened to, that this was a draft. And so the emotions calmed down, for whatever reason.

Hughes: Was some of that due to the way in which Rod Park and Dan Koshland and the other people associated with the CACB were handling themselves?

Taylor: I think some people felt that Dan had his favorites that he was trying to help, certainly among the biochemistry group.

I think that Rod really tried to be extremely fair and was cautious at times to make sure he was being fair. He recognized in his role as vice chancellor that he was responsible for the whole campus, not just his biology colleagues. But I don't think he's been given enough credit for that.

Hughes: Did nonscientists show up at the meetings?

Taylor: No. It was just Berkeley biology faculty.

Following the four town hall meetings, Rod wrote back to the advisory council and said, "Thank you for your first draft. Here are the comments I received." I had to write little letters from Rod, "Thank you for your comment. It'll be taken seriously," and send it back to these people. We had 185 letters or so, as I recall. I remember making confidential lists of all faculty names, whether they were for or against [reorganization], what their issue was, and all of that.

A CACB Subcommittee to Reorganize the Biology Faculty

Taylor: Rod said to the CACB, "You've got to go back and do it again. And this time you're going to have a more open process. You've got to listen to the variety of views on the campus." And so they appointed new members as a subcommittee to the Chancellor's Advisory Council on Biology,¹⁸ and that's when I was brought in. I staffed that subcommittee.

Bob Zuker was the chair of that subcommittee (second iteration), as I recall, because I worked very closely with him. Beth Burnside participated as dean and probably as liaison with CACB's regular body. But nobody else from CACB was on the subcommittee that I can recall; they left it up to their subcommittee.

We had these huge boards, and we had recipe cards. Each faculty member was named on a card. They pinned all the cards up on a board; the CACB sub-committee would then consider an organization scheme, and they'd arrange the name cards according to that scheme. They would consider that scheme and ask: "Well, what do we do with those three over there?" Or, "This organization doesn't work." Then they'd think of something else based on the comments received on the first iteration, and start moving the cards around into a different organization. It was very effective.

Hughes: The general system of organization was according to what type of science people were doing?

Taylor: Yes.

Hughes: Rather than what their departmental affiliations had been.

Taylor: Right.

Hughes: I gather that was the point.

Taylor: That's right.

¹⁸ The second iteration sub-committee was appointed April 10, 1985, chaired by Robert Zucker, and included Stephen Beckendorf, Jeremy Thorner, Wayne Sousa, Milton Schroth, Richard Malkin, David Wood, and Deans Beth Burnside and Albert Weinhold, and reported July 15, 1985.

Hughes: As you said, there were people in engineering who were doing biology. They should have access to people in biology departments.

Taylor: And to the equipment, right, and to a group of colleagues who were working on the same kind of research.

Berkeley Creates a New Model for Biology

Hughes: What were the models for reorganization? Surely, Berkeley wasn't totally creating this reorganization plan de novo.

Taylor: Oh, yes, I think it was, for the most part. I think it was. Dan can tell you more about that. I remember that there were discussions about how some of the eastern schools seemed to be well organized in one area or another. So yes, I guess there were models, but there was certainly no exact model. Berkeley became the model for the rest of the country. Dan and I both received phone calls--and Beth, too--for years. In fact, I had a phone call from UC Davis last year, before I retired. I went down to UC Riverside two or three years ago. They were talking about reorganizing biology down there. Berkeley has been the model for reorganization of biology in many places throughout the United States.

UCSF

Hughes: Your near neighbor, UCSF, on a smaller scale underwent a reorganization of basic science which began in 1970 in the School of Medicine.¹⁹ UCSF claims that departmental walls crumbled and a much more interdisciplinary, cooperative approach was established.

Taylor: I wonder if it's that way today. That's not what I hear about UCSF. But I don't know UCSF very well. I know that Dan knew a lot of people who were over there; a lot of people there are his good friends. So I think Dan can respond as to whether or not there were models or whether, as a result of talking to so many people across the country about the facilities and developing the internal review committee report, they began to get their own models in their mind.

I think the original four senior faculty leaders [Koshland, Glazer, Wake and Schroth] did have some ideas, actually, because in 1981, there was a preliminary grouping of faculty for purposes of determining the size of buildings: how many people are you likely to have in

¹⁹ See the oral history with William J. Rutter, Ph.D. *The Department of Biochemistry and the Molecular Approach to Biomedicine at the University of California, San Francisco*, an oral history by Sally Smith Hughes, PhD, recorded in 1992, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1998.

one research area or another? I never compared the list with the final plan, but I have a sense that the final result was quite different. After all, science changed over the years, faculty retired, and new faculty were hired

A Steady State in Numbers of Biology Faculty Members

- Hughes: What did the subcommittee do about future faculty recruitment and growth?
- Taylor: For the planning of a building?
- Hughes: Yes. It was fine to say, "We have X number of population biologists now," but maybe that field was eventually going to expand.
- Taylor: Well, recall, the [new faculty] searches are not replacements in kind anymore.
- Hughes: But you have to think many years ahead when you're planning a building.
- Taylor: Berkeley enrollment was in steady state by 1981, which meant that we were getting no more faculty. So it had to be assumed that biology wasn't going to get a higher proportion of the faculty at Berkeley but that they, too, were in steady state at the 225 or 235 or 245 faculty level they had. Any planning for changes in size of these subgroups would have to come in terms of flexibility in the building.
- Hughes: But not an increase in overall number of biology faculty.
- Taylor: Exactly.

Designing the Science Buildings for Flexibility

- Taylor: All of the buildings, except LSB--because it was a renovation instead of a new construction--were designed to be flexible. LSA is built in ten-foot modules, and some disciplines have two or three ten-foot modules put together into a twenty-foot or a thirty-foot lab. On the first floor are the neuroscientists, and they liked small, specialized labs, dark rooms and instrument rooms, at least at the time the building was built. So they all had ten-foot modules on the first floor.

The second floor got a little bit more open space, but it was constructed in such a way with its utilities so that everything could be dropped in through the ceilings in these ten-foot intervals. Whenever renovation work is done in the building, it's done in such a way that there won't be any damage to the flexibility of the building or the systems. So that's how we would deal with changes, as some components decreased in size and others increased.

Hughes: Koshland Hall was built on this plan?

Taylor: Yes.

The Second & Third Reorganization Plans

Taylor: We were talking about the academic program process, the first iteration and the second iteration. The second iteration became more of a political document because the subcommittee was instructed to consider seriously the comments received on the first draft, and to talk to colleagues. One could see the process as moving like a pendulum. The first iteration was way over here [demonstrating], with the so-called “high-techs” and the “low-techs”. The second iteration was way over there [demonstrating], with all the politics taken into account and everybody beating on the members of the subcommittee for special consideration.

The second iteration came out as a draft, and Rod Park sent it out to all the faculty, asking for comments again. We got--I don't remember exactly, but something on the order of 125 comments. “This is wrong, that's wrong, you've swung too far, you're not considering this”--all of that kind of thing.

Hughes: Did people look after their own turf, in general, or did they really try to look at the reorganization process holistically?

Taylor: It was a mixture, definitely a mixture. One person who was never satisfied--has been in depression ever since, I think--felt that his department should not have been changed, that it was really important not to change it, and that when it changed, it lost it for him in terms of where he was headed and what he could do. He expressed deep feeling about the academic program. It wasn't, “Oh, boy, I can't adjust, and so we're going to argue for this.” He felt that a serious mistake had been made academically.

I've come to develop great respect for the biologists at Berkeley. I just think they're fantastic. Because of the experience of going through all of this, they continue to plan for themselves. They're the only discipline that I see planning five to ten years out and doing new, innovative, and exciting things that the campus has never done before. They're always throwing out something to the campus. “How can we do that?” As I saw my role, that's where I was supposed to step in and try to make it easier for them, to get rid of the bureaucracy and the barriers that hindered their progress.

Generally it was felt the second iteration of the reorganization plan went a bit too far in the opposite direction. I think it was a subcommittee of the CACB. The subcommittee was to take all of these comments into account from both iterations and draft a third iteration. And they did. That was distributed. We got comments from about sixty faculty. A lot of people, many in the molecular area, thought that the innovative thinking of the earlier plan had been lost in trying to please too many people and that we had lost sight of the original goal.

The External Review Committee Reconvenes, 1986

Taylor: So at that point, the advisory council and Rod Park felt that they had taken reorganization as far as they could. They couldn't do anything further, and it was time to call back the original external review committee. So we invited them back. I think it was 1987. I'm kind of hazy on dates.²⁰

I think seven of the nine or eight of the nine original members were most happy to come back. One or two didn't come back only because they had longstanding conflicts that they couldn't break; otherwise, they would have been happy to be here, too. I think bringing back that original group gave some validity to the reorganization process. They met with the faculty and the administration and the deans and all faculty groups who requested to meet with them. Then they wrote their response to what we had done. They basically said that we had gone too far, that we had lost some of the innovative thinking that had been the original intent, and made some suggestions along those lines.

The Final Reorganization Plan

Taylor: The fourth and final draft was developed by CACB as a result of the External Review Committee's comments.²¹ It again went out to all faculty, and we asked for comments back. I think we got twelve comments. Two of them I could settle, as I recall, by making a few phone calls, talking to [the writers], talking to others, and I was able to work out their issues in a satisfactory manner for them. For the remaining ten, we set up a judicial committee. Now, remember that in most if not all of the iterations--certainly in the later iterations, two, three, and four--we said, "Here's the Life Sciences Addition. These are the people that will be in this building. This will be the configuration of the department." And we went through that, assigning people to buildings and to departments via a matrix, in the later drafts. Departments weren't necessarily aligned with buildings. There might be only one person from a department assigned to a group of ten from another department. They were assigned according to research interests.

²⁰ The second external report, titled "Biology at U.C. Berkeley, Report of the External Review Committee," is dated February, 26-28, 1986.

²¹ "Final Report of the Chancellor's Advisory Council on Biology, Reorganization of the Life Sciences," August 1986.

The ten or so people whose cases were considered by the judicial committee²² were unhappy because of either the reorganization of their department, the department that they were assigned to, or the building to which they were assigned. People had been commenting about these kinds of issues all along, but these ten were truly unhappy and had not been satisfied by the various revisions. A judicial committee was appointed. One person, who was well respected and had never served on any of the committees was appointed to serve as chair of the judicial committee. Such a committee had been planned early on and he had been saved for this function because he was viewed by his colleagues as eminently fair. Other members were also senior faculty members, knowledgeable, well respected and had not been involved in any of the reorganization processes.

The judicial committee asked each person to write out their reasons for disagreeing with the report. Then each person was individually interviewed by the committee and they [the committee and the individual] discussed various options. The committee members talked to department chairs and in a timely fashion resolved to the satisfaction of the complainant in all but one case--the case I told you about earlier where the person felt irreparable damage was being done to his discipline.

I should also mention that CACB was determined early on that departments would not be structured nor buildings aligned into groups of "haves" and "have nots", or stars and those thought less qualified. Members worked hard to gain acceptance for this principle.

The Academic Senate

[Interview 2: January 12, 1999]

Taylor: The Advisory Council on Biology met in secret and wrote the first plan for the reorganization of biology. It's dated December 3, 1984. Rod Park sent it to the Academic Senate committees to comment on if they cared to. Rod wrote to them regularly. Each new draft was sent to the senate leadership and to three or four relevant [senate] committees-- Educational Policy, Graduate Council, Budget Committee, etc. Some committees responded to the earlier drafts and their comments were sent to CACB along with other faculty comments. However, you should realize that senate committee membership changes every year or two, and in the eighties there was no organization or structure to develop historical files or maintain "institutional memory." Thus, each iteration sent to the senate was like a totally new plan they were seeing for the first time. Consequently, when the final plan came out, the new leadership of the senate [Ed Epstein] and the senate policy committee [Carol D'Onofrio] objected strenuously to the lack of senate involvement. They criticized Rod

²² Appointed September 4, 1986, the membership included R. David Cole-- Biochemistry (Chair), John Gerhard--Molecular Biology, Sandy Elberg--Biomedical and Environmental Health Sciences and Dean of the Graduate Division, Emeritus, Don Erman--Forestry, Deane Furman--Professor Emeritus Entomology & Parasitology, William Lidicker--Zoology.

unfairly for not sending early drafts to the senate for comment and for sending a final report to which they felt it was too late to respond--a fait accompli.

Hughes: So the senate didn't take an active role in the reorganization of biology?

Taylor: They didn't take an active role at all.

Hughes: But they were asked--

Taylor: They were invited to, yes.

Hughes: I was looking at a 1987 memo from the chairman of the senate policy committee--

Taylor: Yes, Carol D'Onofrio.

Hughes: Right. She complained about the senate not being included in discussions about the reorganization of biology.²³

Taylor: Yes. She's a good friend of mine. However, I could never get her to understand. As I recall, the issue was finally resolved when Beth Burnside, several other members of CACB, and I met with John Heilbron [who was chair or vice chair of the senate; he later became vice chancellor] and reviewed the process. John was a great peace maker and the senate finally gave its approval in 1988 so we could move forward to the President's Office, the systemwide senate, and others for needed approvals.

More on the Reorganization Plans

First Draft, December 1984

Taylor: [Taylor consults a large binder of documents concerning the reorganization of biology in the 1980s.] All right, so the first reorganization plan of December 1984 from the Chancellor's Biology Advisory Council is here, and you'll see that Dean Beth Burnside was on it. So here is the document that Rod sent out to the biology faculty. He said [in a cover letter], "I'm enclosing the plan and invite you to attend four open meetings in January and February at the Faculty Club." And I described those meetings to you.

Hughes: Yes, and they declined in faculty attendance as time went on.

Taylor: Right. And then here is a letter from the Academic Senate Committee on Educational Policy that reviewed that first draft. But it was only a draft.

²³ Carol N. D'Onofrio, Chair, Committee on Senate Policy, to Roderic B. Park, Vice Chancellor, September 12, 1987.

Second Draft, July 1985

Taylor: Then July 15, 1985, the second iteration on reorganization. I told you that we formed a special CACB subcommittee, and that's when I came in and started working with the advisory council. So here's their report, with copies to the senate, and in each case, we detail what the departments should be, who will be in them, and where the faculty will reside: buildings across the top, academic departments, and faculty names down the side. And then this organization just keeps changing. If you go down to Loris Davanzo's office in University Hall, you should find copies of the letters that people wrote, objecting to these various iterations.

Third Draft, November 1985

Taylor: November 25, 1985. Again, "Enclosed is a copy of "Modifications to the Second Iteration..." So that's the third round and again copies are sent to the senate. Then, in February of 1986 we invited the original members of the external review committee to return. They were wonderful. They came back, and talked to a large number of people, and then Rod received the external review committee's report and sent that out to the campus community to comment on in May 1986, with copies to the senate.

Final Plan, August 1986

Taylor: We, that is the CACB, took the third iteration of the reorganization plan as far as they could; they got faculty and the external review committee's comments on it, and then came up with a final plan, which is dated August 1986. That is the final recommendation of the advisory council, dated August 28 of 1986. Again, Rod sent this plan out to the biology community and to the campus. He says, "As was decided in the initial process, a Judicial Committee has been appointed for a temporary period to adjudicate objections arising from this final report (a) of an individual about his/her assignment to an academic unit or building, or (b) by a unit about assignment of a particular individual to that unit."²⁴ No units objected, only about twelve individuals objected, as I described earlier.

²⁴ Roderic B. Park, the Vice Chancellor, to Berkeley Biology Faculty, September 5, 1986.

Few Objections

Taylor: We had by that point told the departments--and I'm using the global "we"--the vice chancellor, the chancellor's advisory council, and particularly Dan--I think he was just excellent; it was Dan's idea--that no unit was going to get all of the cream of the crop, and no unit was going to be a dumping ground but that all units would share the cream of the crop and the "dregs" among the faculty (I'm sure there were few if any "dregs.") So we had no objections from departments. But when we formed the judicial committee, we did have eleven or twelve objections, as I recall, a decline from the humongous number of people objecting to the first draft.

Hughes: Were they objecting to their own departmental placement?

Taylor: Yes. As it says here, "...objections of an individual to his or her assignment to an academic unit or to a building." We got one person who objected to the assignment to a building because the person thought that there would be few windows, if any, in the lab, and this person did not want to be housed in such a terrible environment.

Hughes: And so it went.

Taylor: And so it went. So by September 6, 1986 or shortly thereafter, the final report was concluded. The reorganization planning was concluded. The judicial committee was appointed. They worked for about a month and a half, and they resolved all of the personal issues. There was only one person who said, "I will never be happy. You could do anything I ask you to do and, under the circumstances, I can't be happy. My department shouldn't have been folded up. Because it did, I think my research, my academic interest is ruined." There was nothing we could do about that at that point.

Hughes: What became of him?

Taylor: He accepted his assignment to a new department and went with grace. It was sad because he felt very strongly about his particular choice of research areas and interest.

Implementing Reorganization

Taylor: With the planning completed, we started the general campus review. And what you have in this binder are the various comments from the reviews by the senate, some of the comments about moving in and out of buildings, some arguments with the senate, and finally approval.

The sheet, "Relocation of Faculty From Current Departments Into Reorganized Departments," November 20, 1987, is particularly useful. From it, you can see that we originally had eight departments in Letters and Science, nine departments in the College of Natural Resources (only two of which became involved in the reorganization), plus the Instruction in Biology unit, which was not an academic department, involved in

reorganization. A graduate group in neurobiology was also involved to some extent, and as a result of the reorganization there were eight or ten graduate groups in biology that were eliminated.

Molecular and cell biology, which was a big department of about ninety faculty at that time--there are fewer now following budget cuts and early retirements--decided to form divisions. They originally formed six divisions; now they have five. They did away with the biophysics division several years after the reorganization and reshaped themselves. The advantage of having a division within a department is that they can reform themselves any time they want to without external review, giving them much greater flexibility. Academic flexibility in terms of the programs and also in the buildings was a big thrust of the whole plan, in order to take advantage of future needs and changes.

Hughes: Were there any other problems with the senate reviews?

Taylor: Only with respect to the senate saying, "We haven't been involved. We weren't consulted," which I showed you was not the case. But we listened to an awful lot of bickering from the people who were active in the senate during that time period.

Hughes: Berkeley has the reputation of having a very powerful Academic Senate. Is this a reflection?

Taylor: Certainly. And it's a reflection of the personalities of the individuals who were active in the senate for those two years. They wanted to have a much more active involvement in reorganization. I don't believe that people outside the department can determine what should be happening, how a department should reorganize itself. That's why I don't think academic planning should be done from top down but from the bottom up.

More on the Chancellor's Advisory Council on Biology

Sources of Power

Hughes: Yet, my understanding is that that's what the Chancellor's Advisory Biology Council was doing. No?

Taylor: No. The CACB, deans, department chairs, and individual faculty all made recommendations to the chancellor and the vice chancellor on various aspects of the reorganization. I think we had more than thirty committees and subcommittees dealing with different aspects of the reorganization--curriculum, space policies, building construction and programmatic needs, graduate groups, staff personnel and department functions, budget reorganization, etc.²⁵ The center of the decision-making process rested with the vice chancellor who listened carefully to Dan Koshland, Alex Glazer, David Wake, Beth Burnside, and other senior faculty.

²⁵ See the reference binder, which lists a number of committees, their charge and their memberships.

- Hughes: Trow made the point that the two sources of power of the advisory council were that it was looking at biology across the board, not from the departmental vision. But he also made the point that any, quote, "advice" that the council gave came with a lot of weight.
- Taylor: Oh, indeed it did.
- Hughes: In other words, I think he was implying that departmental chairs could be overridden.
- Taylor: Yes, and that has created some problems over the years, in that department chairs and others have objected to certain faculty having the ear of the administration. However, Rod always tried to consult with the chairs or deans when a new idea was brought to him outside normal channels. As special assistant to the vice chancellor, Rod expected me to consult broadly and bring back consensus on the pros and cons, if he didn't deal with a particular matter personally.

A Preceding Biology Council

- Hughes: Does some of this relate to the preceding biology council you touched on earlier?
- Taylor: Yes. The College of Letters and Science set its disciplinary councils probably five or ten years before the chancellor's advisory council. They had a physical sciences council, a biology council, a humanities council, and a social science council. Department chairs were members of those councils. The biology council included the eight department chairs that were originally in L&S. The department chairs were expected to speak for their department. You'd never find a department chair allowing a department to be disestablished during his or her reign, because that would be viewed as a failure on their part or as a dishonor.

Department chairs had their own defined roles and therefore couldn't be farsighted or think for the good of biology as a whole, only the good of their department. Consequently, the biology council accomplished little. They had made some attempts at reorganization, but everybody ended up defending their own turf and nothing happened.

- Hughes: This problem began in the mid-seventies?
- Taylor: Yes, or the late seventies. So by forming the Chancellor's Advisory Council on Biology, they said a department chair could not be a member of the advisory council because members are supposed to be thinking of the good of biology as a whole.
- Hughes: Not along departmental lines.
- Taylor: Right. Thinking along departmental lines had been tried and failed under the biology council.

Rising Above Departmental Lines

Hughes: Was it your impression that the advisory council indeed did generally look at the welfare of biology as a whole?

Taylor: Yes.

Hughes: For people who had been brought up in the traditional academic system, power lay in the departmental structure.

Taylor: Yes, that's right.

Hughes: And yet they were able to rise above it?

Taylor: But remember, I told you that I thought one of the important reasons for that resulted from the internal review committee. A senior member of the faculty was appointed to each of the four subcommittees. Each was well respected, and didn't appear to his colleagues to be out for his own personal gain or for that of his department, but for the good of the discipline as a whole.

Hughes: Right. And, of course, there was indirect support in the various reviews that showed that biology at Berkeley was slipping.

Taylor: Exactly.

Hughes: Presumably, the biology faculty wanted to do something about that.

Taylor: Absolutely.

Criticism of the CACB

Taylor: Martin found and you can today find critics of the reorganization, and of the people who were involved in it, and critics of Dan. But they succeeded. Basically, they succeeded.

Hughes: Do those critics voice the same criticism?

Taylor: Some do. For example, one of the early leaders always objected to the Chancellor's Advisory Council on Biology because he saw it as being neither fish nor fowl. It didn't represent the department, and it didn't represent the Academic Senate.

Some people who probably didn't have much influence in biology were critical of the senior leadership, claiming they were power hungry. I wouldn't say that they were. They didn't create anything for their own personal gain, and they all eventually gave up their roles, returned to the faculty, and were led by others. I never saw them choose the best space for

themselves or not follow policies they had helped establish. I think they were quite visionary; they loved Berkeley and biology and they wanted to help improve it. Most of the leaders, and there were many, not least of whom were the handful most often mentioned, did sacrifice--their own research, a Guggenheim, sabbaticals, family time, etc.

Expansion of CACB Membership

Hughes: Trow also says that the original seven-member council was largely molecularly oriented.

Taylor: Right.

Hughes: An evolutionary biologist and an ecologist were later asked to join because there was dissent on that point.

Taylor: Right. And you'll see it when you read this December '84 report. The 1984 members included Lew Feldman, who was in botany and Jim Patton from zoology.

Hughes: Now, this was after the council had been enlarged?

Taylor: No.²⁶ They were part of the original nine people who wrote the first report; I don't know who the seven were Martin refers to. The original report basically said everything important to biology is in molecular biology. Remember, I think I used the reference "high techs" and "low techs"?

Hughes: Yes.

Taylor: That so angered a lot of people, not just those outside of molecular biology.

CACB Advice on Faculty Recruitment

Hughes: Once the advisory council--and Dan chaired it throughout this period--came to its final plan for reorganization, then Dan had the committee turn to new topics, such as the curriculum, establishing policies for reviewing departments' requests for new hires, trying to get the principle across that a retiree will not be replaced in his or her field necessarily, or even in

²⁶ The seven original members were appointed in July 1981; James Patton, zoology and William Libby, forestry/genetics were added in February 1982 for a total of nine members; Lew Feldman, botany was appointed in July 1984 when a biochemist, chemist and microbiologist stepped down. Elizabeth Blackburn, molecular biology, was also added. At that point there were nine members. The chemist was not replaced until July 1985.

the same department. Replacements would occur where the best arguments for the future could be made.

CACB also became very active in giving advice to the deans about the departments' requests for search committee members, the wording and sometimes placement of advertisements, and future areas of recruitment. They also began to suggest more joint appointments. As the campus reviewed departmental requests for new FTEs [Full-Time Equivalent faculty positions], the campus administration, in addition to the deans, started looking to the advisory council for advice. And budget committee, which is a very distinguished body of the Academic Senate that comments and reviews on new FTE positions, got to the point of saying, "We can't consider this request because we don't have comments from the Chancellor's Advisory Council." CACB has become pretty well a routine part of our process.

Taylor: Dan certainly played a significant role in suggesting important areas for CACB's study or review. For example, he felt it was necessary to look at graduate groups. There was a tremendously large proportion of graduate groups on campus that were biology-related. I think that that probably speaks to where the discipline was going because graduate groups are interdisciplinary; they aren't bound by departmental lines; they have faculty members from a variety of departments. [interruption] I think reviewing graduate groups was a natural next step following the reorganization, and, as I recall, a large number were disestablished at the initiation of the chancellor's advisory council.

CACB Develops Space Policies

Taylor: The CACB also looked at space, since we had new buildings that were being planned and constructed. Although the reports had proposed who would go into what building, the campus didn't have space policies per se, and so the advisory council developed some space policies. Using the modular sizes of the new construction in LSA [Life Sciences Addition] and Koshland Hall, they came up with a policy that each senior faculty member will have 1600 assignable square feet. That would include office space, space for their graduate students within their lab, and their own laboratory space, including special needs.

Assistant professors who were viewed as just establishing their research programs should have only about 1200 ASF [assignable square feet], while associate professors and those who were beginning to really develop their research should get 1400 ASF. Faculty with diminishing research needs could expect to have less space and give up space over time. In addition, there should be about 400 ASF of shared support space that the group could use - -common facilities. That was a whole new idea for biology. Historically, everybody had had their own glass-washing facility, their own freezer, etc. CACB reviewed current assignments and made recommendations on [facility] changes as faculty moved into the new buildings.

Hughes: The move toward shared facilities obviously was a cost-cutting measure but was it also intended to facilitate--

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Hughes: --communication amongst laboratory groups?

Taylor: Absolutely. That certainly was a goal. I think that the cost of doing biology had become terribly expensive for individuals, too. There was certainly significant interest in cost-saving and in reducing duplicate equipment that required support from an individual's contracts and grants. This allowed more money for research. But sharing equipment was not imposed so much by the university as it was self-imposed by the biologists.

Building Generic Laboratories

Taylor: Another thing that was accomplished, probably coming more from the administration but very much supported by Dan, was the idea of building generic labs. We did not build labs for individuals, which can be quite costly. We were planning for the future. We didn't know when construction would be funded, and by the time the buildings were occupied, many of the people that were doing the planning could have left. They could retire or move on to another university.

So we built generic labs and set aside some funds, a very small amount, a couple of thousand dollars apiece, to retrofit the labs to an individual's requirements. Remember, I told you that they built the Life Sciences Addition first floor with a lot of little ten-by-twenty foot modules, and then the second floor was a little bit more open, and as you went up in the building modules became more open. Each faculty group had representatives designing the space to the requirements of the group (e.g. neuroscientists or cell and developmental biologists). Representatives would meet with their groups and come to a consensus about the space configurations that best suited their needs and about the kind of support space they would share. One group might have light-tight modules within individual labs, where another group would opt for a dark room in the shared space.

Some floors might need a lot of cold rooms and freezers, where other floors wouldn't need many and might need more imaging facilities. Individual needs that others in the group did not need were handled from the two thousand dollars or so that we set aside for individual retrofit. However, the lab remained basically generic. Dan had a lot to do with gaining approval for these policies.

Dan also suggested that the university hire a person who would do the retrofit work in faculty labs, rather than have the physical plant people do it. So we started something new that Dan initiated: Rod Park authorized the hiring of a person, Roy Pickrell, who established the Academic Facilities Office. AFO met with faculty to discuss their specific needs and developed faculty-approved plans and cost estimates. Each lab had about \$2000-3000 to spend; extensive renovation above this amount had to be paid by the faculty member. Most renovations were completed for less than the allotted amount, and savings paid for department or building extras. Roy did an excellent job for the faculty and the university,

saving us millions of dollars over the ten plus years it took to complete the construction of two new buildings and renovation of LSB, now Valley Life Sciences Building.

Hughes: The AFO was a short-term appointment?

Taylor: It was originally intended to be. But as the length of the building time spread out, it became longer and longer. It has since been established as a permanent part of the campus because it has saved us so much money. We would go to the physical plant people and ask how much to convert a lab. Beth Burnside had one example of over \$800,000 for someone that they were bringing into Stanley Hall. Roy Pickrell brought his crew in and estimated \$400,000 and did the project for \$400,000. Had we done it through the campus, they would have estimated \$800,000, had cost overruns, been late, and it probably would have cost us about \$900,000 or \$1 million. That was our experience with the physical plant and Planning, Design & Construction folks, and that's the experience that Dan has had with them. Those are some of the things that I can recall that Dan did through the advisory council.

Dan also suggested that when a building was near completion, the faculty walk through it to develop a "punch list." After all, they had done a lot of planning, and they would discover rather quickly mistakes made during construction that the architects and engineers weren't necessarily looking for. If we discovered these before the university accepted the building, the contractor, not the university, had to correct them.

Hughes: So then the construction company had the expense of redoing it.

Taylor: Rather than the university, yes. Certainly in biology we have followed that pattern, and I think have been successful with all of our new buildings. Of course, this is a process that is not necessarily popular with the architects, contractor and even the university's Planning, Design, and Construction folks. It delays completion when all of them are anxious to get on to the next project.

A Uniquely Berkeleyan Reorganization Plan

Hughes: I know I've asked this question before, but I ask it again. How much of this reorganization scheme of great complexity was being created de novo at Berkeley? Were people either formally or informally looking around to see what other institutions had done in these various areas? How much was unique to Berkeley?

Taylor: The reorganization of so many faculty is unique to Berkeley. You normally can't take a large group of faculty that are as independently minded and reorganize them like Berkeley did in biology. It really is unique. As I said, we've had a lot of other institutions call us and ask for advice. How did we do it? This is what they were thinking of doing. Rod and Dan have been invited to visit places and to explain just how we accomplished this. I've responded to many phone inquiries and have gone to one or two places to describe our process.

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TIMELINE
REORGANIZATION OF BIOLOGY AT BERKELEY

- Late 1970s Biologists begin to complain to administration that UCB can't attract & retain able young biologists.
- Spring '80 Vice Chancellor appoints Internal Biology Review Committee to evaluate programs and space needs.
- Spring '81 Report of Internal Biology Review Committee: Program reorganization & modern space desperately needed.
- April 1981 Report of External Biology Review Committee: UCB needs flexible administrative structure and new bldgs.
- July 1981 "LSB: A Facility Plan for the Biosciences, UCB," prepared by Kaplan/McLaughlin/Diaz.
- July 1981 Project Planning Guide: "Biological Sciences: Construction & Alterations," Submitted to Systemwide recommending construction of 2 new bldgs. and renovation of LSB.
- 1982 Site Studies: Life Science Addition
- 1982 UC Capital Improvement Plan
- April '83 Final Environmental Impact Report
- 5/20/84 California legislature approves funds for phase 1 of reorganization.
- 12/4/84 Recommendations for reorganization by Chancellor's Advisory Committee on Biology.
- July 1985 Second iteration from CABC
- Feb. 1986 [Second] Report of External Biology Review Committee
- Aug. 1986 Final report of CABC, incorporating comments of External Review Committee.
- Jul.1,'89 Administrative reorganization of biology completed: 2 new departments (Integrative Biology, Molecular & Cell Biology) and 6 divisions in L&S created; one new dept. (Plant Biology) created in College of Natural Resources.

Outline for contents of binder on the reorganization of biology at the University of California, Berkeley.

This reference binder, in (reverse) chronological order, was compiled by Louise Taylor, Director of Planning and Analysis and Special Assistant to the Vice Chancellor on Biology, for her use during the reorganization of biology. It is available for research at the Bancroft Library. It contains:

- Vice Chancellor Park's correspondence disseminating the various draft reorganization plans:
 - December 6, 1984: Chancellor's Advisory Council on Biology's "Reorganization of the Biological Sciences," dated December 3, 1984;
 - July 19, 1985: Chancellor's Advisory Council on Biology Subcommittee's "Second Iteration for Reorganization of the Biological Sciences," dated July 15, 1985;
 - November 25, 1985: Chancellor's Advisory Council on Biology's "Modification to the Second Iteration: Reorganization of the Life Sciences," dated November 21, 1985;
 - May 1, 1986: External Review Committee's report;
 - September 5, 1986: Chancellor's Advisory Council on Biology's "Final Report: Reorganization of the Life Sciences," dated August 28, 1986.
- Correspondence with the Berkeley Division of the Academic Senate [pink flags];
- The formal request for approval by the Office of the President of Berkeley's proposed reorganization plan, dated February 1, 1989;
- Vice Chancellor Park's announcement to the campus of the reorganization of biology effective July 1, 1989, dated June 23, 1989;
- Other correspondence to the Biology community related to the reorganization;
- A chronology of the Chancellor's Advisory Council on Biology's membership; and
- A list of some of the department, administrative, planning, and space committees involved in the reorganization, along with their membership and their charge.



Daniel E. Koshland, Jr.

Born in New York City on March 30, 1920, Daniel Koshland received his B.S. from the University of California in 1941. He received his Ph.D. from the University of Chicago in 1949 and was a Postdoctoral Fellow at Harvard University from 1949-51.

Professor Koshland was a Group Leader on the Manhattan Project (1942-46), a Senior Biochemist at Brookhaven National Lab (1951-65) and an Affiliate at Rockefeller University (1958-65). In 1965 he became a Professor at the University of California, Berkeley and was a Guggenheim Fellow (1971-72). He was Editor of *Science* Magazine from 1985-95.

Among his numerous honors are the T. Duckett Jones Award of the Helen Hay Whitney Foundation (1977), the Distinguished Lectureship Award of the Society of General Physiologists (1978), the Pauling Award (1979) and the Edgar Fahs Smith Award (1979) of the American Chemical Society, the Waterford Prize of the Scripps Institute (1984), the Rosenstiel Award of Brandeis University (1984), the City College of NY Bicentennial Distinguished Scientist Award (1987), the Chauncey Leake Award of the University of California at San Francisco (1988), the Merck Award of the American Society of Biochemistry & Molecular Biology (1990), The National Medal of Science (1990), the University of California Alumnus of the Year (1991), the Lasker Special Achievement Award

(1999), the Berkeley Medal of the University of California (2000) and the Seaborg Medal of the American Chemical Society (2000).

Professor Koshland was Chairman of the Biological Division of the American Chemical Society (1968), President of the American Society of Biological Chemists (1973), U.S. Representative of the International Union of Biochemistry (1973-74), Chairman of the Department of Biochemistry at the University of California, Berkeley (1973-78) and Chairman of the Editorial Board of the *Proceedings of the National Academy of Sciences* (1980-1984).

He is a member of the National Academy of Sciences, the American Academy of Arts and Sciences and the American Philosophical Society. He has been a member of the Council of the American Academy of Arts and Sciences (1975-79) and the Visiting Committee for Biology, Harvard Board of Overseers (1975-84).

Editorial boards on which he has served include *Biochimica et Biophysica Acta*, *Journal of Molecular Pharmacology*, *Journal of Biological Chemistry*, *Accounts of Chemical Research*, *Biochemistry*, *Journal of Molecular Catalysis*, *Journal of Molecular Biology*, *Bioorganic Chemistry*, *Annual Reviews* and *Science* 83.

Professor Koshland is the recipient of numerous lectureships and honorary memberships that include the Japanese Biochemical Society, the Royal Swedish Academy of Sciences, and the American Medical Writers' Association. He has received honorary degrees from the Weizmann Institute of Science, the Carnegie Mellon University, the University of Chicago, the University of Massachusetts at Amherst and Brandeis University.

Curriculum vitae

- Name:** Daniel E. Koshland, Jr.
- Born:** March 30, 1920, New York, New York
- Education:** University of California 1937-1941 B.S.
University of Chicago 1946-1949 Ph.D.
Harvard University, Postdoctoral Fellow 1949-1951
- Professional Experience:** Professor, University of California, Berkeley 1965-
Editor, *Science Magazine* 1985-95
Senior Biochemist, Brookhaven National Lab 1951-1965
Affiliate, Rockefeller University 1958-1965
Group Leader, Manhattan Project 1942-1946
Chemist, Shell Chemical Company 1941-1942
- Honors:** Albert Lasker Award for Special Achievement in Medical Science, 1998
Clark Kerr Award, University of California, 1994
Alexander M. Cruickshank Lecturer Award, 1994
American Academy of Microbiology Distinction of Fellow Award, 1994
Gilbert N. Lewis Medal, University of California, 1993
Givauden Rouve Award, Sarasota Florida, 1993
University of California Alumnus of the Year, 1991
The National Medal of Science, 1990
Merck Award, Amer. Society of Biochemistry & Molecular Biology, 1990
Chauncey Leake Award, University of California, San Francisco, 1988
City College of NY Bicentennial Distinguished Scientist Award, 1987
Rosenstiel Award, Brandeis University, 1984
Waterford Prize, Scripps Institute, 1984
Edgar Fahs Smith Award, American Chemical Society, 1979
Pauling Award, American Chemical Society, 1979
Distinguished Lectureship Award, Society of General Physiologists, 1978
T. Duckett Jones Award of Helen Hay Whitney Foundation, 1977
President, American Society of Biological Chemists, 1973
The Berkeley Citation, University of California, Berkeley, 1970
National Academy of Sciences
American Academy of Arts and Sciences
American Philosophical Society
Honorary Member, Japanese Biochemical Society
Honorary Member, Alpha Omega Alpha, Medical Honor Society
Honorary Member, Royal Swedish Academy of Sciences
Honorary Fellow, American Medical Writers' Association, 1990
Faculty Research Lecturer, University of California, Berkeley, 1980
Visiting Fellow, All Souls College, Oxford, 1972
Guggenheim Fellow, 1971-72
Chairman, Biological Division, American Chemical Society, 1968
- Honorary Degrees:** Honorary Ph. D., Weizmann Institute of Science, 1984
Honorary.Sc. D., Carnegie Mellon University, 1985
Honorary LL. D., Simon Fraser University, 1986
Honorary D.H.L., Mt. Sinai School of Medicine, C.U.N.Y., 1991
Honorary LL.D., University of Chicago, 1992
Honorary ~~Ph.D.~~, University of Massachusetts at Amherst, 1992
Ph. D.
- Honorary Name Lectureships:** Walker Ames Lecturer, University of Washington, 1964
Eli Lilly Lecturer, Eli Lilly Company, 1968
Phillips Lecturer, Haverford College, 1968
Alpha Chi Sigma Lecturer, Washington State University, 1969
Carter Wallace Lecturer, Princeton University, 1969
Harvey Society, 1969
Rennebohm Lecturer, University of Wisconsin, 1970
National Sigma Xi Lecturer, 1970
Christian Herter Lecturer, New York University, 1971
Schaeffer Memorial Lecturer, Washington University, 1971
Battelle Lecturer, University of Washington, Seattle, 1971
Welch Foundation Lecturer, 1971
Leo Marion Lecturer, National Research Institute, Canada, 1972
Weizmann Memorial Lecturer, Weizmann Institute, 1971
Phi Beta Kappa Lecturer, 1974 and 1976
Johns Hopkins University Lecturer, 1974
Nieuwland Lecturer, University of Notre Dame, 1975
Sutherland Memorial Lecturer, University of Miami, 1976
Weissburger Lecturer, Rochester University, 1977
John Edsall Lecturer, Harvard University, 1979
NIH Distinguished Lecturer, 1982
Carnegie Mellon Distinguished Lecturer, 1982
Pauling Lecturer, Stanford University, 1982
Rudin Lectures, Columbia University, 1985
William H. Stein Lecturer, Rockefeller University, 1985
Robt B. Woodward Visiting Professor in Chemistry, Harvard, 1986-87
Philip R. Jonsson Visiting Professor, University of Texas, 1990
R.C. Fuller Lecturer on Science and Public Policy, U.Mass.at Amherst, 1992
Linnaeus Lecture, University of Uppsala, 1992
- Service on Editorial Boards:** *Biochimica et Biophysica Acta*, 1968-72
Journal of Molecular Pharmacology, 1970-74
Journal of Biological Chemistry, 1961-72
Accounts of Chemical Research, 1970-82
Biochemistry, 1972-77
Journal of Molecular Catalysis, 1977-
Journal of Molecular Biology, 1972-78
Science, 1969-75
Bioorganic Chemistry, 1971-
PNAS, Chairman, Editorial Board, 1980-84
Annual Reviews, 1982-
Science 83, 1983-86
- Educational and Governmental Agencies:** Chairman, Academy Forum, National Academy of Sciences
Council of American Academy of Arts and Sciences, 1975-79
Visiting Committee for Biology, Harvard Board of Overseers, 1975-84
Chairman, Public Policy Committee, Amer.Soc.of Biol. Chemists, 1973-75
U. S. Representative, International Union of Biochemistry, 1973-74
Chairman, Department of Biochemistry, U.C. Berkeley, 1973-78
Chairman, Gordon Conference on Proteins, 1968
Chairman, Chancellor's Advisory Council on Biology, University of California, Berkeley, 1982-1993

RODERIC B. PARK - 2003

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Date and place of birth: Born 1/7/32, Cannes, France Nationality: U.S. citizen

Education:

A.B., Harvard College, 1953
Ph.D., California Institute of Technology, 1958

Academic Posts:

Postdoctoral Fellow, California Institute of Technology 1958
Postdoctoral Fellow, University of California Radiation Laboratory, 1958-60,
with Dr. Melvin Calvin.
Assistant Professor of Botany, University of California, Berkeley, 1960-63.
Associate Professor of Botany, University of California, Berkeley, 1963-66.
Professor of Botany, University of California, Berkeley, 1966-89.
Professor of Plant Biology, University of California, Berkeley, 1989-92.
Professor Emeritus of Plant Biology and The Vice Chancellor Emeritus,
University of California, Berkeley, 1993 - present.

Academic Senate Posts

Member, Berkeley Academic Senate Select Committee on Education (Muscatine
Committee) 1965-67.
Member, Berkeley Academic Senate Policy Committee 1966-68.
Member, Berkeley Academic Senate Budget Committee 1969-70.
Member, College of Natural Resources Executive Committee, 1992.

Academic Administrative Posts

Chairman of the Department of Instruction in Biology University of California,
Berkeley, 1968-69.
Provost and Dean College of Letters and Science University of California,
Berkeley, 1972-80.
The Vice Chancellor University of California, Berkeley, 1980-90.
Chair, Biosciences Building Committee University of California, Berkeley, 1982-
90.
Member, Ten Meter Telescope Executive Management Committee University of
California, Berkeley, 1983-86.
Convener, Council of Vice Chancellors, University of California, 1987-88.
Executive Assistant to the Chancellor University of California, Berkeley, July-
August 1990.

Acting Director, The University Herbarium and The Jepson Herbarium, 1991- 93.
 Chancellor, The University of Colorado at Boulder, 1994-1997.
 Senior Associate for Academic Development, University of California, Merced,
 2000-2001

Boards, Committees and Trusteeships

Assistant Editor of Annual Reviews of Plant Physiology, 1966-67,
 Associate Editor of Annual Reviews of Plant Physiology, 1967-72.
 Editor of W.H. Freeman Co. Biology Series, 1966-72.
 Board of Directors, William Kaufmann, Inc. 1975-86.
 Member, Corporation of the Woods Hole Oceanographic Institution 1974-78.
 Director, Member, Council on College Level Services of the National College
 Board 1978-80.
 Associated Harvard Alumni 1975-78.
 Member, Harvard Board of Overseers, 1981-87.
 Board of Trustees, Athenian School 1981-93 Chair, 1986-93.
 Trustee, University of California Berkeley Foundation 1985-1990.
 Member, University Labor Relations Council 1984-1990.
 Chair, National Association of State Universities and Land Grant Colleges
 (NASULGC), Council on Academic Affairs, 1988-89.
 NASULGC Senator from Council on Academic Affairs, 1989-91.
 Trustee, the Jepson Endowment, 1991 - present.
 Chair, Jepson Trustees 1993-present.
 Business Advisory Committee, Annual Reviews Inc. 1991-94.
 Director, Richmond Yacht Club Foundation, 1991- 94.
 Woods Hole Marine Biological Laboratory Council of Visitors, 1994-2002.
 College of Natural Resources Advisory Committee, 1995-present.
 Chair, College of Natural Resources Development Committee, 1998-present.
 Trustee, University of California Berkeley Foundation, 1999-2001.
 Trustee, Tabor Academy, 1999-present.
 Board of Visitors and Fellows Department of Viticulture and Enology
 2002-present, Chair 2003-present

Academic Specializations:

- (i) main field: Plant Physiology and Molecular Biology
- (ii) other fields: Geochemistry
- (iii) current research interests: Molecular Biology of sieve tube elements in
 relation to plant defense mechanisms and phloem transport.

Honors, Membership in Professional Societies:

Arthur McCallum Summer Fellowships 1955-57
 Woods Hole Oceanographic Associates Fellowships 1955-58
 New York Botanical Gardens Award 1962
 Miller Professor 1969-70

D.Sc.h.c., York University, 1980
AAAS, ASPP
Elected AAAS Fellow 1990.
Appointed to The Berkeley Fellows 1999

Research Publications

84 publications and two books in the fields of terpene biosynthesis, stable carbon isotope geochemistry, photosynthesis and plant ultrastructure and DNA sequences in conifers.

Manuscript in preparation

It's Only the Janitor - A Guide to the Customs and Inhabitants of the Academy.
This is a somewhat whimsical account of the major issues facing higher education and strategies for dealing with them based on experience at Berkeley, Harvard, Colorado, UC Merced and elsewhere.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name LOUISE KUULEINANI EDLER TAYLOR

Date of birth 2-19-1938 Birthplace HONOLULU, HA

Father's full name FRED THEOBALD EDLER

Occupation ACCOUNTANT Birthplace POTSDAM, GERMANY

Mother's full name ADRICE ADELE RICE EDLER

Occupation HOUSEWIFE Birthplace ARCATA, CA.

Your spouse CARTER COLLINS TAYLOR

Occupation ATTORNEY Birthplace EVANSVILLE, IND.

Your children CARTER COLLINS TAYLOR, JR.

ZACHARY RICE TAYLOR

Where did you grow up? HONOLULU, BERKELEY, SAN FRANCISCO

Present community PIEDMONT

Education B.S. '59 UC, BERKELEY

Occupation(s) BUDGET DIRECTOR to 1972, PLANNING

DIRECTOR 1972-1998

Areas of expertise UNIV. OF CALIF. BUDGET and POLICIES

Other interests or activities INTERIOR DESIGN, GARDENING,

READING, ARCHITECTURE

Organizations in which you are active PRESIDENT - UCB EMERITI ASSN.

2002-03 BERKELEY REP to UC COUNCIL OF EMERITI ASSOCIATIONS.
PIEDMONT CIVIC ASSOCIATION, PIEDMONT CAPITAL IMPROVEMENTS CMTE.

SIGNATURE Louise B. Taylor

DATE: 2-26-03

Sally Smith Hughes

Graduated from the University of California, Berkeley, in 1963 with an A.B. degree in zoology, and from the University of California, San Francisco, in 1966 with an M.A. degree in anatomy. She received a Ph.D. degree in the history of science and medicine from the Royal Postgraduate Medical School, University of London, in 1972.

Postgraduate research histologist, the Cardiovascular Research Institute, University of California, San Francisco, 1966-1969; science historian for the History of Science and Technology Program, The Bancroft Library, 1978-1980.

Presently research historian and principal editor on medical and scientific topics for the Regional Oral History Office, University of California, Berkeley. Author of *The Virus: A History of the Concept*, Sally Smith Hughes is currently interviewing and writing in the fields of AIDS and molecular biology/biotechnology.

