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University of California
Berkeley, California

Daniel Koshland, Jr. Retrospective Oral History Project:

Bruce Alberts

Interviews conducted by
Sally Smith Hughes
in 2012

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Bruce Alberts, 2012

Bruce Alberts is currently editor-in-chief of *Science* magazine, arguably the premier scientific journal in the world. From 1993 to 2005, he was president of the National Academy of Sciences, another highly prestigious position. He is also a member of the Department of Biochemistry and Biophysics at UCSF, of which he was chairman from 1985 to 1990. He speaks in the two interviews from the perspective of a decades-long colleague and friend of Dan Koshland. Of particular note is his informal advisory role in the planning and execution of the Marian E. Koshland Science Museum in Washington, DC.

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Introduction by Sally Smith Hughes

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

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

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

Interviewees included:

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

Project Staff included:

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

Sally Smith Hughes
Berkeley, CA, 2014

Interview #1: March 19, 2012
Audio File 1

01-00:00:00
Hughes: Let me first explain, because the funding for this project comes from Yvonne Koshland. Dan and I had done a long series of interviews but we stopped in 1999, and Yvonne wanted his last eight years to be covered. She gave me a list of people that she would like interviewed, and when I saw your name on it, I said, I've been wanting to interview Bruce Alberts for all these years. Could we make it longer than just about Dan? And Yvonne said yes. I really want you to talk about Dan too.

01-00:01:22
Alberts: But that's much later then. Do you like to go in chronological order?

01-00:01:32
Hughes: Yes.

01-00:01:35
Alberts: That would make sense! [laughter]

01-00:01:40
Hughes: I will start where I usually start, which is, please tell me about your family of origin and your education.

01-00:01:58
Alberts: You mean that far back?

01-00:02:00
Hughes: Yes! Definitely. We want to know where this man came from!

01-00:02:05
Alberts: [laughing] Oh my God, this will take forever. I was born in Chicago, Illinois on April 14, 1938. And we lived in Chicago for two years. I don't remember much about that, but then we moved to a Chicago suburb called Glencoe where I spent all my years through high school in the same house. My father, Harry C. Alberts, originally studied mechanical engineering, then worked in the U.S. Patent Office in Washington. While he was doing his day job he was going to night school and became a patent attorney, so he had his own practice in downtown Chicago as a patent attorney the whole time that I was growing up. And of course he very much wanted me to take over his practice. Obviously, even if I enjoyed patent law, the idea of working for your father was not very appealing, at least not in those days, not with a father who was as strong-willed as my father. [laughing] It's hard to grow up and work for your parents.

At any rate, Glencoe is on the North Shore. It's an upper-middle-class suburb. It has relatively good schools. I went to all the public schools, ended up at New Trier High School. I got very interested in chemistry because my homeroom was the chemistry room at this very large high school. They had this very nice system that fifteen minutes in the morning when you came in, you were in the same homeroom all four years, so you had a homeroom teacher. They tried to make some community out of what was otherwise a pretty chaotic environment with so many students. My homeroom teacher was Carl [W.] Clader, who was the lead chemistry teacher at the school, and our homeroom was the chemistry lab. So we were sitting in the lab fifteen minutes every morning at a lab bench. In front of us was a well with fuming sulfuric acid, fuming nitric acid—all those things that you can't get to these days. [chuckling] So it made chemistry very much alive to actually be able to see metallic sodium and throw it in water and have it explode. At any rate, I did have him as a chemistry teacher, I guess it was my second year in high school, and I got very interested in chemistry.

01-00:05:04
Hughes: What attracted you to chemistry?

01-00:05:16
Alberts: The logic of chemistry. It was the first real science that I got involved in. I never took biology in high school, because in typical fashion I signed up too late and the class was full. [laughing] So I took radio and became a radio amateur instead. So chemistry was my first real science course, and I thought it was very logical and interesting and obviously important. The chemistry book was very much oriented to practical uses of chemistry. And Carl Clader was a very interesting and lively character. We had a back-to-school night, I think when I was a junior in high school, and I wanted to see what I could do with chemistry. There were two talks—these were all by parents who had anything to do with chemistry. One was by a chemical engineer, a very boring talk with lots of big pipes and it seemed a really dreadful kind of thing to do. I guess he was processing large amounts of chemicals. The other one was by a doctor who talked about how important chemistry and science were for medicine. I think he overstated the case. But anyway, there were no scientists in view anywhere for me. I didn't even know you could be a scientist, so I decided I'd be a premed, largely because of my interest in chemistry. Only much later did I find out that you could actually be a scientist and use chemistry!

01-00:07:07
Hughes: Really!

01-00:07:08
Alberts: Well, we all had heard that Einstein worked in a patent office.

01-00:07:15
Hughes: True.

01-00:07:15
Alberts: None of us had ever seen any scientists, and I think that's still largely true today. Most kids never have any contact with a scientist.

In those days you applied to colleges without ever seeing them. Nobody ever went to visit colleges. I think I applied to Northwestern, Amherst, and Harvard, and I chose to go to Harvard because my mother thought this would be the best. So many people go to Harvard for the wrong reasons, and she'd heard of Harvard. So in the fall of 1956 I arrived at Harvard and I had never seen the place before. Because of the background I told you about, I came as a Harvard premed. Of course, as a premed you have to take all these science courses. So freshman year I took my first biology course, which was really boring. And then chemistry, which was good.

01-00:08:29
Hughes: Now this was probably pre-molecular biology, wasn't it?

01-00:08:31
Alberts: Oh, of course.

01-00:08:35
Hughes: What year are we now?

01-00:08:35
Alberts: Nineteen fifty-six.

01-00:08:39
Hughes: Well, there was Watson and Crick.

01-00:08:40
Alberts: Yes, but we didn't learn about that. [laughter] It was still not in the textbooks. Actually, the college science courses I had weren't that interesting, but of course you took them because you were a premed. The way I got to find out what science was is that after being in a laboratory, because all these science courses had laboratories—physics, chemistry, biology—so I was in a laboratory probably three afternoons a week for three years. The worst laboratory of all was the physical chemistry laboratory, which was my junior course, and even though I liked the course I couldn't stand the laboratory. After the first semester, I thought I might be able to take the rest of the course without the laboratory. I went to my tutor who was Jacques Fresco, and he was, I guess, an early molecular biologist of sorts. He was working in the laboratory of Paul [M.] Doty who was a famous chemist. He had started as a polymer chemist and then started doing very innovative work applying what he knew about polymer chemistry to proteins and nucleic acids, which nobody else was doing in the early 1950s. Jacques Fresco, my tutor, was a senior postdoctoral fellow in the laboratory of Paul Doty. He discovered that I could get out of the physical chemistry course only if I did independent research, and so he suggested I come and work with them.

01-00:10:48
Hughes: Why was he being so helpful to you?

01-00:10:50
Alberts: Well, he was being paid to be my tutor, I suppose. [laughter] So I was one of his tutees, and I had started that I think my sophomore year. So he would just give me something to read and I would read it. It wasn't a heavy duty tutorial like at Oxford, but it was modeled after it.

01-00:11:11
Hughes: I'm speculating that he was seeing something in Bruce Alberts.

01-00:11:18
Alberts: Well, you'll have to ask him. [laughter] I think anybody who asked to get out of the physical chemistry lab he might have said come and work with me. But I had gotten very good grades at Harvard, so he knew my grades. And of course we had interacted a bit talking. I don't know whether I made a good impression on him or not. Obviously he was important in getting me to see what science was. That was the second semester of my junior year. So when I took the second semester of physical chemistry, I didn't take the physical chemistry laboratory but I worked in a research laboratory instead.

01-00:12:07
Hughes: The powers that be counted that?

01-00:12:07
Alberts: They counted that as my physical chemistry lab. It didn't affect at all how I did in physical chemistry because the lab had nothing to do with the course, as usual. I'm now a big critic of those labs. In fact *Science* magazine right now has a competition for the best inquiry-based college laboratories. We're just publishing one a month. Based on my terrible experience, I call them "cooking" laboratories. And I had thousands of hours of that stuff!

Anyway, so I then decided I would spend the summer at Harvard working in the same lab and use that [research] for my senior thesis. At Harvard a senior thesis is optional, but of course since I was in the lab and I was talking to my tutor, I found out that I could do that. So I spent the whole summer of 1959 working with Jacques Fresco in Paul Doty's laboratory.

01-00:13:18
Hughes: Were you unusual in being an undergraduate in a prestigious laboratory?

01-00:13:21
Alberts: Yes. At that stage it wasn't the normal thing to do. Now it is. I was surrounded by graduate students and postdocs. At any rate, that was a great summer. Jacques gave me a problem that was solvable. It turned out to be very interesting. We published [1960] two papers from my summer in the lab.

01-00:13:54
Hughes: Wow! As an undergraduate?

01-00:13:53
Alberts: Yes. So I had the misimpression that science was very easy because I had just lucked into a problem. [laughing] It had all been set up for me.

01-00:14:06
Hughes: Was your problem related to the protein theme in the lab?

01-00:14:16
Alberts: No, it was the structure of RNA. At that point there was something called the RNA Tie Club.

01-00:14:24
Hughes: Oh, I've read about that.

01-00:14:24
Alberts: It was a group of people inspired by George Gamow, the guy who wrote *One, Two, Three...Infinity*, who was a real innovator and energizer of the scientific community. Paul Doty was part of the RNA Tie Club, and the point was that the structure of DNA had been solved, but RNA did not have two complementary strands so how could it form helical structures? What was its structure? We figured out what the structure was in the summer project by just using synthetic polymers. As I said, it was all set up for me. The strategy was already set up by Jacques Fresco, and I had the materials. I just made measurements, and it all worked out very well.

01-00:15:20
Hughes: So you were the hands, so to speak.

01-00:15:23
Alberts: Right, exactly. I had to make decisions, and I worked a lot of hours. So I got a lot done and it was very interesting, exciting.

01-00:15:43
Hughes: When you say you figured out the structure, you mean the fact that it was a single strand?

01-00:15:46
Alberts: No, we knew it was single-stranded. Doty and colleagues had shown from the optical properties of the UV [ultraviolet] absorbance that it was partially helical. DNA was completely helical. But RNA was partially helical, so how was Watson-Crick base pairing possible? Because the polymer didn't have complementary strands as far as anybody knew. They'd made a random copolymer of the four nucleotides by using the enzyme polynucleotide phosphorylase, which is how you made synthetic RNAs at that time. That also had a helical structure. So it wasn't that RNA had specific regions that were exactly matching and could form a helix like DNA. So everybody thought the structure was some kind of mystical, special structure. But through these experiments that we did it was clear that you didn't need that. All you needed was a random sequence and then the ability to make, when it didn't match, a little loop on one side. We had shown that loops formed. My fundamental

experiment was showing that loops could form when you had a mismatch. And then from that it was clear that you could easily make RNA molecules partially helical. It didn't require a special structure, just a regular complementary helix with a few mispaired bases where they didn't fit would do it. I bought a bunch of beads and threw them on the floor, four different colors, and I picked them up randomly and showed—that was the second paper in *Nature*—that if you did that and allowed this looping out when you didn't have a match, you could get the 50 percent or so helicity that an RNA molecule had. So basically that summer project solved the RNA Tie Club problem in a very simple and unexpected way, because people thought RNA structure was going to be something very special.

01-00:18:10
Hughes: What happened to your reputation after those two papers as an undergraduate?

01-00:18:13
Alberts: My reputation? [laughing] What do you mean my reputation? I was an undergraduate.

01-00:18:16
Hughes: Well, that doesn't sound to me like a minor discovery.

01-00:18:21
Alberts: Yes, but it was done in a lab and so I had other coauthors much more famous than me, including Paul Doty on the second paper.

01-00:18:32
Hughes: Was he paying attention to you by the second paper?

01-00:18:33
Alberts: Well once we had the results, yes. [laughter] In fact, he rewrote the paper that I had written. He's a very good writer. So I did very well that summer. By the end of the summer, before we even had written these papers, I decided I wasn't going to medical school, I was going to go to graduate school and get a PhD, which disappointed my parents, because everybody wants their kid to be a doctor and take care of them. [laughing] Not only was I not going to be a patent attorney, but now I wasn't even going to be a doctor! And [being] a doctor of course is much more prestigious.

01-00:19:18
Hughes: Well, it also flashes back to your previous statement that students didn't know that you could become a scientist.

01-00:19:24
Alberts: Right. The image of scientists is you're weird, and you're working in isolation. It's all wrong. I think my parents had no real idea what scientists did, nor did I. I had to stay in Cambridge, Massachusetts for a year because my wife, Betty Neary Alberts—I had gotten married right after graduation, and my wife had one more year of school. So my idea was to spend a year working in Paul Doty's laboratory on a different idea I had ginned up during

part of a course I took my senior year, where you had to write a research project. I had come up with an idea of how to solve the genetic code. [laughter]

01-00:20:26
Hughes: [Facetiously] Just a minor problem.

01-00:20:29
Alberts: We were very ambitious.

01-00:20:31
Hughes: You and Herb Boyer. You know Herb Boyer tried to solve the same problem early on too.

01-00:20:36
Alberts: Yes.

The course that I was taking was taught at MIT, for some reason. It was taught by a famous guy, Cy[rus] Levinthal, and he really liked this paper. I got an A+ or something. Thinking that science was easy and having no sense of experimental strategy, I spent that year trying to do the experiments that I had hypothesized to solve the genetic code as an amateur scientist in Paul Doty's lab in the first year of graduate school. I was in the graduate program but my plan was to shift to Stanford after that—I had been accepted at Stanford—because my wife wanted to go to the School of Education at Stanford where she was accepted.

01-00:21:34
Hughes: So your consideration of Stanford had nothing to do with its scientific reputation?

01-00:21:39
Alberts: I didn't know anything about Stanford, really, except that it was good. At any rate, in the end I got intrigued with the promise of solving the genetic code, and I decided I wanted to continue to work on that. So my wife agreed that she would stay in Boston and not go to graduate school. Instead she started teaching in the Boston public schools. That was probably a mistake because I didn't know how to do science, and I spent years trying to solve the genetic code by a method that did not work. If I had known more about science I would have done the controls first. In fact, the method wouldn't work because the controls that I eventually did showed that you couldn't do what I had postulated you could do.

01-00:22:38
Hughes: Had Dr. Fresco disappeared from your life?

01-00:22:41
Alberts: Yes. He had moved on to Princeton. So yes, only that one project was done with him.

01-00:22:53
Hughes: So there was nobody giving you advice?

01-00:22:53
Alberts: Well, I was with Paul Doty, but he had thirty-seven people in his lab, and he was on President Kennedy's science advisory panel. And he was central to Pugwash Conferences, doing what eventually developed into what they called the Doty Group that held regular discussions with Soviet physicists to try to make a bridge of understanding between nations—which is probably how I got infected with science policy, although I didn't realize it at the time. In fact, Doty just died. His memorial is May 4 [2012]. I'm going up to Harvard. But anyway, the whole style of the Doty Lab, given his other commitments—and I think his nature anyway was just let people do what they thought was interesting.

I've written up in *Nature* how I failed my PhD exam.[1] You have that paper? You should definitely see that one.

01-00:24:10
Hughes: I have not seen that.

01-00:24:10
Alberts: After four and a half years of work, Paul Doty thought I was finished. I wrote a thesis and he helped me write and rewrite. He was very useful to help me write. I didn't know how to write scientific papers. I went into this oral exam with Harvard faculty that nobody had ever failed, and as soon as I walked in they said I had failed. [laughing] They didn't believe my experiments. Paul Doty would normally have been there but he got called away to Russia or someplace, so he wasn't there to defend me. I think he was kind of upset.

So I spent another six months in Cambridge, redoing more experiments on so-called naturally cross-linked DNA, which became the topic of my thesis because the other thing didn't work out. Eventually I got a PhD. But it was very inconvenient, because we had already bought our plane tickets to Geneva. I'd bought a car that was sitting in a garage in Geneva, and I had to eventually pay parking for it for six months. We also had given up our apartment, that was the most important thing, so we had no place to live. So I spent six months moving around. When somebody would go on vacation, we'd go into their house or their apartment, and it was very hard. I had a baby, Beth Lynn Alberts, I had a one-year-old kid, and my wife was typing my thesis, and it was chaos! Even more chaos than usual in my life. [laughing] At the end, Paul Doty put me up in a hotel the last month.

01-00:26:06
Hughes: Looking back, do you think there was justification in the committee rejecting your first thesis?

[1] *431*:104 (October 28, 2004).

01-00:26:15
Alberts: Not really, because the rejection was based on the fact that they didn't believe that naturally cross-linked DNA existed, and they made me do more experiments to show that it did exist. But to me it was obvious that it existed. The fact is we still don't know where it originates from. So I thought at the end of my career I should come back and try to figure that out! But I haven't had time! Maybe when I finish at *Science* magazine [as editor-in-chief]. Because now we have many new technologies and techniques. Nobody's working on this.

01-00:26:58
Hughes: I've heard the term but it doesn't mean much to me. Could you say in a nutshell why people would be interested in cross-linked DNA?

01-00:27:08
Alberts: Well, cross-linked DNA happens when you treat DNA with certain mutagens, chemicals that create a covalent bond that links the Watson [strand] with the Crick strand. You can see it very easily, because when DNA is covalently linked, when you denature it by strong heat or alkali and then remove the heat or the alkali, normally DNA then takes the structure of RNA, that is it's a single strand and it's got hairpin helices, the kind of thing I showed for RNA. But if it's cross-linked, the bases are held in register, so that part of the DNA molecule that contains the cross-link will immediately zipper up and form a perfect helix. So that was already very well known. But it turns out that DNA quite naturally has a few of these cross-links in it, which I discovered by accident when I was trying to prove another theory. After the genetic code experiment, I tried to do another overambitious project, which was to show you can figure out how you replicate a double-stranded helix inside of a cell with only one enzyme, DNA polymerase. This is described in my article in *Nature*, which I'll give you.

01-00:28:34
Hughes: Yes, I'd like to see that.

01-00:28:38
Alberts: That led me accidentally to discovering this naturally cross-linked DNA, because one of my hypotheses was that there would be naturally cross-linked DNA in cells. Unfortunately, it wasn't where it was supposed to be! [laughing] It was randomly located all over the place. But the reason why I failed the PhD exam was basically that they didn't believe that this thing existed, I think. It was a famous committee. It was Wally Gilbert, Jim Watson, Matt Meselson, Charlie [Charles C.] Richardson, I think, from Harvard Medical School. It was a very distinguished committee!

01-00:29:34
Hughes: Was that intimidating? Those were—*are* still—huge names.

01-00:29:37
Alberts: Well, I knew the faculty. As graduate students we had interacted with the faculty. It wasn't intimidating, because nobody ever failed this thing. I went into my oral exam very relaxed. I thought it was just a formality! And they didn't really examine me. They just said they had read my thesis, and I had to do more experiments. It was just like getting a review on a paper. They had thought of some more experiments I didn't do. There may have been a little bit of a sense on the committee that Paul Doty was not looking after me well enough, and they were going to show him that he should spend more time at Harvard. But he was doing this important thing for the government. I don't know. At any rate, as I wrote in the *Nature* article, it was a very important part of my career because it made me think very deeply about why I hadn't done better on my thesis. If they had just passed me, I probably would have continued to make the same kind of mistakes, and again, that's described in the *Nature* paper. But in science having a really well-thought-out strategy is critical. And I was way overconfident because of my undergraduate experiences! [laughing] We also thought that we could predict, like Watson and Crick, how things worked. But now we know we can't.

01-00:31:08
Hughes: Well, they didn't do experiments.

01-00:31:10
Alberts: No, but that was a big success in theoretical biology. But we haven't had many successes since then.

01-00:31:22
Hughes: I do think its interesting that Watson was on the committee and damning you for not doing enough experiments when he didn't do a single one!

01-00:31:33
Alberts: I'm trying to think. Jacques Fresco sent me something that showed the committee members. I know it was Gilbert, Meselson, and Charlie Richardson. I'm not sure that Watson was on the committee. Watson and Doty were close friends, and he probably would have supported me.

01-00:31:47

Hughes: You had a bit of bad luck.

01-00:31:53
Alberts: Yes, I suppose. So then I went on to a postdoc. In those days a postdoc was only one year. I already had a job at Princeton.

02-00:32:03
Hughes: Really?

02-00:32:05
Alberts: Yes. I had a job before I left, from Fresco recommending me.

01-00:32:10
Hughes: Why did you go to Geneva for a postdoc?

01-00:32:19
Alberts: Well, I went to Geneva because the people who were in Geneva recruited me to come. They were setting up a new department of molecular biology at the University of Geneva, and Alfred Tissières and his colleague Pierre Spahr were the head of this thing, and they were both at Harvard. They recruited a bunch of people that they knew to come as postdocs. I'd never been to Europe. I'd never been out of the country really, so it was very appealing to go to Geneva as against going to Stanford or someplace like that. I think that it was a very good experience, because there were lots of outstanding postdocs there from the United States, and we all interacted with each other and learned a lot from each other.

01-00:33:14
Hughes: Who was there?

01-00:33:19
Alberts: I'm trying to remember all of them. Ray Gesteland, who became dean of science at Utah. I can give you a whole list, actually.

01-00:33:40
Hughes: You'll have another chance when you review the transcript.

01-00:33:43
Alberts: Yes, I can make a list of people. [Added in transcript: John Collier, David Pettijohn, Rolf Benzinger, Winston Salzer, John Richardson, John Hershey.]

01-00:33:44
Hughes: Did that help you, having met at a personal level these people who were then going to go on to be movers and shakers?

01-00:33:54
Alberts: Yes. They all came from different backgrounds, and so they had different approaches and knew different techniques. That convinced me that the way you do creative things is to mix people with different backgrounds together. Much later, I was able to set up programs of this type as President of the National Academy of Sciences. At any rate, it was a very wonderful year. I met Dick Epstein who became my real advisor, even though I had applied to work with Tissières. He introduced me to the bacteriophage T_4. He had done all the genetics of bacteriophage T_4. I spent most of my early career working on the biochemistry of DNA replication with that bacteriophage, using the mutants that he had identified and I didn't know about when I was an undergraduate or graduate student, because I hadn't read the literature. His work was published in '63, and I only found out about it at the end of 1965. It would have spared me time doing bad experiments in the latter part of my graduate career.

01-00:35:05
Hughes: That's the work that you continued at Princeton?

01-00:35:09
Alberts: Yes. During my one year [1965-66] at Geneva I developed a technique called DNA cellulose chromatography, a form of affinity chromatography. That was a result of my failing my PhD exam. I had decided I'd take a month off and figure out what went wrong and whether I should continue doing science. You know, failing an important exam makes you doubt—am I'm really not able to do this kind of work?

I decided I would only do experiments that I knew, whichever way they came out, that I'd learn something. On my list of the kinds of experiments to do was to design new methods that would then allow you to do things that nobody else could do. That's also been a theme of my career. That came out of Doty's lab, because when I was there, there were a bunch of postdocs and graduate students racing to solve the genetic code by what then became the standard method using synthetic polynucleotides. The [Marshall W.] Nirenberg poly-U experiment on the genetic code was, I think, 1961. So I had started working in the lab in 1960 trying to solve the genetic code a different way. But as soon as the synthetic polynucleotide route came out, then people started making all these mixed copolymers and everybody was doing the same experiment.

By that time I had dropped my genetic code experiment. I knew it wouldn't work, and I was working on other things. But around me were all these people racing, unsuccessfully, to beat out other labs in getting the next codon. I felt that that was an awful kind of science to do, because even if they beat the other lab by a month, they weren't really contributing anything. So part of my conclusion, in thinking about how to do science, was to always try to do something that would not be done by other people. So that's why developing this method made sense, because then I could do experiments that other people couldn't do.

01-00:37:17
Hughes: I see. Is this the technique for isolating—

01-00:37:26
Alberts: For isolating proteins that work on DNA without having to know exactly what they do. That technique allowed me to discover one of the mysterious T_4 proteins that was known from genetics to be needed to replicate DNA. We had mutations in seven different T_4 replication genes from Epstein and his colleagues. I could therefore easily ask, what's the difference between the proteins that bound and eluted with salt from a DNA cellulose column in a wild-type infection compared with a mutant infection? My first major discovery was the T_4 gene 32 protein, which turned out to be the first single-strand DNA binding protein. That wasn't really found until I got to Princeton the next year [1966]. My first year [1965] in Geneva all I think I did was show that this method would work, that that there were lots of proteins that bound to

the column-produced T_4–infected cells and that the column bound things that we knew should bind, like RNA polymerase. That first year in Geneva ended up being the basis for a 1968 paper on DNA cellulose chromatography which was really my first independent work. It was published in the *Cold Spring Harbor Symposium on Quantitative Biology* (*33*:289-305, 1968).

01-00:38:48
Hughes: Were you sole author?

01-00:38:53
Alberts: No, the work also involved two technicians and two Princeton undergraduates.

01-00:38:53
Hughes: Was that technique then taken up by other people?

01-00:38:57
Alberts: Yes.

01-00:39:00
Hughes: Did it become an alternative to the synthetic approach?

01-00:39:03
Alberts: Well, it was used by many other people. It was an early affinity chromatography technique, and it has been used for different purposes. But I particularly focused on using it for studying DNA replication, and that eventually led to isolating all the T_4 replication proteins that were needed for replicating a double helix. These seven proteins form a protein machine, and we only got it to work in 1975. By then we had all the proteins purified. But then one magic day we mixed them all together in the right combinations and we could finally replicate double-helical DNA.[2]

01-00:39:53
Hughes: How did you feel about that?

01-00:39:55
Alberts: [laughing] That was my great day! See, I'd been trying to do that since my failed graduate thesis.

01-00:40:06
Hughes: I want to go back to your comment about competition. Competition, you feel, doesn't necessarily lead to good science; it's just one-upmanship. Did I get that right?

01-00:40:21
Alberts: Well, you can get good science done, but you're not going to make a major contribution, because if you aren't there, it's going to be done anyway.

[2] *Proceedings of the National Academy of Sciences USA, 72*: 4800-4804, 1975.

01-00:40:34
Hughes: Is it also that you, Bruce Alberts, don't thrive under the culture of competition?

01-00:40:42
Alberts: Yes, that's probably true. There are some people who love competition—that's their whole thing.

It wasn't that I don't like to compete with people, but the consequences of working that way are multiple, and they're all, to me, negative. First of all, you know you're not going to make a very unique contribution because your discovery may be a month earlier but if you weren't there it would still be found. Second of all, these are laboratories where you don't want to tell people what you're doing, because your competitors might find out exactly what you're doing and that might help them beat you. Thirdly, you're trying to do things in a hurry rather than doing them well necessarily. And so all three aspects I found unappealing.

01-00:41:46
Hughes: You were learning a lot as a young man.

01-00:41:48
Alberts: Well, we learn from our mistakes. I made lots of mistakes! I give a talk about the importance of failure. Actually I have a short video talk, "Learning from Failure," in the iBioSeminars series. Do you know iBioSeminars?

01-00:42:02
Hughes: No, I don't.

01-00:42:03
Alberts: My colleague Ron Vale in the next lab decided, maybe four years ago, five years ago, that we should tape outstanding scientists who come to UCSF and make their seminars available around the world. iBioSeminars is a whole website, and my little talk on the points of failure is up there. Students like that. But it's not only *my* failure; you learn from other people's failures. So I was seeing the failure of competition. My colleagues and some of my fellow graduate students at Harvard were all competing unsuccessfully to publish these papers before anybody else on the genetic code. So you learn from failures of other people as much as your own!

01-00:43:13
Hughes: At Princeton you were presumably also teaching. How did that go?

01-00:43:17
Alberts: When I got to Princeton I had no grant because my grant had been delayed. It didn't get a high enough priority to get funded right away. It took a year to get grant money. My first year at Princeton was terrible, because they'd given me a year off from teaching so I could start my lab, but I had no money to start my lab. [laughter] So I had enough money to hire only one technician, and I hired a woman who was very nice but totally incompetent. After one or two

months I realized this was not going to work and I should fire her, but at that point she told me she had a brain tumor. She was getting dizzy and all that stuff, so how could I fire someone with a brain tumor? So what I actually did was avoid coming in during the day and let her do whatever she wanted to do, because she drove me crazy. And I came in at night and worked in the lab, but I couldn't do very much. Finally I think after about eleven months or so she said she'd just been re-diagnosed. She didn't have a brain tumor, she had hypoglycemia, so she could be cured.

At this point I immediately fired her, which was good, because I was able to hire another technician whose name was Frank Amodio. Frank wanted to go to medical school and didn't get in. He worked in my lab for a couple of years and then got into medical school. He was terrific, just the opposite of my first technician. My first graduate student was also a disaster. He flunked out at the end of his first year. He had come to graduate school at Princeton from a Catholic school, and he said he wanted to do molecular biology to understand religion, which was probably not the right motivation. But anyway, he wasn't good at science.

01-00:45:35
Hughes: Was he assigned to you or did you choose him?

01-00:45:39
Alberts: Nobody wanted to choose me, so he was the only guy that chose me. [laughing] So then Keith Yamamoto came to my lab and Glenn Herrick.

01-00:45:46
Hughes: Well, then you were home free.

01-00:45:48
Alberts: So Glenn and Keith came at about the same time, and they were both terrific. Glenn Herrick ended up being a professor at Utah.

01-00:45:59
Hughes: They were both working on—is it T_4 phage?

01-00:46:01
Alberts: Well, no. Almost all the T_4 work was done by myself and the technicians, first Frank and then a wonderful woman named Linda Frey. The graduate students in the early years took different projects, maybe because they thought T_4 was too risky. They chose their own projects. Glenn Herrick went looking for an analog of the gene 32 single-stranded binding protein in mammalian cells. Keith decided to work on steroid receptor proteins, which he's still working on with enormous success. I had predicted that they would bind to DNA cellulose because they move to the nucleus when you add steroid. In fact Keith's first paper was showing that the steroid hormone receptor would bind to DNA only when it had bound the steroid. So anyway, only after we had more success with the T_4 system did I actually get a postdoc working on it, Jack Barry. Jack, Linda Frey, I—Frank Amodio until he left for medical

school—got the system working well enough so that now other graduate students wanted to work on it, finding other proteins.

01-00:47:53
Hughes: Let's talk about teaching, because you had learned some things that you didn't like about how science was taught. So how were you teaching it, and what were you teaching?

01-00:48:04
Alberts: Well, I wasn't assigned to teach a course with, and what I didn't like about science at Harvard were all of the course labs. I was assigned to teach a required graduate course called "The Physical Chemistry of Macromolecules," which was also taken by advanced seniors at Princeton. For some reason, the three required courses for those graduate students were taught each of three quarters back to back, and the quarters were forty lectures long. I don't know why, but they decided that those core courses would each be taught in five lectures a week. So I had to give my forty lectures in eight weeks.

01-00:49:03
Hughes: You'd never taught this course, had you?

01-00:49:04
Alberts: No, I had never taught the course. [laughing] That was tough! It was my second year at Princeton. Now I had grant money, but when I had to teach this course I had no time to do anything but prepare the lectures. I learned a lot, actually. You learn a lot when you teach. As soon as I got back from Geneva I had another child. That's why we came back in October 1966. My son Jonathan,my second child, was born in December. By the time I was actually teaching this course, fourteen months after the one son I got another son! And so my wife had three young children, and I was supposed to be teaching this course. She decided after one week of it she would move to her mother's in Hawaii until I was finished with this eight-week course, because I was totally unavailable; this, as I now tell her, proves I was useful when I was there!

01-00:50:17
Hughes: You would look at the rosy side of it! [laughter]

01-00:50:17
Alberts: She claims I didn't do that much, but I obviously did something.

Anyway, that was a killer because I would be all weekend preparing my lectures, and I could get that done through Wednesday or maybe half of Wednesday, and then for my Friday lecture I'd be up all Thursday night almost every week. I started drinking coffee then. [laughing]

01-00:50:45
Hughes: The department socked you with the course because nobody else wanted to teach it?

01-00:50:47
Alberts: I think it had not been taught before, because they had just designed a new curriculum. But it turned out that there was a wonderful professor, the chair of the chemistry department there, Walter Kauzmann, who was a real expert in anything to do with the course I was teaching. He was enormously useful. But mostly I was learning, because half of the material I was teaching I didn't know. In the end it enabled me to do things in my research career that I wouldn't have done without a more quantitative background, that other people didn't have. But anyway, my first two years at Princeton were a real struggle. [laughing]

01-00:51:36
Hughes: How long were you there?

01-00:51:41
Alberts: I was there ten years in the end. [1966-76]

01-00:51:44
Hughes: Well, we have to move a little bit faster!

01-00:51:46
Alberts: Yes! We haven't even gotten to UCSF.

01-00:51:51
Hughes: Well, is there anything more to say about the Princeton years?

01-00:51:54
Alberts: Finally, it was very successful for us. By the end of 1976, we were replicating double-stranded DNA I a test tube with a mixture of T_4 proteins. I went on sabbatical to London [to Lewis Wolpert's laboratory, Middlesex Hospital Medical School, 1976] for six months to work on something completely different. While I was there, I got job offers from Harvard—from the same people who had flunked me out—and from UCSF. I decided to go to UCSF. So at the end of my sabbatical I came to UCSF [1976].

01-00:52:37
Hughes: Why? There was no comparison in those days between those two institutions.

01-00:52:40
Alberts: Oh, well, that's what my mother said. She had this thing about Harvard. That's why I went there as an undergraduate. She couldn't understand why I went to UCSF. But you could understand why—the same reason I didn't go to work for my father. I didn't want to be a young kid feeling like a student again among those famous professors. And the Harvard atmosphere was not nearly as appealing to me as UCSF, because Bill [Rutter] had really tried to set up a collaborative environment.

01-00:53:15
Hughes: And that appealed to you.

01-00:53:17
Alberts: Yes.

01-00:53:20
Hughes: I read that your friend and former student Keith Yamamoto put the bee in Bill's bonnet to try to recruit you. Is that actually true?

01-00:53:27
Alberts: Well, yes. Actually what happened is interesting. It must have been the summer of 1974. I gave a talk at a Gordon Conference and Gordon Tomkins was there. He wanted me to come to UCSF. He was a very personable and exciting guy. So actually I came and gave a seminar at UCSF, and I had a job offer from Bill and Gordon, but I turned it down because I decided I'd stay at Princeton. Then the next summer, 1975, I brought my whole family out to San Francisco for a couple of months to write a review article with Keith for *Annual Reviews of Biochemistry* on advances in biochemistry, about steroid receptors. And that's when I changed my mind about San Francisco, because I had never lived here before. It was very appealing. Then right after that I went to London, and then they came back to me with another job offer.

01-00:55:12
Hughes: Now was it San Francisco, the place, as opposed to liking the culture and the work that was going on in UCSF biochemistry?

01-00:55:22
Alberts: UCSF was so much different from Princeton. I had been enormously frustrated with the president of Princeton, Bill Bowen, because near the end of my tenure at Princeton I was department chairman, very young. I was acting chair [1973-74], really, and I was supposed to recruit the next chairman. We had two famous people in line who wanted to come to Princeton. One was Al Lehninger who wrote a famous biochemistry textbook and the other was Roy Vagelos who had not yet gone to Merck.

01-00:56:03
Hughes: Yes, he was at Washington University, St. Louis.

01-00:56:04
Alberts: St. Louis, yes, and he wanted to come to New Jersey because his parents were aging in New Jersey, which is why eventually he came to Merck. I had an incredibly frustrating time as acting chair. We'd decided as a department we wanted Roy to come, which was a very good choice. Roy tried to get some kind of commitment to having a small building for our department because up to that point half of us were in the chemistry building and half of us were in the biology building. We all had pretty crummy space, because we were the second-class citizens in those places. I was in the sub-basement of the chemistry building with no windows at all, only pipes. I had lots of sewage pipes overhead. I had a secretary half-time, and she had to sit in the hall because there was no office! It was very primitive.

But Roy was not willing to come without some kind of promise of a building, and the building that they had planned at that point was going to cost three point something million dollars. It eventually got built as a very small annex to the chemistry department. Roy kept going back and forth with Bill Bowen, and at that point the Princeton endowment was something like $500 million, and Bill would not promise to build this building unless Bill could raise the money. So Roy decided he couldn't deal with that kind of a president. And I was tearing my hair out. I was going to see Bowen and telling him how important biology and molecular biology were. I still remember what he told me. He said, "Everybody thinks their department is important. The German department, the French department. Go win some Nobel Prizes, and then I'll know your department's important." They were putting all their resources into physics because they had won Nobel Prizes in physics. Then much later Bill Bowen got religion, after we had all left. He got religion on biology after he had taken a sabbatical at Stanford and saw what Stanford was doing. But I had left; everybody else had left, basically.

01-00:58:39
Hughes: Not a good record.

01-00:58:41
Alberts: No. [Harold M.] Weintraub went to Seattle. Now the University of Washington has the Weintraub Building at the [Fred] Hutchinson [Cancer Research Center]. Arnie Levine went to Stonybrook and later became president of Rockefeller University. Abe Worcel went to get an Einstein professorship at [University of] Rochester. Uli Laemmli went back to Geneva. Eventually Marc Kirschner left.

01-00:59:08
Hughes: He went to UCSF, right?

01-00:59:10
Alberts: Yes, he came here after me. But basically the Princeton department more or less disintegrated. We had a great group of young people.

Audio File 2

02-00:00:00
Alberts: What do you want me to talk about now?

02-00:00:03
Hughes: The recruitment to UCSF.

02-00:00:11
Alberts: So I was recruited by Bill and Gordon. I didn't know that Gordon was going to die, of course. So I thought I was coming to be with Bill, Gordon, Keith. I didn't really know the others, Reg [Regis B.] Kelly and Chris[tine Guthrie]. I knew them a little bit, but of course I got to meet them. I knew all these people a little bit from scientific meetings, but I wasn't close to them. Gordon

ran a huge, chaotic lab like Paul Doty's, and I didn't think he was running the lab very well. But he was such a charismatic person, just a very warm—

02-00:01:11
Hughes: And an idea-a-minute person.

02-00:01:12
Alberts: Yes, right. And some of them crazy. Keith would say, the problem was that Gordon liked *every* idea! The good ones and the bad ones. So he wasn't that good at running a lab. He wouldn't direct people.

02-00:01:34
Hughes: Well, how was it for you starting up your lab there?

02-00:01:35
Alberts: Here [UCSF], you mean.

02-00:01:36
Hughes: I'm thinking of the Parnassus campus. [This interview took place in Alberts's office at the UCSF Mission Bay campus].

02-00:01:42
Alberts: So I came here, basically directly from my sabbatical in London. And I moved one person. He had been my former postdoc, had gone off to get a job as an independent scientist and had not been successful. His name is Jack Barry. He came to help me set up. It was very important. Actually he did a great job. My research was already going very well. It was pretty straightforward what to do next, trying to figure out more details of the replication process. So it was nothing like settling into Princeton. It was a very small group of faculty. You've probably interviewed Keith about that. [As of 2012, a long series of interviews recorded in 1993 await Dr. Yamamoto's review and approval.] We used to have a meeting in a little conference room on the ninth floor. We'd meet there every week for a couple of hours and talk about somebody's science. Jim [James A.] Spudich was there. There weren't more than like eight or nine people in the room. Of course the department had many other members, because Bill had come here into a pre-existing department. But Bill somehow managed to create two departments. [laughing] He ignored the rest.

02-00:03:35
Hughes: Well, he also slowly got rid of them.

02-00:03:36
Alberts: Yes. So there was an in-group and an out-group. And I didn't really interact much with the rest of the department. Bill sort of redefined who was actually in the meaningful department.

Maybe the first weekend I was here, Herb Boyer, who I'd met previously, invited my wife and me over to his home for lunch on a Sunday. And then he wanted to take a walk and he offered me a— He said, do I want to help him with Genentech? Not to move to the company, but to help him. And of course I said no. [laughing]

02-00:04:21
Hughes: Well, I want to hear what you thought about commercialization at that point.

02-00:04:26
Alberts: Well, you've probably heard all this. Basically, the whole culture of academia was against making money. If you wanted to make money then you weren't a serious scientist. That was the extreme version. Even as an undergraduate at Harvard it was the same thing. Anybody who wanted to go to the business school, we thought they were deranged or unethical almost. Not unethical but not intellectual. So part of the idea of academia was it was sort of pure knowledge for knowledge's sake.

That was not the main reason why I didn't want to help Herb. He asked me to be in some kind of advisory role. I just felt I was so far behind, and as usual I had no time. It wasn't that interesting. I didn't think I knew anything about it. So it wasn't at all appealing to me. If he'd told me I would have made huge amounts of money maybe I would have changed my mind! But none of us thought Genentech was going to go anywhere anyway.

02-00:05:51
Hughes: He was offering stock?

02-00:05:54
Alberts: Well, I assume that's what I'd have gotten. I really never got that far. But if he offered stock, I would have assumed it was going to be worthless!

02-00:06:05
Hughes: Yes, well, most people did. How wrong they were.

02-00:06:11
Alberts: Yes. But then, as I tell my wife, everybody would be after my money like they're after Herb's. And they'd be mad at me because I didn't give them enough, or I didn't give it to them, I gave it to somebody else. Lots of disadvantages to having too much money. [laughing]

Anyway, the early days in the department were exciting. It was a good place to work. I didn't have as much teaching as I had at Princeton. What I taught was the same stuff that I had taught at Princeton but much less of it. It wasn't until we started the cell biology course that we really started to create something that I thought was unique. I can't remember exactly when we gave that first cell biology course, but that was a critical event. I had been working on a cell biology book meanwhile. So I was thinking a lot about cell biology. But we all noticed that there were all these small courses called cell biology in different departments at UCSF, and none of them were that interesting from the point of view of molecular and cell biology, which was the new field that our textbook was dealing with.

So a bunch of us got together, and I can't remember all the people. Certainly it was Marc Kirschner—so Marc was already here—me, probably Reg Kelly. Anyway, this went on for many years. I can't remember who taught the first year, but basically we decided we'd give this cell biology course for graduate students. But so many postdocs were interested in it that we gave it at seven o'clock at night. It was in that big auditorium on, I guess it was, the fourteenth floor. It's probably not there anymore. And it was attended by faculty, by postdocs. The room was almost full every time. We all worked together. We all went to each other's lectures. It created an intellectual group around modern cell biology. Again, whenever we were teaching it we were learning some of it ourselves. So that was exciting. That I think was a precursor to the PIBS [Program in Biological Sciences] program.

02-00:09:04
Hughes: Yes, I was trying to remember when PIBS was started.

02-00:09:04
Alberts: Probably it was about '83, something like that.

02-00:09:11
Hughes: Was it that early?

02-00:09:15
Alberts: Well, the precursor to PIBS. PIBS didn't get founded till we had the [Lucille P.] Markey [Trust] grant.

02-00:09:26
Hughes: Which was about 1989.

02-00:09:27
Alberts: I don't remember. If you look at Rudi Schmid's oral history which I have here somewhere it has a chronological table. Actually Henry Bourne's book, [*Paths to Innovation: Discovering Recombinant DNA, Oncogenes, and Prions, in One Medical School, Over One Decade*] has it also.

02-00:09:38
Hughes: Were you the spearhead in all this? Because it seems to me it reflects what you tell me that you'd been learning, i.e. the importance of collaboration, cross-fertilization, that sort of thing.

02-00:10:02
Alberts: You know, I can't remember. I'm sure it was more than me. But Marc and I had both been at Princeton where we had had a wonderful program. The precursor to PIBS [Program in Biological Sciences] was a Saturday three-hour, once-a-week course that was called Special Topics in Biochemistry, except that it had a focus on cell biology. In that special topics program, two or three faculty members would get together about something they wanted to learn, and we would give it together three hours every Saturday morning for maybe a month or something like that. It wasn't a full course. That had been such a stimulating experience for us all and it led to several publications. So

that was clearly the model that we had in mind for UCSF. We said, look, we have these cell biology courses that are not really modern or exciting, and they're all duplicative of small courses. Why don't we just give one and try to do it the way we did the special topics, with multiple faculty teaching together?

02-00:11:26
Hughes: Was this PIBS, or was this the precursor to PIBS?

02-00:11:29
Alberts: That was the precursor of PIBS, I think. You'll have to look at exactly when that was. I can find it. I have my lecture notes. I actually kept a lot of my old lecture notes.

02-00:11:46
Hughes: Well, PIBS now has almost iconic status. But I'm suspecting, as with almost anything in academia, that it took some persuading to get it launched.

02-00:12:02
Alberts: Yes.

02-00:12:03
Hughes: UCSF was becoming a more flexible place, but I imagine that the departmental walls were still pretty high, and you were trying to break them down.

02-00:12:20
Alberts: I think I was chair of biochemistry by that time, because Bill had already gone to Chiron.

02-00:12:24
Hughes: Yes, you become chairman in 1985.

02-00:12:28
Alberts: Yes, so I specifically remember the big debate, because we already had an outstanding graduate program. That's what universities do over and over: let's form an interdisciplinary PIBS program, but let's keep our old program because it's so wonderful we don't want to give it up. So I did play a major role in persuading the Biochemistry and Biophysics Department faculty to give up the old program. I still remember arguing with some specific faculty members who objected why this was important, because otherwise the new program wouldn't have flourished. I had known of other interdisciplinary programs at universities that had departmental programs, and they were always second-rate. They didn't get the attention of the top faculty.

02-00:13:22
Hughes: The Department of Biochemistry was so much the kingpin in those days that there probably was a little resistance from other departments. They may have thought, what are these power figures going to do to our department if they take over our instruction?

02-00:13:37
Alberts: You'll have to ask people from other departments how they felt about it.

Mike Bishop ran PIBS. It was very elaborately set up as I remember. I think the rule was each faculty member could be in no more than two programs. A lot of the rules have fallen apart now, because people forgot. In order to be in that program you had to contribute to the teaching. And if you weren't doing that then you were thrown out, and you couldn't take graduate students from that program. So the penalty was not being able to take graduate students.

02-00:14:20
Hughes: Why didn't they want you in more than two programs?

02-00:14:23
Alberts: Because you can't possibly contribute adequately to more than two.

02-00:14:25
Hughes: Yes, you were spread too thin.

02-00:14:27
Alberts: Right. And now we have people who *are* like that. So people would join any old graduate program in order to have access to graduate students. That's a great benefit, but in order to earn that you had to contribute to their training, and that was the idea. And then there were these councils that basically would review every few years who should be members. They were generally lenient. If an outstanding scientist wanted to join, there was a recruiting tool to join. They'd let that person join but then they would look later as to whether they actually did anything. In fact I still remember we threw out Y.W. Kan because—[laughter]

02-00:15:13
Hughes: Because he didn't do anything?

02-00:15:15
Alberts: He didn't teach, yes. Y.W. hates teaching, actually. I don't think he feels he's very good at it.

02-00:15:22
Hughes: What about Bill Rutter? By then he was off at Chiron?

02-00:15:27
Alberts: Yes, Bill wasn't even full-time at the university anymore.

02-00:15:32
Hughes: And he was never interested in teaching anyway.

02-00:15:32
Alberts: No, he wasn't, not very.

02-00:15:36
Hughes: Well, I think he even said in his oral history that he felt the first thing the department should do is shine in research, and that teaching would be second.

02-00:15:48
Alberts: Yes, that's the Harvard tactic.

02-00:15:52
Hughes: It seems to me you almost turned it around.

02-00:15:56
Alberts: Well, my feeling is that by doing things like the cell biology course we greatly improve research, because we've got people talking to each other, thinking much more creatively by teaching together.

02-00:16:05
Hughes: It must have been a great recruitment tool, particularly for students, to have access to all this brain power.

02-00:16:11
Alberts: Yes, sure, then all of a sudden we were getting all the students from Berkeley, and so Berkeley had to change. Everybody had to change. It was a major recruiting tool. Harvard did it when Marc went there.

Basically it helps when you're not very successful as an institution. When you're behind you have to do something different. So that's why Harvard doesn't want to change its culture because they think they're already successful.

We thought some of our strategic advantage was creating a collaborative culture at UCSF. I think that was part of it. In fact I still remember when we were doing recruitments, unlike Harvard who just wants research stars, we wanted people who were known to be interested in the community and in the department and would contribute. There are some very famous scientists that we didn't offer a job to for that reason. So that was intentional, to try to hire people into that community, and people who would be willing to work for the community not just for their own research.

02-00:17:52
Hughes: And you were largely successful in achieving that?

02-00:17:54
Alberts: I think so, yes. We also had a rule when I was chair, for the same reason. If you have a huge lab you have no time to contribute to the university. You're just running your own business, and you're using up lots of resources at the university. So we had a rule—no more than twelve people in your lab. I would go around trying to inspect people like Bob [Robert M.] Stroud who never could count. [laughter] I said, "What are these sixteen people here in the room?" "Oh, they're all visiting." I still remember that.

02-00:18:32
Hughes: What about the space problem? In Microbiology when Herb Boyer rose to fame everybody wanted to come and learn recombinant DNA technology. Herb Heyneker describes in his oral history how Herb welcomed him the first day he came to UCSF, and Herb Heyneker said something like, “Where am I going to work?” Herb Boyer picked up a board and put it over the sink and said, “Here you go.” [laughter] But it wasn’t really that bad, was it, in Biochemistry?

02-00:19:30
Alberts: No, we were better off. Bill had gotten space. But you couldn’t easily get that—that was part of the reason to have small labs; it wasn’t the major reason—pressure on space. The major reason was that if you want to be part of the community, you can’t be running a twenty-five person lab. Howard [M.] Goodman had a huge lab. He left. So it was good that some people left. He was also not a good citizen. He and Bill were fighting all the time!

02-00:20:03
Hughes: Goodman and Herb Boyer fell out too. That was pretty much over by the time you became department chair, wasn’t it?

02-00:20:16
Alberts: Yes, Howard Goodman had left.

02-00:20:19
Hughes: Yes, Howard Goodman had left for Harvard.

02-00:20:26
Alberts: We had a pretty convivial group by that time. By the time I was chair all the old people had either disappeared or they had found other more convivial places to join. They weren’t really a problem of mine. I didn’t have to deal with removing people.

02-00:20:49
Hughes: Tell me why you agreed to become chairman.

02-00:20:53
Alberts: Well, that’s a good question. [laughing] Early on Bill made me vice chairman. I’m not sure why.

02-00:21:03
Hughes: He probably wanted your teaching expertise.

02-00:21:05
Alberts: Yes, maybe.

02-00:21:08
Hughes: And other things.

02-00:21:09
Alberts: I had already been chair at Princeton, you remember, acting chair. So I was an obvious person to be chair.

02-00:21:16
Hughes: Who else would there have been?

02-00:21:20
Alberts: Well, Keith [Yamamoto] could certainly have been chair.

02-00:21:23
Hughes: He was pretty young.

02-00:21:27
Alberts: Yes, he was. Reg Kelly could have been chair. You'll have to ask why was I selected. I have no idea. Why did I *take* it?

02-00:21:40
Hughes: Yes, why?

02-00:21:42
Alberts: That's a good question. That's a harder question. [laughing]

02-00:21:45
Hughes: You were already really deep in a lot of things, including your research.

02-00:21:48
Alberts: Yes, I'm generally a sucker for these things. As you can see, my later career I keep on taking on these things. I thought it would be good for UCSF and good for science. They certainly didn't offer me anything—there was no perk for taking it. I didn't get money; I didn't get lab space; I didn't get anything.

02-00:22:05
Hughes: You didn't get money?

02-00:22:12
Alberts: Well, maybe I got a small raise, but that wasn't really the reason.

02-00:22:14
Hughes: I'm just looking at the date of the first edition of *Molecular Biology of the Cell*, and that was 1983.

02-00:22:27
Alberts: Right, and we started working on it in 1978. That was another whole struggle.

02-00:22:36
Hughes: You already had a lot on your plate, and then taking over as chairman. Biochemistry was not just any old department at UCSF; it was kingpin. Maybe the other departments were beginning to catch up.

02-00:22:50
Alberts: We helped them a lot by making joint appointments when I was chair. We made joint appointments for the Jans. That was a major recruiting tool. Yuh Nung and Lily Jan. Joint appointments for Cori Bargmann and joint appointments for Marc Tessier-Lavigne.

02-00:23:17
Hughes: Was that a new policy? Had Rutter made joint appointments?

02-00:23:22
Alberts: He must have done, yes. He gave Herb [Boyer] a joint appointment eventually. Maybe to keep Herb here. But what we didn't notice—it's funny—is the three people I mentioned all grew their labs to be huge! We didn't control the space they were in—anatomy or physiology—so that whole tradition of twelve-person labs got ignored by the joint appointees.

02-00:23:59
Hughes: And I guess you didn't have enough control—

02-00:24:04
Alberts: Well, we didn't have any control. We didn't even have knowledge of it.

02-00:24:10
Hughes: Were joint appointments a good route to go? It would seem to feed into this collaborative policy.

02-00:24:17
Alberts: Oh yes, it helped a lot. Those people are all stars. They're all recruited [away]. Cori's at Rockefeller now. Marc Tessier-Lavigne is president of Rockefeller after going to be head of research at Genentech. And of course the Jans are spectacular scientists, both members of the academy. I think their being in those departments then raised the quality of their future recruitments, so it was a very important step for UCSF.

02-00:24:47
Hughes: Did you have a say in who was recruited?

02-00:25:01
Alberts: Oh, sure. Whenever we did joint appointments of course we had a lot of say. In fact I personally recruited both Marc and Cori because I read about Cori in the book [*Natural Obsessions*, 1988] by Natalie Angier about Bob Weinberg's lab. I was at MIT on their visiting committee, so I asked to have lunch with her. She was supposed to be the perfect graduate student. I don't know if you remember that. And then Marc—I met him at a Gordon Conference. A friend of mine, Martin Raff, at University College, London, had known him as a postdoc. He had worked with him, and he thought he'd be spectacular. So I recruited him at the Gordon Conference, and actually many other people that came here—Ron Vale. Martin Raff told me about Ron Vale as well! [laughing] So that's the way you recruit. Erin O'Shea—I met her as a graduate student at a Gordon Conference. It was a much smaller community in those

days. The way to find out who to hire was to call all your friends, and ask who was the best person they'd seen at their university.

02-00:26:19
Hughes: What was the chairmanship doing to your research productivity?

02-00:26:26
Alberts: [laughing] Well, that plus everything—the textbook—that's a good question. I think my lab was doing pretty well. We got into a new subject. I didn't talk about that at all. I told you that Keith Yamamoto started his steroid hormone receptor project in my lab. Subsequently I had a couple of postdocs work on it, but then it died out.

But a much more profound transition was when a woman by the name of Victoria Foe came to my lab. I put her up in my lab because she seemed like such a wonderful person. She needed a space to work because her boyfriend, and later her husband, was a professor here in a different department. Anyway, she was doing control experiments for a dream experiment of her own design. She wanted to do her own thing. A control experiment she needed required that she look at early *Drosophila* embryos and carefully time every nuclear division. That was supposed to be three months of work and then she'd do her real experiment. Well, it ended up that she worked on that for probably four years. We published a giant, nearly hundred-page paper, now a classic—Foe and Alberts characterizing the early division stages of *Drosophila* embryo. Having that going on in my lab, we got very interested in the cell biology of how this was possible. How could the nuclei do this elaborate dance inside the embryo, march towards the surface exactly in step, and arrive at the plasma membrane one and a half minutes into cycle twelve? That told us that the cytoplasm of a cell is much more highly organized than we had thought at the time.

So then I started what became the major project in my lab before I left for the academy in 1993, which was basically redoing the same thing we had done for DNA cellulose—made an affinity column out of cytoskeletal polymers: one out of microtubules, and a second one out of actin filaments. I had all these graduate students and postdocs doing that stuff, finding all the proteins that bound to the columns. And it was incredibly surprising, because at that point everybody thought there were maybe ten proteins that interacted with microtubules, and ten proteins that interacted with actin filaments. In fact we found over a hundred in both cases. And we had a hard time publishing it because people didn't believe us at first.

02-00:29:34
Hughes: Where was the hundred-page paper published?

02-00:29:39
Alberts: *Journal of Cell Science* (*61*:31-70, 1983).

02-00:29:42
Hughes: I didn't know the journal took something that long.

02-00:29:42
Alberts: That's the kind of journal that publishes those. [Added in transcript: In fact, I now see that it was forty pages in print, so I am remembering the typed version that we submitted.] That paper has more citations than any other of my publications—except for my book.

02-00:29:57
Hughes: But you were able to keep all this going.

02-00:30:01
Alberts: Well, basically you have good people in your lab.

02-00:30:03
Hughes: Well, yes, but they still take some monitoring. And I imagine a lot of the ideas were coming from you.

02-00:30:12
Alberts: Well, obviously I think my lab would have done better had I not been doing so many other things. I was away a lot, writing the textbook. It took a tremendous amount of time, much more than being department chair.

02-00:30:26
Hughes: You said you started on the textbook in 1978. Just you?

02-00:30:33
Alberts: No. This was Jim Watson's idea. Jim Watson had recruited a team of authors, I think in 1977, to work on a textbook. He had exactly the right vision, and it resulted in all those things up there—all the different languages [pointing to a shelf with numerous translations of *Molecular Biology of the Cell*].

02-00:30:50
Hughes: How many translations have there been?

02-00:30:53
Alberts: Oh God, I don't know. Over ten. So basically he had the vision. It has sold over two million books so far.

02-00:31:09
Hughes: Is there any science text that beats that?

02-00:31:09
Alberts: Oh there must be. I'm sure the textbooks for introductory biology do. But basically he had—exactly as Jim often did—the correct vision that at that point it was the time to bring together two separate fields—molecular biology, which he had worked in, which had been developing rapidly, and cell biology, which was a completely different kind of field at that time. It was a classical field in which you looked at cells through microscopes, first light microscopes and then electron microscopes. And you wrote up textbooks of what you saw

in the microscope. In fact I still remember in my sub-basement chemistry lab at Princeton, trying to figure out what cell biology was and ordering a little textbook which I probably still have, and trying to read this thing—it was so foreign. I hadn't learned essentially anything of that stuff in my own career. But at any rate, obviously Watson knew more about cell biology than I did, and he said that now was the time that we should try to explain all this microscopy stuff in terms of molecules. So this was the time for a new textbook because that would be the way that we would start to drive the field. Previously Lehninger's *Principles of Biochemistry* had done the same thing for biochemistry. Before Lehninger's *Biochemistry*, biochemistry was much less powerful, not nearly as much taught as it became afterwards.

So he got a bunch of us together. I wasn't in the first group of authors. A guy at Cold Spring Harbor, Joe [Joseph F.] Sambrook, was supposed to do the nucleus stuff. Joe, for some reason, got tired of it. He was a scientific director at Cold Spring Harbor where he met Jim. For whatever reason he decided that he was going to drop out after maybe a year. They hadn't really written anything. They'd just had meetings. So I suddenly got a phone call. I think it was the spring of 1978, from Jim Watson and Martin Raff. Martin Raff was at Cold Spring Harbor. He's normally in London. And they asked me if I would like to work on this textbook, and we were going to have a meeting the next summer, which we did. It was at Jim's private home in Martha's Vineyard for something like a month. But the selling point, as usual with Jim, was this is not going to be very difficult. [laughter] It would take this first summer month then one more month in the next summer. That was basically the idea.

02-00:34:12
Hughes: Which I hope you pooh-poohed.

02-00:34:15
Alberts: Well, no—well, I didn't know! I knew Martin Raff, I'd met him. I knew him from my sabbatical in the UK, and I knew Jim very well from my time at Harvard.

02-00:34:28
Hughes: And you got along with him?

02-00:34:30
Alberts: Yes. I had gone to his group meetings. Jim is remarkably accessible and interested in young scientists. So I was going to his group meetings in addition to Paul Doty's group meetings. I often would go talk to him and ask something. Before I went for my postdoc and when I failed my exam I went to talk to him about what I had learned and to get his advice. So at any rate, basically he was appealing to us to try and take up a challenge that obviously was interesting, and to work with those people who I knew were very good people. But it took us so long to learn how to write that book—we failed miserably at first.

02-00:35:19
Hughes: How did you divide up the task?

02-00:35:22
Alberts: Well, the original idea was that each of us was going to draft a chapter or two chapters and the rest we would merely edit after chapters had been drafted by outside experts. And then there were all these other chapters—the book is only half as big as it was supposed to be. Jim had contracted with the publisher for all these people to send in manuscripts on different organisms. It went on and on and on. Well, we got all those drafts, but they were not usable. The second summer is when we got all that stuff. That's when it got depressing, because then we realized we had to write it all ourselves. In retrospect, it was hard enough for us to learn how to write this kind of text. It's not like writing scientific papers. And to expect outside people to be able to do this, even when they're experts—experts know it too well. They can't write at this level. So almost nothing of what we got, that came in, could we use.

02-00:36:27
Hughes: But you could use it as a basis for the factual part, could you not?

02-00:36:31
Alberts: Well, we did use outside people. I got assigned to write the chapter on internal organelles—the endoplasmic reticulum and the Golgi apparatus. I'd never heard of them before! [laughter] So I did a lot of reading. But most importantly, I had George Palade who was an expert to advise me.

02-00:36:45
Hughes: I know that name.

02-00:36:50
Alberts: I'd send him a draft. He's from Romania. I would make up all these theories about how an organelle might work, because we didn't know much. So I was trying to do molecular speculation, I guess. So he'd write these charming things like, "Oh, this is very interesting, but you may want to know the following facts." [laughter] And then he'd lay out ten points very clearly, ten facts that I did not know, to show that what I had written was complete nonsense. He would never say that you wrote complete nonsense, however.

02-00:37:26
Hughes: He was too polite.

02-00:37:31
Alberts: So it was a great, like teaching my physical chemistry and macromolecules course, where I learned a lot of physical chemistry that was relevant to my future research. Like teaching cell biology here, I learned a lot of cell biology.

The strategy we adopted starting from that first summer was that Martin Raff and I would each focus on preparing one chapter until we got it right, and we got feedback until we each had a prototype model chapter. That was a good strategy. We were supposed to write things we knew, so I was writing the biochemistry chapter, and he was writing the immunology chapter because he had been an immunologist. So I would draft stuff and give it to him, and he would draft stuff and give it to me. He knew no chemistry; I knew no immunology. It was useful because what you wanted was a naive reader. And I think through that process we eventually learned how to write in a different style. If I pick up an immunology paper, for example, in the standard scientific literature, it doesn't mean anything to me! It's all gobbledygook. Experts write with so much background knowledge that they can't clearly express themselves, often, for students. So Martin and I were great for each other, because he knew almost no chemistry. He was an MD. And I certainly knew no immunology. So we asked very naive questions and it was very useful. [laughing] And again, I learned a lot from writing that chapter and he learned a lot from writing his chapter.

02-00:39:40
Hughes: How did you visualize your audience?

02-00:39:47
Alberts: It was supposed to be for advanced undergraduates. I guess I visualized Martin as my audience, really, because he was no more educated about that topic than an advanced undergraduate. It got to the point where Martin would make excessive changes on my chapter, and usually at the beginning I was sort of checking that they were okay. But after a while I just let him make the changes. His wife was doing all the typing. She was getting paid to be a typist. She was a very good typist, a professional. Her main job was being a secretary. But it got to the point where I would just let it be typed in and then I'd fix it later, because I could read it much better, and it was so little that needed to be changed. I think he was doing the same thing for my corrections.

So really Martin and I started this thing. The third key person at the start was Keith Roberts, whose job was mainly at that point to draw those wonderful figures. Keith is very interesting. He's a plant cell biologist, and he eventually would write a lot. But in the beginning he was just doing the drawings. He had come as a seventeen-year-old high school graduate to work a year as a technician in Jim Watson's lab at Harvard. I don't know how in the world he got there. He came from the UK. He must have had a fantastic recommendation from somebody. Jim Watson was writing the *Molecular Biology of the Gene*. As Keith tells it, Jim's secretary was supposed to be drawing the figures. I guess she knew something about drawing but she didn't know any science. One day he passed her and she was crying, and it was because Jim had just told her how bad the figures were! So Keith started drawing them, and he actually is the guy who developed all this whole cartoon style that spread everywhere through all these textbooks. He's very artistic.

That first summer, 1978, it was really me, Keith, and Martin. We were doing those two chapters and Keith was trying to illustrate them. That was a lot of fun, watching what Keith would do. We'd give him some horrible scribbled idea and he'd make it really nice and more conceptual.

02-00:42:33
Hughes: How fortuitous, this seventeen-year-old talent!

02-00:42:37
Alberts: That's right. What happened is that not only did Sambrook drop out, but other authors who were on the book were fired by Watson. Eventually we took in one of the contributors, Julian Lewis, who turned out to be a wonderful colleague. But subsequently they've tried to reproduce this model in publishing by bringing in groups of people to work together and most of them have failed. It's not easy to get people that can work together that way.

02-00:43:12
Hughes: I thought these authors were all on the first edition—Alexander Johnson?

02-00:43:22
Alberts: No, not on the first edition. Those are all brought in. The first edition I have right here. Let me get it. [Taking book from shelf] Sandy Johnson is a UCSF faculty member, as is Peter Walter. I'll tell you how we brought in those kinds of people. It was the Watson book, okay? But actually Watson wrote very little in the end. He didn't have the patience for the kind of thing we're talking about. He was great at the first drafts but he didn't like revising. So there is Watson and Julian Lewis, who brought us a wonderful chapter, the only one written outside that was wonderful, on developmental biology. We made him an author, and he's been an author from the first edition till now. Martin Raff of course. And there is Dennis Bray who replaced several other authors, including some quite famous people, who in series had failed to write the first chapters. Bob Goldman was supposed to be writing the first chapter of the book, but he couldn't write things that we were happy with. Then this very famous senior cell biologist, Keith Porter, was brought in in the summer of 1979 to write this first chapter. I don't think he actually wrote anything. He would come and bring us text that he said was written by "the boy". I had no idea who "the boy" was. [laughter] It was horrible. I can say this honestly because Keith's now dead.

02-00:44:53
Hughes: And Bray?

02-00:44:57
Alberts: Then we brought in Dennis Bray, and he was brought in the last because we needed somebody to write the first section, and Dennis writes at this level very well. He's written several books since, so he did a lot of the introductory material and editing.

02-00:45:12
Hughes: So it's Watson, Alberts, Lewis, Raff, and Bray.

02-00:45:15
Alberts: Right. Every edition we continued to bring in people to write. We'd pay them to draft chapters. I think we'd generally have four different people writing different chapters, and we could never predict who was going to be successful. Because like Watson, some people who were brilliant at writing the first draft didn't want to have anything to do with revising. They were all terrific scientists, because we were very selective who we asked.

02-00:45:56
Hughes: But those two things don't necessarily go together—terrific scientists and terrific writers.

02-00:46:02
Alberts: Well, for this level of book you have to be a pretty good scientist, because you have to know what's important, and that's not easy, because most of what's important—

02-00:46:06
Hughes: I was thinking of it the other way around.

02-00:46:09
Alberts: —is what you leave out. The reason why our textbook is more successful than others—we try not to put anything that's not really important, including facts that aren't necessary. Alexander Johnson and Peter Walter, both at UCSF by the way, were two of the most successful authors. Walter not only wrote reasonable stuff, they took criticism well, and they revised. [indicating editions on the shelf] So they became authors, but they weren't authors even on the third edition. See, the same authors went through three editions, then the fourth edition. But in the third edition they each drafted a chapter, which they got paid for. They're especially acknowledged in the preface. But they only came in as authors in the following edition. The new edition that we're doing right now, we're bringing in David Morgan from next door as an author. We're all getting old! [laughter] He had drafted chapters in the earlier edition.

02-00:47:14
Hughes: That will be the fifth edition?

02-00:47:14
Alberts: The sixth.

02-00:47:27
Hughes: I went online and what comes up is *Molecular Biology of the Cell: The Problems Book*.

02-00:47:34
Alberts: Oh yes, that's a separate book. So we brought in two people to write that.

02-00:47:47
Hughes: But that's no relationship to—

02-00:47:48
Alberts: Well, there is a relationship. They write problems for each of the chapters. They come to these book meetings, and John Wilson and Tim Hunt sit with us. Tim Hunt's a Nobel Prize-winning scientist. John Wilson has written many textbooks. They write all these problems because we realized that we were doing students a disservice, because in order to explain all these things we had to leave out most of how the experiments were done.

02-00:48:12
Hughes: I see.

02-00:48:15
Alberts: We decided that we should have a problems book that actually had students deal with the kind of experiments that developed the knowledge that's in the book. And I must have a copy of the problems book here somewhere. I have several of them. [John and Tim] look at what's in each chapter, they have our chapter, and they say let's make five problems around this section of this chapter, and then the next section we'll have another five problems. The problems are at different levels. Some of them are easy, but the most famous ones are quite hard. Lehninger's book, for example, has a problems book. It's commonly done in these advanced courses.

There's also *Essential Cell Biology*.

02-00:49:05
Hughes: Well, that was one of my questions.

02-00:49:06
Alberts: We had a wonderful person who worked for Garland Publishing, Ruth Adams, who was sort of responsible for getting the book together. She kept on saying, because she was not a scientist, we need a dumbed-down version. [laughing] The first edition Keith Roberts kept track of the time. I would go to England; it was almost all done in England, some in Cold Spring Harbor for the summer, but mostly in England. We'd go for one month at a time, over Christmas, over Thanksgiving, so that first edition we were together more than 365 days—isn't that amazing? We're working twelve to sixteen hours a day. We would all sit in the same room. We were so overwhelmed with just getting the book out that we couldn't think of producing a dumbed-down version. But we now have three editions of the small book.

That again was an enormous learning experience. After we finished an edition of the big book, the authors would have some years where we don't do anything. So we decided that this was the time to do the dumbed-down version. And again, we thought it would be much easier than it was. We thought we could just take the most essential parts out of each chapter, that's what we did, and then paste it together and then write a few connecting things. We did that and it didn't work because at a lower level you need a different way of writing than what we were used to. It's a different audience. The way we found it didn't work is that we actually had students read it and then talk to us. We met with all these students whom we paid to read the book. They were actually taking a course, but they read the book, and then they talked to us about what they didn't like and what they didn't understand. That whole thing was organized by a woman who was very important to the book, Miranda Robertson, who is a professional science editor. She worked at *Nature*. She works at another company now. She's a wonderful writer and helped us write this book. In addition to Martin and me marking up each of our own chapters, after we got done with it she would go over it and almost rewrite it! [chuckling] We don't need her that much now; she doesn't work for us anymore. But in the early days we needed her. She was the one who organized the *Essential Cell Biology* book by getting these students together and figuring out what they didn't understand, and that was critical.

So it ended up we had to write the little book almost from scratch, just a different language, much more explanation. The little book has only 25 percent of the number of words as the big work. It's deceiving because it's bigger type. But I think it's a wonderful book. It's actually now selling more copies than the big book, because cell biology has gotten to be so complicated. It's overwhelming. If I take one of the more complicated chapters in the big book, even though I've read it probably fifty times—say the one on cell signaling with all these facts—after I haven't read it for a year I can't remember most of it. Actually, the best way to get the conceptual framework would be to read the analogous chapter in the small book and then read the big one. When you're reading *Essentials* you can keep it all in your head.

02-00:54:18
Hughes: Well, and then you have the problem of science moving on and having to update.

02-00:54:25
Alberts: So the problem we have now is that there's so much in cell biology. You see how thick the red book is? Those are other languages, but I have the red one here somewhere in English. Well, we cheated the last time we did the red book, because we put the last five chapters on a DVD inside the book so it wouldn't be so big. [laughter] But we got feedback from the faculty who said it's too much. We had covered things sort of in the same way we've been doing: every year we try to include all the most essential facts and leave out everything else. But it's gotten just too much for a course. This new edition, the sixth one, our most difficult task is cutting back to the size of the previous (4^{th}) edition. I would say that almost the most important thing that textbook writers can do is to really find the essential core so that somebody who doesn't know anything can actually get their head around it. For us who are scientists, we have no conception of how hard it is to learn this stuff. What's wrong with most textbooks is that they're written by experts who have no conception that what we know is just overwhelmingly complex for beginners.

02-00:55:42
Hughes: One last question for today. What was your competition? I've been trying to think of the title of the book for which [David] Baltimore is one of the authors.

02-00:56:02
Alberts: Yes, [Harvey] Lodish is the first author. We're called *The Molecular Biology of the Cell*, and they're called, I think, *Molecular Cell Biology*. It's a similar effort. It started much later than we did.

02-00:56:15
Hughes: You didn't consider that competition?

02-00:56:18
Alberts: Well, there are lots of competing books now. The first edition was completely new. It was read cover to cover by many outstanding scientists. It was bought by all these scientists because they didn't know this field. That's not true anymore. The textbook is now mostly for students. There are so many more resources. There are all these review articles, and you get them on the web. What were we saying?

02-00:56:56
Hughes: I was just asking about competition.

02-00:57:01
Alberts: Oh yes. You'll have to look up when the first edition of their book came out.

02-00:57:07
Hughes: It was 1983.

02-00:57:07
Alberts: They saw the success of our book, and they decided they were going to do the same thing. I think Harvey Lodish at MIT, a very energetic guy, was the organizing author. They've had probably the same number of editions we have. They seem to turn them over more quickly, but it's a very different book.

02-00:57:23
Hughes: How, in a nutshell?

02-00:57:29
Alberts: My feeling is that it's good because it's written by researchers, so it's not rehashed like high school textbooks, which are written by people who don't know anything about the subject. But I would claim that they put too many facts in and the figures are too complicated. They haven't really taken enough consideration of the students, and for that reason I think our book is more successful.

02-00:58:01
Hughes: Does it sell better?

02-00:58:01
Alberts: Yes. All these statistics are secrets. Nobody publishes how many they sell. Garland [Science] and other companies that sell books in this market call the professors and offer them free samples and ask them what they've been using. Telemarketing it's called. So we have a whole telemarketing group that's excellent, in Connecticut. So they actually know how many have sold because they've talked to all the cell biology professors in the United States. They know how many copies, roughly, of each textbook is being sold. So that's how we know that our textbook is dominant still. Both of them are.

02-00:58:48
Hughes: Well, I think we should stop for today.

02-00:58:51
Alberts: Oh great!

02-00:58:54
Hughes: You must be exhausted!

02-00:58:55
Alberts: No. What time is it? It's already after five? My goodness!

02-00:59:06
Hughes: Well, I had a slot from three to five.

02-00:59:08
Alberts: I know. I didn't realize that we'd been talking that long. Too many words. We never got to Koshland.

02-00:59:18
Hughes: No, but we will. Do you realize that we have another two hours scheduled?

02-00:59:25
Alberts: When is that?

02-00:59:25
Hughes: Not till the end of May.

02-00:59:27
Alberts: Okay, fine.

Interview #2: May 30, 2012
Audio File 3

03-00:00:01
Hughes: Dr. Alberts, this is the second interview, as you know. We are going to focus, I hope, on Dan Koshland.

03-00:00:13
Alberts: Right.

03-00:00:13
Hughes: But to do so, I thought we should get you to the presidency of the National Academy of Sciences. My knowledge is that despite the presidency of the National Academy being probably the greatest honor in American science, you were lukewarm about the nomination.

03-00:00:43
Alberts: I had a pre-call from the nomination committee, probably nine months earlier, probably early 1992. They asked would I be willing to be considered as president? And I said no, because it's a full-time job and, I thought correctly, you have to close your laboratory. I suppose I could have moved it or tried to struggle along, but I knew that wasn't going to work. Nevertheless, in August that year, 1992, when I was at a book-writing meeting with my colleagues in Norfolk, Connecticut for *Molecular Biology of the Cell*, I got this telephone call which actually made me angry, because I had told them I didn't want to consider it. And they said, "We know you didn't want to consider it, but we want you to at least have the courtesy of meeting us because we chose you anyway." [laughing]

03-00:01:53
Hughes: Who were "they"?

03-00:01:58
Alberts: Well, they were the committee. The charismatic guy who did most of the talking was Harry Gray, who is a distinguished chemist at Caltech, very outgoing, sort of a Dan Koshland type. He, I think, was co-chair of the committee. They said, "Well, you know we did all this work. So at least have the courtesy to meet us." We met at the Hartford Airport lounge because it was about an hour away. I could drive there. There were maybe six people there from the committee, four to six people, including Harry. Basically they said the same thing as when much later the AAAS [American Association for the Advancement of Science] offered me the job at *Science* magazine. The committee knew I was really passionate about doing something about science education. They thought this was an important role for the academy. At that point we were in the middle of producing the first ever science education standards for the United States—a huge project. I was on that committee [National Committee on Science Education Standards and Assessment] and I knew it was going terribly. [laughing]

03-00:03:10
Hughes: You mean it wasn't going anywhere?

03-00:03:14
Alberts: Well, it was off track, yes, and it was wasting time, and I was sending e-mails to my predecessor [as NAS president] Frank Press about how broken the process was. They had a lot of money, and there were too many people involved, and it was chaotic. They gave me a couple of months to decide—and my initial reaction was not to do it. It was a very difficult decision because of course I enjoyed my life in San Francisco; my wife [Betty Neary Alberts] didn't want to move to Washington; I didn't want to move to Washington, but you'd have to.

03-00:03:53
Hughes: And you had a huge lab.

03-00:03:56
Alberts: It wasn't huge. It was maybe twelve people.

03-00:04:01
Hughes: Oh really? I thought it was more like thirty.

03-00:04:01
Alberts: No, no, no. I never had— I always argued against huge labs! [laughing] I couldn't have a huge lab. As the department chair, in fact, I had a set of rules: no lab bigger than twelve.

03-00:04:13
Hughes: Because above twelve there wouldn't be enough interaction with the lab director?

03-00:04:18
Alberts: Right. I wrote one editorial about this, in *Cell*, in 1985. "In Biology Small Science is Good Science," which is why they offered me the job of academy president, because from that editorial they asked me to chair the Human Genome Project committee at the academy in 1986. The reason why they really wanted me was because I was in favor of small science. I had been outspoken about it, and the Human Genome Project was opposed by most biological scientists in the United States for being big science.

03-00:04:55
Hughes: And taking money from their projects, right?

03-00:04:55
Alberts: Well, of course, and changing the nature of biological research. Anyway, I chaired that committee for the Commission on Life Sciences of the National Research Council on "Mapping and Sequencing the Human Genome" and negotiated a compromise. The committee was set up with people on opposing sides, very strong people—Jim Watson and Wally Gilbert on one side and David Botstein and Shirley Tilghman on the other side.

03-00:05:17
Hughes: Power people.

03-00:05:20
Alberts: So we had a strategy not to discuss our recommendations till we had met for about eight months or so and got more data. By the time we had done that we could come to consensus. That was a very successful science policy effort. It was the first one I had ever been involved in. It was because of that that the academy kept on asking me to do more science policy work for the National Research Council. So by the time they asked me to be president I had been chair of the biological division of the National Research Council and had spent a lot of time at the academy as a volunteer.

03-00:06:02
Hughes: You were a known entity.

03-00:06:02
Alberts: Well, yes, I had been involved in science policy. I was known to be somebody who was interested in trying to fix all these problems that we have in our society. I didn't know why actually they chose me as president, and I never did learn. I still don't know who else would have taken the job. I did know that it would be very unlikely that they would focus on science education, because not that many academy members were interested.

03-00:06:34
Hughes: Had Frank Press served only one term?

03-00:06:39
Alberts: Two terms. Twelve years.

03-00:06:41
Hughes: Which is the limit, isn't it?

03-00:06:42
Alberts: Yes.

03-00:06:43
Hughes: Was he a big advocate—as big as you—of science education?

03-00:06:47
Alberts: He was certainly an advocate. I wouldn't say he was as big as me. [laughing] He did an important thing; he started the National Science Resources Center with the Smithsonian, a joint project, in 1985, and I give him a lot of credit for that. And of course he took on the National Science Education Standards. But he had assigned the task of looking after it to his vice president, because it wasn't something he was expert in. That was Jim Ebert, and Jim Ebert wasn't watching either. So one of the first things I had to do was change the leadership from Jim Ebert, and I got Rick Klausner to do that.

03-00:07:27
Hughes: I imagine that you went in with some very discrete goals that you wanted to accomplish in that first term. I doubt you were thinking of two terms yet.

03-00:07:38
Alberts: Four years is all I'd promised them. And not even six years! That's what I told my wife. Well, I certainly wanted to get the science education standards to be an effective report, and I spent half my time for two years actually working on it.

03-00:07:56
Hughes: Now was the SEP [Science Education Partnership] program at UCSF already in place?

03-00:08:00
Alberts: Oh yes, the SEP was well underway.

03-00:08:03
Hughes: You should say a little about that too, because that shows your early interest in education.

03-00:08:10
Alberts: Yes. My children all went to the public schools of San Francisco. I was seeing that they weren't getting much science. Science was sort of an option. You could choose woodshop, band, or science at the middle school. I remember that. But that wasn't what really caused me to get involved. It was really my wife becoming president of the PTA, first of Lowell High School, which is the selective academic high school, and then in the mid-eighties of the whole San Francisco PTA. The school board meeting was broadcast on the radio every two weeks. I had to listen to her speak, and I heard all this craziness that was going on at the school board. And that's when we started the Science Education Partnership. At any rate, that was part of my knowledge about science education, realizing how desperate the situation was because scientists were not sufficiently involved.

03-00:09:11
Hughes: Well, explain what the basic idea of the program was.

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Alberts: Well, the basic idea was to use volunteers at the university [UCSF] and our resources— For example, all the supplies and the old centrifuges and microscopes and things that we had no more use for, and which were going into storage in a warehouse at Oyster Point, to try to make those available to the school district. If the school district teacher wanted to have a fruit fly to show the student, they had to order it a year ahead—or at least it was very complicated. So we had all these resources—frogs, fruit flies, worms, yeast, Petri dishes, all kinds of things that we could give them. The schools were like a third-world country in the middle of a first-world city. And that's still a problem. Anyway, so it started with basically getting a group of teachers together from the district and asking them what they thought UCSF could do.

That was the reason why it worked. We didn't say here's what we're going to do; we asked the teachers what they wanted. And David [J.] Ramsay and I worked together on that. He deserves a lot of credit. He was a vice chancellor for—something. [Senior Vice-Chancellor of Academic Affairs]

03-00:10:43
Hughes: Another program linked to education—because I do think of you as Dr. Science Education—is the PIBS program at UCSF, the Program in Biological Sciences. I understand that it was a way of getting the basic science departments to collaborate, to share.

03-00:11:05
Alberts: Right. Mike Bishop was the godfather of PIBS. Rudi Schmid was the other godfather. [laughing] It worked because our department supported it, because basically we had by far the strongest graduate program. By working with Rudi and Mike I got our faculty to agree to stop that departmental program so it wouldn't compete with the PIBS programs. The traditional thing they'd do is to form an interdisciplinary program on *top* of everything else, and that becomes a minor program, because it's in competition with the departments. So with Rudi's help offering resources, we could convince the faculty that this made sense, and so I think that was a tremendous success. That story is well described in Rudi's oral history [http://www.dahsm.medschool.ucsf.edu/oralhistory/archival/pdf/schmid.pdf].

03-00:12:10
Hughes: And in Bill Rutter's [http://content.cdlib.org/ark:/13030/kt7q2nb2hm/] and elsewhere. I think it's Bill who argued that the culture of UCSF, which is described as being very collaborative, has a lot to thank the PIBS program for, that instead of scientists working strictly within departmental walls there was much more integration across the departments.

03-00:12:39
Alberts: Right, of course. Well, PIBS was a symbol of what we thought was unique about UCSF. Because we were certainly not in the first rank when I got here! So we had to try harder! [laughing]

03-00:12:49
Hughes: I know that. Bill had spent much of his time building up the research end.

03-00:12:55
Alberts: Of course.

03-00:12:56
Hughes: And my understanding is that you then took up the educational part.

03-00:13:01
Alberts: Well, I was one of them, of course.

03-00:13:05
Hughes: Well, we could go on forever. Let's get back to the National Academy of Sciences. You mentioned the Human Genome Project. I should know the dates better, but were you directly involved as president in that fiery controversy between [J. Craig] Venter and the NIH?

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Alberts: No, no, that was much later. [laughing]

03-00:13:28
Hughes: You were out of the picture?

03-00:13:31
Alberts: Well, I was president but I wasn't directly involved in that controversy. They tried to drag me into the controversy but I didn't want to be dragged. [laughing] By that time Don Kennedy was head of *Science* magazine, so he talked to me a fair amount to try to get the academy's agreement that what he was doing was okay. I was trying to do a little mediation on the Venter side concerning open data availability, but it wasn't successful really. In the end, what Venter had promised to do with the data when they published in *Science* magazine, Kennedy had carefully negotiated, the company [Celera Genomics] wouldn't let him follow through with, so everybody was criticized. Venter was criticized; *Science* magazine was criticized.

03-00:14:29
Hughes: [Francis] Collins was criticized. Yes, it was a mess.

03-00:14:34
Alberts: Yes. But I didn't spend much time on that as president of the academy. I tried to duck out of it.

03-00:14:40
Hughes: Well, we could spend the whole time on your twelve years as president. Maybe one more question about your presidency, and then we've got to talk about the [Marian Koshland Science] Museum [of the National Academy of Sciences]. Given your reluctance, why did you accept a second term as president?

03-00:15:00
Alberts: First of all, it takes time to do anything. The first year I don't think I accomplished anything, except that we had to change some of the employees. We tried to make changes, but you can't make changes without changing the people. So I thought I was a complete failure one year in. I think many people thought that. [laughing] But finally, when there was enough change of personnel to get people who really agreed with the changes I wanted, then things started to happen.

03-00:15:35
Hughes: Please outline where you wanted the changes to lead.

03-00:15:46
Alberts: Well, I was trying to make our education efforts more effective. I was trying to get rid of all the little silos that we had, of competing units, and trying to get more cooperation into the academy. There were lots of issues, like computer services were not working, and we were wasting lots of money on things that weren't effective on the operational side. But the biggest real issue was the fact that traditionally the National Academy of Sciences has been the boss, because our charter is from Abraham Lincoln, under which the National Academy of Engineering and the Institute of Medicine and the National Research Council, the other three components, had been established. When I got there, I really felt that we could do a lot better by working together. The engineers, especially, had been very resentful about that. There's always a proposal that the head of the National Research Council, which does all the studies, should alternate between the president of the National Academy of Sciences and the National Academy of Engineering. But that would never work, in the sense that my members would have to vote for it, and they would *never* give that up. So instead I tried to work as if we were much more equal.

We had terrific people—Bill Wulf, after initial craziness with his write-in candidate, was the NAE [National Academy of Engineering] president for most of the time when I was there. Initially, Ken Shine and then Harvey Fineberg were the Institute of Medicine presidents. The three presidents worked very closely together. We had very frequent presidents' meetings to make major decisions. We launched a capital campaign, so we all had to think big and have some vision for the place. The war between the National Academy of Engineering and the National Academy of Sciences that had gone on for decades, I think, ended. The engineers felt that they were much more respected as a real valuable partner.

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Hughes: Does that remain so today?

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Alberts: I hope! [laughter] I haven't followed it closely. But anyway, my major intention was to break down the silos, including the three academies, and get us all to work as a team.

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Hughes: The National Science Education Standards Report came out in 1996, and that was in your first term. That was a big deal!

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Alberts: Yes, that was a big deal, yes. So I said I spent half my time— I actually wrote large parts of the standards, which is not what the president's supposed to do! Rick Klausner would come. He was volunteer head of the standards project. That was before he was the head of the National Cancer Institute. He was just the head of a lab at NIH, but I knew he was interested in education and a very effective leader, and so he and I would meet almost every weekend in my

apartment and work on the science standards. He would edit parts of it; I'd edit the other half. And that's not what a president's supposed to do!

03-00:19:20
Hughes: Were there teeth in it? It's fine to write a report, but how do you get it applied?

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Alberts: Well, that was the problem. We produced a great report in 1996, but the governors who'd asked for it in 1989, by the time it got funded and done the governors had largely changed. The states have a dominant role in education according to our constitution. It's a states' right. In fact what happened was disappointing. States like California and many others made their own standards, and in many cases didn't pay much attention to the National Science Education Standards.

We have, in California, a very factoid-based set of standards, with the idea that more difficult stuff at lower grades makes science teaching better. So we have the periodic table in fifth grade, for example. Anyway, there was a lot of politics that got involved, the Hoover Institute, and all kinds of people who didn't really know much about science education. And now we're trying to recover from that with the new common standards. But anyway, I think the National Science Education Standards, as a document, was a very successful, high-quality document. But the fact that it didn't have any teeth— The federal government doesn't have any real role in deciding what states do, and the states were supposed to voluntarily use it, and some of them did and some of them didn't. But all in all, the standards movement I think has been a disaster because of No Child Left Behind and all kinds of craziness—fifty different states making different standards. So I think I'm very disappointed with what happened.

03-00:21:10
Hughes: Would you summarize the gist of what you hoped would happen as participatory science instead of lecturing to students?

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Alberts: Right. A fundamental focus of the science standards is on science teaching as inquiry—students doing active learning of science. But of course what got omitted was that, because that's harder to do and more difficult to test for. We're in danger of that happening again because the new way of talking about it is [with] an emphasis on the "practice of science." And again the big worry in the new common standards is that formally they will say they're doing it, but they won't be doing it, because it's harder to do than just to teach facts.

03-00:22:04
Hughes: Okay. The museum. Am I right in thinking that you had had a long history with Dan?

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Alberts: Yes. Dan and I were both biochemists. We went to many of the same scientific meetings. Anyone who actually interacted with Dan knew he was a live wire. And so we had good chemistry. We were always joking with each other for some reason, and he was joking with everybody. He always had something interesting to say. He knew many of the same people that I did, and I'd read his papers and thought some of the stuff that he was finding out was fascinating and important. We wrote about it in our textbook. At any rate, we had many interactions, although not formal ones. We never published together. So I knew him well. Everybody liked Dan.

The other thing I should say that's important is that one very useful tool to get the presidents to work closely together was planning together a capital campaign, which we did, so that we all worked together with our members. The National Academy of Engineering has some very wealthy members because they have many industrial members, for example. They had always done fundraising separately for their own academy but not really synergistically for the whole entity. So part of this idea of breaking down the silos was to get us all to work together on common projects, and that I think was quite successful. So one of the big projects was public understanding of science and science education, and of course this was the kind of thing that led to the idea for us having a small museum.

03-00:24:10
Hughes: Dan came to you, I read somewhere, not quite sure what he wanted to do in Bunny's memory?

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Alberts: Yes. When Bunny died he was devastated.

03-00:24:23
Hughes: Yes, I know he was.

03-00:24:23
Alberts: I knew Bunny very well also. He wanted to do something for Bunny, and so I developed this set of options, one of which was the museum. I can't even remember what the other ones were, but obviously something to do with science education, a very ambitious scheme in science education. Actually, I never knew that Dan had any money until I came to the academy.

03-00:24:52
Hughes: He didn't broadcast that, did he?

03-00:24:53
Alberts: More than that: every time in the airplane he's back in coach class with his wife. What was it, his eightieth birthday? They had a big birthday celebration for him at the California Academy of Sciences. Arthur Kornberg's talk about him was all about how bewilderingly humble Dan was. You know Arthur; I know Arthur. He would only go someplace if they would send him first class

and give first class tickets for his wife as well. So Arthur and Dan were absolutely opposite.

Arthur talked about Dan getting off a plane, I think in Israel somewhere, where they had some committee meeting. Dan climbed out of coach all rumpled, no sleep, and had to go to work right away. Arthur couldn't understand why Dan insisted on flying coach. But even at the end he was doing that, because Betty and I came back from an annual meeting on the same plane as he and, I think that was, Yvonne [C. Koshland, Dan's second wife] in the back. [laughing]

03-00:25:57
Hughes: They were in coach.

03-00:25:59
Alberts: So how would you know that he had any money? I only learned that when I got to the academy and the development office said he had money.

At any rate, we came up with a variety of ideas. The museum was originally going to be located near the Einstein statue at the old building, because the idea that I had was that all these school tours come to take their picture at the Einstein statue.

03-00:26:29
Hughes: The old building of the National Academy of Sciences?

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Alberts: Yes, on Constitution Avenue.

03-00:26:34
Hughes: Which is off the mall?

03-00:26:35
Alberts: Yes, it's right off the mall; it's sort of on the edge of the mall on Constitution Avenue. So the original idea was to exploit the fact that these people always came there for their Einstein statue photograph and to make a museum connected to *it*. When Dan chose the museum option, then we got architects to try to figure out how to do that, and it turned out to be really difficult because of the location and the rules. Sue Woolsey, who was my chief operating officer, was a valuable resource on all these kinds of things. She really led the effort to locate it where it ended up being located, at the new Keck Center of the National Academies.

03-00:27:27
Hughes: She was an employee of the museum?

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Alberts: She was the chief operating officer of the academies. There were two. Bill Colglazier was the executive officer responsible for all the reports, all the intellectual work of the academies and the National Research Council. The

two of them were equal. They were my direct reports for most of my term. She was responsible for all the things like the financial system, human resources, computer services, building. She was heavily involved in building the new Keck Center, and she had lots of talents that most scientists lack. [laughing]

03-00:28:18
Hughes: Dan talked to me a little in 1999 about his ideas for the museum. I don't know that any ground had been broken, but I remember him saying that he was very interested in creating a very interactive place. He wanted the man in the street/woman in the street to be welcomed and drawn into science.

03-00:28:51
Alberts: He used to talk about Joe Six-Pack.

03-00:28:51
Hughes: Yes. [laughter]

03-00:28:53
Alberts: Joe Six-Pack I don't think is that interested in science. But anyway, that was his dream. So by 1999 we should have broken ground. The museum opened in 2004.

I really spent a huge amount of time with Dan once we decided to do the museum. He was very much a hands-on person. So he and I went over all the details. We had lots of problems. The first person we hired to run the museum project didn't work out and we needed to replace him.

03-00:29:49
Hughes: Why didn't he work out?

03-00:29:54
Alberts: I can't remember all the details. The initial designs involved hiring a firm that knew how to do this kind of stuff.

03-00:30:07
Hughes: To do the exhibits, right?

03-00:30:07
Alberts: To do the exhibits. But the new idea here was this is the National Academy of Sciences. It was going to be closely based on members of the academy, the high-quality scientists, working with designers to create the exhibits, which doesn't happen almost anywhere else in the museum world. The scientists are really not much involved. In fact I knew this because I had been asked to serve on a committee of the Smithsonian to see how they might get more science connected to their museum development process. They had already been struggling with the absence of this. So we wanted to actually set a different example, and we did actually. But that meant that not only scientists who were expert in climate change, which was our first exhibit, were very much involved, but also Dan and I. In fact, he and I were the major scientists

involved in the DNA exhibit, which was also one of the first exhibits. So he and I spent a lot of time editing things, writing things, going back and forth. Dan was very much hands-on about what actually was going to happen in the museum, especially in the DNA exhibit, one of the two major exhibits.

03-00:31:26
Hughes: How did the public respond?

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Alberts: You mean after it was open? I think it was quite successful. It's a small museum. It's six thousand square feet, and the Smithsonian is probably a hundred times bigger.

03-00:31:42
Hughes: Do people find it? Do people who are going to the museums on the mall come on over?

03-00:31:51
Alberts: Well, we're not on the mall. That's another problem, the location. A major part of the audience are school kids who come on buses to the museum. We have a major program to enlist them. There's also a major website associated with the museum that was part of Dan's vision. His sister [Frances] Sissy [Koshland Geballe] and Ted Geballe really supported that part of the effort. So Dan and I worked together with all the people involved in the museum. We had numerous dinners in Berkeley. Patrice Legro, a senior staff officer in the education division of the academy, was the new person that we appointed to take over the museum, and she did a very good job. We had volunteers from the academy working on this thing. But Dan and I spent a lot of time on one of the two exhibits together. In fact I still remember editing things that he wrote, and he editing things that I wrote.

03-00:33:12
Hughes: Did you generally see eye to eye?

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Alberts: Yes, yes.

03-00:33:21
Hughes: Dan apparently thought that he had made a sufficient endowment, but then the economic downturn came along and apparently there were funding problems.

03-00:33:38
Alberts: Well, that's still a problem, in the sense that the vision was the museum would either be free or very inexpensive, so you're not going to make much money from receipts. So in the end Dan decided on a five-dollar entry fee, just to make sure people wanted to come and didn't just walk in to get warm, because it was small. I think students are free. There are all kinds of free things as well. The fact is if you're going to have a science museum you have to renew the exhibits every four years or so. You can't just have an initial exhibit sit there. We had thought that these great exhibits we had designed,

once we finished with them, we could sell them to other museums. Although we have sold some to other museums, basically it's a giveaway. We don't really make money on that, because you can't sell it for a reasonable price, and you have to help them with advice and technology and all that kind of stuff. So the DNA exhibit—which I was very proud of; I thought it was great—I think it's now in Dallas permanently in a science museum and gets many more visitors there than it had in Washington.

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Hughes: Will it travel from there?

03-00:35:08
Alberts: I don't know what their plans are, or whether they're going to leave it there permanently or somebody else will take it. The fact is that in order to create new exhibits, given the endowment, you don't want to spend out the endowment. You want to spend only 5 percent, or 5.5 percent or something. And given the crash that was a real problem. You have to get support from other sources. They've been quite successful in getting money from the National Institutes of Health for biology exhibits. From others also. And there's always, of course, the idea that we should be raising more money.

03-00:35:53
Hughes: Of course! Well, you mentioned the most recent meeting before we were recording. Was the topic fundraising?

03-00:36:05
Alberts: Well, that was part of it. That wasn't the major topic. The major topic was what is the future of the museum? What have we learned from our visitors and from our experience? Basically, the first exhibit on climate change had a little feature, that we thought was a minor feature, where a researcher who studies public attitudes about climate change was collecting data on this computer. People would make their choices: if they want to get their greenhouse gases below some number what will they trade? A smaller automobile? Less heat in their house? Less air-conditioning? All these different things. The people in the museum were actually participating in research by doing this. We discovered that that was one of the most popular features of the museum, so the idea now is to be much more participatory, so that the new exhibits have more of that. There's now a sense that we should be involving the community more in all these issues and having actual—we're doing experiments. The museum's doing experiments involving the community and events that make use of museum resources and having the ability for them to contribute, like contests for them to make little videos about some subject that we specified and a question. Then they put the best of the videos in the museum and on the website. So it's basically trying to make the museum more a participatory thing.

03-00:38:02
Hughes: But also the scientists are looking at what the public is saying, right?

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Alberts: Yes, of course.

03-00:38:13
Hughes: Well, that’s unusual.

03-00:38:19
Alberts: So it has become a place to experiment with science communication. I think that’s a very interesting and important topic, especially now that the nation’s becoming more and more anti-science. Not only anti-evolution but now anti-climate change, for reasons that nobody seems to understand, except that there’s a lot of money put in to try and confuse people.

03-00:38:39
Hughes: Right. Another controversial issue is the creationism movement in this country. Were you involved in that at all as president of the National Academy of Science?

03-00:38:52
Alberts: Of course, of course. Every ten years or so we put out a book on science, creationism, and evolution for school boards and the public. I was very heavily involved in the second edition of that.

03-00:39:10
Hughes: Are those books used?

03-00:39:14
Alberts: Yes. Well, they’re not only used but they’re tools that the academy uses whenever there’s a challenge. For example, in Tennessee or Oklahoma, we enlist local scientists and engineers and give them those booklets as resources. So it’s not the academy doing it directly; it’s the academy providing a source for people in the state to use.

03-00:39:35
Hughes: Which is wise politically, too.

03-00:39:38
Alberts: Yes, well it is, and was in a way it works. But those issues seem to never go away.

03-00:39:47
Hughes: Isn’t it the truth. Well, anything more you’d like to say about the museum and Dan?

03-00:39:53
Alberts: Well, I had a great time interacting with Dan. Dan, I think, was a very unusual person. He was not a conventional thinker. And some of the ideas he had were crazy! So part of my job was trying to humor him while we tried to get him to think more carefully about these ideas. [laughing]

03-00:40:25
Hughes: Switching briefly to his science, I think if you asked him what scientific work he was most proud of he would say induced fit, the induced fit model.

03-00:40:39
Alberts: Oh, of course. It's still taught in biochemistry courses.

03-00:40:44
Hughes: Were you around when he was having such controversy with the French group?

03-00:40:52
Alberts: Of course I was around.

03-00:40:54
Hughes: I mean, were you paying attention?

03-00:40:56
Alberts: I wasn't talking to Dan about it, but I was paying attention because I was teaching that stuff.

03-00:41:04
Hughes: And induced fit eventually won out?

03-00:41:08
Alberts: I would say that there's truth in both views, and induced fit is certainly part of the answer but it's not the whole answer. So as usual, there's a lot of truth in what Dan said. He also made many other major contributions. His work in chemotaxis was incredibly pioneering. His theoretical work, which is quite well appreciated now, is how when you link a series of reactions, like in cell signaling networks, if you're on the right part of the kinetic equilibrium fit for each relay station protein in the system, you can get a very sharp response from what initially is a gradual signal. And that's very important for biology. Cells have to decide yes or no. They can't say well, maybe. [laughing] And part of the way that's done is through this mechanism he identified in a very important theoretical paper. He had also important theoretical papers on allostery. He made a wide range of contributions. He was really a very important figure in the field of molecular biology and modern biochemistry.

03-00:43:17
Hughes: At the end he was working on alternate sources of energy, and he actually had applied for a patent.

03-00:43:24
Alberts: Yes, I talked to him about that actually. I visited him in his lab the last few years of his life, and he still had students buzzing around, and it was incredible. He was still excited about these different interests that he had. I don't even know half of the things he was interested in.

The other major thing we haven't discussed about Dan, and I'm sure you've talked about that with others, is that he had an incredible effect on Berkeley—the kind of thing I tried to do in the academy getting rid of silos. He was focused on that intensively at Berkeley way back in the early eighties. When I came to UCSF—I came here in 1976—Berkeley was basically a set of small departments in biology. I would repeatedly see them, when they were hiring new faculty, not hiring the best of our postdocs, because small groups were making decisions with inadequate knowledge and trying to fill some narrow idea of what they needed at Berkeley in that particular department. They seemed to have millions of departments.

03-00:44:49
Hughes: Plus there was real tension between biochemistry and virology when [Wendell] Stanley was in control. Never the twain shall meet.

03-00:45:00
Alberts: Well, Stanley was out of there by the time of what was called the Palade committee. It was really Dan's doing to get George Palade who was a very wise man, as well as a very distinguished scientist.

03-00:45:14
Hughes: Oh! The reorganization of biology, right.[3]

03-00:45:19
Alberts: Dan, I think, engineered that.

03-00:45:18
Hughes: Oh, he did.

03-00:45:22
Alberts: It came as an outside report which said that Berkeley life sciences needed to reorganize.

03-00:45:31
Hughes: Well, Berkeley was slipping in the academic ratings.

03-00:45:31
Alberts: Yes, they were. When you realize how hard it is to do that kind of thing at a university with tenured professors, incredibly entrenched self-interests, it's amazing that they were able to do it [achieve the reorganization of the life sciences]. That was only because of Dan, as far as I could tell. For universities to do the right thing is often difficult.

03-00:45:58
Hughes: Well, you know it feeds right into what I've been trying, in a minor way, to capture for this oral history retrospective. In interviews with [Robert] Tjian

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[3] See the oral history: http://www.lib.berkeley.edu/cgi-bin/roho_disclaimer_cgi.pl?target=http://digitalassets.lib.berkeley.edu/roho/ucb/text/reorganization_biology.pdf

and Randy Schekman, they made it very clear that what Dan had done in the reorganization of biology in the eighties and into the nineties played right into these two major multidisciplinary buildings [Li Ka Shing Center for Biomedical & Health Science and Stanley Bioscience & Bioengineering Facility] that are now operative on the Berkeley campus.

03-00:46:28
Alberts: Well, that was the next step! [laughing]

03-00:46:31
Hughes: Dan may be gone but his mark is still on that campus.

03-00:46:41
Alberts: He was quite a visionary. I don't know anything about his personal feud with Stanley, but I didn't really know Stanley.

03-00:46:48
Hughes: Oh, I wasn't implying it was between Dan and Stanley. I understand that Stanley wanted his own fiefdom and didn't want a lot of interaction with anybody outside *his* department.

03-00:47:05
Alberts: Originally, Berkeley had a lot of prima donnas. [Melvin] Calvin had his own building. It was a pretty Germanic kind of university for a while, modeled on the Germanic idea that there are these people who know everything and they get all the resources.

03-00:47:30
Hughes: Dan got the Lasker [for lifetime achievement in medical research], as you may remember. I forget the year [1998]. Did you have any part in that?

03-00:47:46
Alberts: I don't think so. I don't remember. I often write letters. I may have written a letter. Later he headed the Lasker Award for Public Service, and I was on that committee with him for years. He, again, was a very efficient chair for that committee.

03-00:48:13
Hughes: He gave money to their organization.

03-00:48:16
Alberts: Oh, he did? I didn't know. I know he gave a lot of money to Berkeley, which is great. And then he gave a lot of money to the Koshland Science Museum. But I don't really know his other charitable interests.

03-00:48:33
Hughes: He gave a lot of money, $40 million no less, to the new Stanley Hall.

03-00:48:38
Alberts: The renovation?

03-00:48:43
Hughes: Yes.

03-00:48:43
Alberts: Stanley Hall was torn down, wasn't it?

03-00:48:48
Hughes: Yes, it was, and there's a new multidisciplinary basic sciences building there.

03-00:48:54
Alberts: Is it called Stanley Hall?

03-00:48:53
Hughes: Yes. It should be Koshland Hall, but there's already a Koshland Hall, which is interesting in itself in that the tradition is that buildings are only named after dead people.

03-00:49:19
Alberts: I know. You heard from Randy [Schekman] and Tij [Robert Tjian] [in the interviews for this oral history retrospective on Dan's last years] that he was just a tremendous force at Berkeley, and all the people on the right side of history really appreciated him.

03-00:49:35
Hughes: You mentioned PIBS [Program in Biological Science].

03-00:49:48
Alberts: When Bill Rutter came here [1969-70] nobody had ever heard of UCSF in basic sciences. It's much easier to do innovative things when you don't have anything to lose, which is why Harvard has had a hard time doing innovation. I was an overseer at Harvard for six years, but when you think you've been successful you don't want to change anything. It favors the conservatives who don't want to change anything. I've always been in favor of change but many people are not.

03-00:50:36
Hughes: Did you overlap with Bill Rutter? Because he was an overseer too.

03-00:50:46
Alberts: No, he was finished.

03-00:50:45
Hughes: I'll bet you were saying the same things.

03-00:50:51
Alberts: Oh yes, well, I had chaired a visiting committee to the Department of Molecular Biology and Biochemistry [at Harvard] in 1983, and our report in '83 was the same as the report of the committee Bill was on with Mike Bishop, and that must have been in 2000 or something. Nothing had changed. [laughing] So that was part of the problem. There's no real governance

mechanism at Harvard, because nobody thinks they need change. We always get the best students anyway; why should we have to change anything?

So Berkeley had a lot of that attitude as well when Dan was trying to change it, so he had a much more difficult task than we had when we were establishing PIBS.

03-00:51:44
Hughes: You were starting from nothing at UCSF, and he was trying to change existing structures.

03-00:51:53
Alberts: Well, we had the Biochemistry Department. By that time the Biochemistry Department was pretty strong, but that was where we built everything, to get good faculty to come, like the Jans [Lily and Yuh-Nung], or Marc Tessier-Lavigne, who's now the president of Rockefeller University, or Cori Bargmann—all these quite famous scientists now.

03-00:52:15
Hughes: How about Harold Varmus and Mike Bishop?

03-00:52:17
Alberts: Well, no, they were already here. But to hire those kinds of people I just mentioned at UCSF we had to offer them joint appointments in Biochemistry for many years. So the Biochemistry Department that Bill created was then the nucleus from which all the other basic science departments were able to develop their own world-class quality faculty.

03-00:52:45
Hughes: Did offering the joint appointments help you get the top scientists?

03-00:52:50
Alberts: Of course, because for one thing they would then have access to our graduate students. This was before PIBS. Faculty want outstanding graduate students to be in their laboratory. So when any new recruit had competing job offers, it was very attractive to have a joint appointment in Biochemistry, which really meant that they had access to Biochemistry graduate students. Once PIBS got really strong, that wasn't necessary anymore because the strong graduate students were all over the place, and you didn't have to be in Biochemistry to get them. Anybody in any department could.

03-00:53:31
Hughes: I remember Bill drawing a contrasting portrait with Biochemistry at Stanford under Arthur [Kornberg], who wanted the walls of Biochemistry to be pretty high and firm!

03-00:53:47
Alberts: Yes, well Arthur built a very good department, but he was exactly the opposite of Bill. Basically he wouldn't even take MD/PhD students in the department as graduate students.

03-00:54:00
Hughes: Is that so?

03-00:54:04
Alberts: In fact, they weren’t doing much teaching of their own students because I and others were offered— I did it for a couple of years and then I caught on and stopped doing it. They had me come down and talk to their medical students. They offered me some money—I don’t remember how much it was—to come and give the lectures on DNA replication to their medical students. Well, Arthur Kornberg was the world’s expert in DNA replication! So why was I giving those lectures?

03-00:54:35
Hughes: Why wasn’t he doing it?

03-00:54:37
Alberts: Because they didn’t want to teach, even though they were the Department of Biochemistry in the medical school. So for years they had money that Arthur had obviously gotten from Stanford to pay people from UCSF and elsewhere to come do the teaching that they were supposed to be doing. It was a very focused but effective research department. And there were good things that Arthur did within the department that were very innovative. They didn’t have the huge labs that we have here where multiple faculty occupy the same space. They had small labs, small little rooms in the medical research building, and I think they’d have four scientists per room. But Arthur very innovatively said each room would have people from two different groups in it. So if I had a lab of ten people, I’d have people in say five different rooms, and they’d be shared with all kinds of other people. That’s a very innovative. So within the department he was brutal about breaking down walls, but he didn’t want anything to do with such endeavors outside of the department.

Audio File 4

04-00:00:00
Hughes: In 2008 you became editor-in-chief of *Science*.

04-00:00:01
Alberts: Of course, here I also followed Dan.

04-00:00:03
Hughes: Yes, you did. Not directly though.

04-00:00:06
Alberts: Floyd Bloom was there for five years, and that’s how long I’m staying.

04-00:00:16
Hughes: He followed Dan. And then who was next?

04-00:00:19
Alberts: Don Kennedy.

04-00:00:20
Hughes: Don Kennedy, of course.

04-00:00:21
Alberts: He was there for eleven years or something.

04-00:00:25
Hughes: Did you take advice from all those people when you became editor-in-chief?

04-00:00:33
Alberts: I don't think Dan knew I became editor. When did Dan die?

04-00:00:44
Hughes: Two thousand and seven.

04-00:00:46
Alberts: He missed it. I didn't know about this until fall of 2007. He was dead. I would have certainly talked to him had he been alive. I did meet with Don Kennedy, of course.

04-00:01:09
Hughes: Why did you take that position?

04-00:01:10
Alberts: This time it wasn't a committee, but a search firm called me. They call me a lot for different positions. And they said, "We're looking for an editor of *Science* magazine, and I gave them a bunch of names of young people, I mean younger than me. They asked me if I would have any interest, and I said no because I'm too old. [laughing] They knew about the trick that had worked before. So this time it was David Baltimore, who was president of AAAS [American Association for the Advancement of Science]. He said, "We know you didn't want to be considered, but science education can be a focus of *Science* magazine."

04-00:01:51
Hughes: [laughing] They knew how to get you.

04-00:01:54
Alberts: They must have talked to Harry Gray, or maybe Harry Gray was on the committee. This time also, my initial reaction was negative, but it wasn't nearly as difficult as being asked to become president of the NAS. It was a half-time job, thanks to Dan.

04-00:02:09
Hughes: He was the first one to make it half-time.

04-00:02:11
Alberts: That was a big deal, because you can't even mention the name of *Science* magazine without talking about the huge role that Dan played in changing the nature of the magazine. *Science* magazine was founded by Thomas Edison in 1880 or something like that. It had been prominent, but by the time that Dan

got there it had really decayed. I remember myself looking at the lead articles in biology and saying, "Why in the world is this important? Who's making these decisions?" They had got to be, I would say, a very wonky/idiosyncratic choice of papers in my field of biology—and I don't know about other fields. I think it was mostly biology, though, that *Science* covered.

So Dan took over, and he just dramatically changed *Science* magazine. It's hard to believe, but they had no scientists as editors. They had only people who knew how to write English. [laughing] I don't know how they ever decided— They sent manuscripts out to referees, but how did they know how to decide? I don't know how that place worked. Phil Abelson had been editor-in-chief for a huge, long time. I only met Phil when he was quite old, and apparently when he was younger he was very effective. But *Science* magazine had really decayed. Dan, of course, was offered the same full-time job that Phil Abelson had. My understanding is that David Hamburg, who was also a friend of mine, was ultimately decisive in getting the AAAS to offer it to Dan for half-time when he wouldn't take it for full-time.

04-00:04:10
Hughes: My memory is that Dan said he wouldn't come unless it was half-time.

04-00:04:15
Alberts: Oh no, he wasn't going to come. He turned it down. So they decided that Dan was worth this, so they would have him half-time. This was before e-mail, so I can't imagine how he did anything!

04-00:04:27
Hughes: He only spent one week a month in Washington, DC.

04-00:04:31
Alberts: Yes, that's what I'm supposed to spend too, but I have e-mail all the time. They didn't even have FedEx, I think! I don't know how they got stuff back and forth, but maybe everything was slower back then.

There are wonderful stories at *Science* about all the differences Dan made, from some people who were there when Dan came and from the first scientists that he hired. He then recruited John Brauman, and they paid John Brauman to be a special advisor on non-biological sciences and gave him the task of building up the chemistry and physics. John Brauman just retired about a year ago. He's been chair of the senior editorial board for—I don't know—twenty-some years. We had a nice going-away party honoring John. It's hard to imagine now. Dan charged John, since Dan was not an expert in physics and chemistry and John is a distinguished chemist, with building up *Science*'s reputation so that the best physical sciences papers would come to *Science*. And they *do* now. And basically John did it by actively recruiting the best physicists and chemists to submit articles to us.

Dan did the same thing in biology. The two of them basically recruited famous/outstanding scientists to write review articles for *Science* magazine in their specialties. I haven't gone back and looked at all those old reviews, but that would be an interesting thing to trace: who wrote those reviews, how many were there— There are large numbers. I know John was responsible for getting one a week. I don't know how the heck he did this! But he had lots of friends, so basically he did it by twisting arms like Dan did—and that built the reputation. Plus, changing the nature of the editors. We now have twenty-three, not only PhD scientists in different fields, but people who've at least had distinguished research postdocs. I hired five new editors. They all at least had had a postdoc. We didn't hire any of them unless the advisor said that they were very disappointed they weren't going to go on to become great scientists. So the quality of the people we can get now to be editors at *Science* magazine is outstanding. That all started with Dan. So the net result is that *Science* magazine really is one of the top two journals in the world: *Science* and *Nature*.

04-00:07:30
Hughes: Dan had a competitive streak, and he wanted to be very competitive with *Nature*. That was one of his motives for accepting the editorship.

04-00:07:43
Alberts: Yes.

04-00:07:43
Hughes: I think by the end of his tenure, *Science* was pretty close to *Nature* in circulation and attention.

04-00:07:52
Alberts: Well, circulation is much bigger.

04-00:07:53
Hughes: Than *Nature*?

04-00:07:57
Alberts: Yes, than *Nature*, because *Science*'s circulation is tied to membership. Our circulation's now a hundred thousand, and it used to be even more than this. It was an amazing feat because it really demanded a tremendous effort. I don't know how Dan did it with all the other things he was doing—teaching, running a research lab.

04-00:08:23
Hughes: He didn't stop.

04-00:08:33
Alberts: Well, he must have been working—

04-00:08:35
Hughes: Oh, I'm sure. He didn't go home and forget *Science* magazine. On the other hand, there is something about being right there at *Science* that keeps people motivated.

04-00:08:45
Alberts: Oh, of course. Well, he was famous at *Science* magazine for just walking down the halls and going into everybody's office. That was Dan.

04-00:08:56
Hughes: What are you trying to accomplish at *Science* magazine?

04-00:09:02
Alberts: Well, one of the things I'm trying to do, of course, is science education, make it a bigger part of the magazine. For years *Science* magazine took research papers in many fields of science, but not in education. I'll take research papers in education. That's actually new with me. Don Kennedy, of course, also had a passion for science education, and he started something called the Education Forum.

04-00:09:35
Hughes: I remember.

04-00:09:39
Alberts: He got a small amount of support from Howard Hughes [Medical Research Institute] to enable him to convince the board he should do this. I suppose that's one way to put it, because everything costs money. [laughing] And so I inherited a tradition of publishing an Education Forum, maybe one a month, a two-page article on education.

04-00:10:05
Hughes: You get outsiders to write it?

04-00:10:06
Alberts: Yes, outsiders. We've expanded that. Pam Hines, who was overseeing Education Forum, is now editing the research papers, along with other things. We have a special new editor in the commentary section who focuses on the Education and Policy Forum. And then we have a young PhD I hired, Melissa McCartney, who has been running all these contests because that's one of the new things we introduced. It started with the idea that there was all this wonderful stuff out on the web for science education that's free, but nobody could find it, and it's lost in all the other stuff. So our first contest, which went on for two years, was for the best science education websites, and Melissa was hired to run that, and she's done a great job. We had to set up a whole series of reviewing people—teachers and others—who were not the reviewers that we normally used. The first year we chose twelve winners and published one at the end of every month. And then it was so successful, we ran it for a second year. So we've now published twenty-four of those. They're up on the web on a special education portal that is open access, that is, you don't have to be a subscriber to *Science* magazine to get that.

04-00:11:42
Hughes: That's great.

04-00:11:43
Alberts: And now we're publishing the winners of the third year of the contest, which is now for the best inquiry-based college introductory laboratory exercises. Because college science laboratories are like cooking from recipes—they've been that way for forty years. In college the best science always spreads into the research arena, but the best education doesn't spread. So this is a way of advertising. Each winner gets at the end of the month a two-page article that we publish in the print magazine. All the instructions for running that laboratory unit are on the web as supplemental material, and we're putting it all up on the special website that's open access.

We're trying to see if we can use *Science* magazine to do two things. One is to advertise outstanding research in education, outstanding education programs, and the best materials for teaching science. But a substantial part of the point is to use the prestige of *Science* magazine to give the people who are doing outstanding education work publications in *Science*. This helps their CV, because *Science* has this tremendous cachet in the research community, whereas education doesn't. And so we try to do a little bit to influence the academy—academic tradition—in this way.

04-00:13:16
Hughes: Are institutions paying attention that have a teaching component? Do people know that all this is there?

04-00:13:22
Alberts: I hope so! [laughing] We're just putting together the first collection of all the website winners into an electronic booklet, and we're going to advertise that with the National Teachers Association with the National Science Teachers Association.

These are the projects that wouldn't have happened if I wasn't there—but there are many good things that have happened that would have happened anyway. [laughing] The newest project is called Science in the Classroom, where we went out again to get money, this time from the National Science Foundation. So I could argue to the AAAS board, "We should do this." Our aim is to take one *Science* magazine research article a month—it has to be carefully selected so it's not the most complicated one—and make it usable on the web as an article that can be read by both high school students in Advanced Placement courses and students in first-year college courses, to make sure that students actually know how science works and what science is.

This is a big point of the editorials that I write. Of course I write many of the editorials on education. I recruit many more, but my major point is that if we don't change the nature of college science education, then we can't change education at lower levels. If we don't put inquiry into science in the freshman biology course, then we can't expect to change the definition of science education at lower levels, because those colleges have all the prestige. Every introductory course—I've written editorials about this—Biology I, its major mission should not be to get the kids to know all of biology—it's impossible. But instead to understand some central principles of biology and—very important—to really understand how science works and what science is. The same thing is true about introductory physics. It shouldn't be only about physics. It should also focus on the students understanding the nature of the scientific enterprise. A first or second year college course is likely to be the last course that most people ever take in science. Teach people what science *is*, not by telling them but by giving them some experience with it, both in the lab and in class by reading real science papers and discussing how does the science work? How does this article build on previous science? What aspects of previous work does this scientist say he's discarded, disproved? How do you build up a body of science knowledge by repeated publications that build off of other people's work, either negating what they did or confirming what they did? So I would contend you can't really teach that effectively without having the students read at least a few scientific papers.

So we're trying to take advantage of the fact that our papers are short. They're four pages. The first one we have worked on as a prototype is a paper we published more than a year ago about the fungus that's spreading across North America killing all the bats. So that's been in the newspapers, and the article is directly linked to our own news articles.

04-00:16:47
Hughes: Students can identify with the subject.

04-00:16:50
Alberts: Yes, exactly. That's the point.

Of course if we're going to do this we need real teachers advising us how to make this useful, so we're going back and forth with an advisory group of college and high school science teachers. Originally we thought we'd have to rewrite the article. There's a group in Israel who's done that and published about it. But then we saw by experimenting that rewriting wasn't really adequate, and it's a lot of work. We instead decided to use the real text of the article, and the real figures, but we'd annotate them to make them easier to understand. For example, we have this little thing you turn on, hit a glossary. I think it's called Learning Lens, or something like that. You have these little bars you can click on. One of them is a glossary, so if you hit 'glossary' every complicated word lights up with green over it, and you mouse over that, and it gives you the definition of that word.

Another feature is, what is the data in this paper? So if you hit 'data' then the data lights up. What are the assumptions of this paper? What are the conclusions in this paper? So you have little buttons to identify different aspects. We're trying to create this as a really easy-to-use teaching tool for teachers. But importantly, we have to have teachers use it and keep on experimenting with how to make it work.

We're now at a stage we're getting our second NSF grant, and we're now planning to do one every month and to try to make this an important part of the modern education reform movement. Because if we don't change the nature of how we teach these students from textbooks and have them learn things only that way, we're not going to change the nature of science education. So we are trying to spread inquiry-based science labs, hopefully making use of some of the resources we'll have identified in two years of this contest, twenty-four winners. But we also want to change the tradition so that in a first-year science class students are expected to read a few papers of actual research and discuss what science is. [Yolanda O'Bannon enters.] What's happening? [laughing]

04-00:19:05
Hughes: You're running out of time.

04-00:19:07
Alberts: My boss has come in.

04-00:19:08
Hughes: Can we have three minutes more?

04-00:19:10
O'Bannon: Yes.

04-00:19:37
Hughes: I don't want to end this session without hearing you on the subject of bird flu publication censorship, a topic much in the news at present.

04-00:19:42
Alberts: I see. Well, last weekend I spent time going over the final edits on that manuscript that we're publishing in June.

04-00:19:52
Hughes: Please explain what the situation is. It touches really big issues in science, such as whether potentially dangerous research should be published in the open literature.

04-00:20:04
Alberts: I've spent a lot of time on the bird flu publication issue, which is part of a much broader issue that the Academy identified post-9/11 when there was the anthrax scare. We were asked to do a study. It's now called the Fink Report (*Biotechnology Research in an Age of Terrorism*), after Gerry Fink who was the chairman of the study. We had eight academy members on this committee, plus a bunch of experts on security and terrorism, to ask, in the biological sciences do we need to have a mechanism to prevent certain things from being published in the open literature? In the sense that some things that are

published might be so useful to people who want to do us some kind of harm, whether crazy people in a lab who are unhappy with society or real terrorists. So might there be such a time? And if so how might we go about deciding what should be published and what not published? And what should we do as a US government to set up mechanisms? This so-called Fink Report was very important.

In my reaction to the bird flu incident I thought it was critical to support their conclusions, which was yes, we must envision a time where there will be some things that should not be openly published, and they listed seven categories of concern. Second, we need a mechanism and that mechanism should include both scientists and people who are experts in security issues. And that led to the setting up by the US government of the National Science Advisory Board for Biosecurity, NSABB. And that board should look at papers and make decisions about whether they should be published, not published, or published in a special form with critical information omitted, so-called redaction. And finally, the Fink committee emphasized that the government needs to set up a mechanism so when that happens, and we think it will happen, there is a mechanism to get omitted information to the people with a need to know. Because otherwise how can we have this redaction mechanism?

So over the years, starting in 2005, this NSABB was set up. They never had ruled for not publishing something in full. And then suddenly in our paper [in *Science*] and a paper submitted to *Nature* last fall, they let us know that they thought it should be only published in a redacted form.

04-00:22:56
Hughes: Explain what the—

04-00:22:59
Alberts: The redacted form would say here are our conclusions, but we're not going to give you the exact details that could allow somebody to produce this kind of a virus, which is a bird flu virus. Bird flu is thought to be highly lethal when it infects people, but it's not clear how lethal because we don't have good data. But it's thought to be highly lethal when it affects people. It's very hard to infect people. You almost have to breathe in an infected chicken's lungs, because it doesn't survive in aerosols. The model for human infection with flu viruses, the best model, is an animal called the ferret. And using ferrets, two different groups, one in Wisconsin and one in the Netherlands—and we had the Netherlands paper—showed that with certain mutations introduced into the virus, now it could transmit in air and affect neighboring ferrets in separate cages.

04-00:23:56
Hughes: Which made it much more infectious, right?

04-00:24:01
Alberts: Well, which should make it much more infectious in humans if the ferret's a good model. Anyway, so this is why the NSABB wanted us to say that with appropriate mutations you could mutate the virus so that it can spread in air, so therefore we should be much more vigilant about all these birds and other animals infected with bird flu all around the world—Indonesia, Vietnam, China. But they didn't want us to say what exact changes they should be looking out for because then they thought that a terrorist or somebody could design by reverse genetics such a virus.

04-00:24:41
Hughes: And if I remember correctly, it was only one genetic change. It was a very simple change.

04-00:24:47
Alberts: No, it's more; it's at least five. But three of them people could guess at from what's already been published. So if you're sophisticated it's only two. But anyway, I led an effort in collaboration with Phil Campbell to try to respect their judgment, because we didn't want the NSABB mechanism to disappear. Before that mechanism it was only security people overreacting and making a lot of negative suggestions.

But Phil and I made a unified front that we'd only do this redaction if they had a mechanism, a transparent mechanism, to get the information that was missing to people with a need to know, such as in our case Indonesia, where the virus was taken and mutated and it's circulating all around Indonesia. The Indonesians—that would be an incredible issue for American diplomacy if the Indonesians can't get access to this data that they need to protect themselves. It's just a non-starter. And the government tried very hard for a month or so to try to develop a mechanism. They had lots of meetings, and in the end they weren't able to come up with a mechanism.

They then had a meeting at the World Health Organization in Geneva where this international group said this is not possible. You must publish everything because you don't have any mechanism to get this information only to the people with a need to know. And then the NSABB came back and they had another meeting and they reversed their position, partly based on new information about exactly how easy it is to spread this virus, and what happened to the ferrets. There was a lot of misunderstanding, because the first decision was made only by telephone meetings with the authors and they didn't have enough details. So now they've reversed themselves, and we are to publish it. I promised the committee when I met with them in private that I'd have the last look at the manuscript to make sure that it was clear—they were unhappy with the draft manuscript that they got.

04-00:26:50
Hughes: You're publishing the whole thing?

04-00:26:52
Alberts: So we're publishing the whole thing. In fact, we gave them much more space to explain themselves so there can be no misunderstanding. We're publishing a very long paper with a very long electronic supplement in June [2012]. *Nature*'s already published their paper.

04-00:27:08
Hughes: Oh, has it?

04-00:27:08
Alberts: We were supposed to publish at the same time but we couldn't even get our manuscript. The Dutch decided that they wouldn't let the manuscript out of the country until recently because it was subject to so-called export controls. But then they gave us an export control license to publish the paper finally. Anyway, so we were delayed and *Nature* was not, because their manuscript was not from the Netherlands.

04-00:27:31
Hughes: Are the papers pretty similar in their conclusions?

04-00:27:35
Alberts: Well, they're complementary. They find different mutations.

04-00:27:40
Hughes: That's too bad that they weren't simultaneous publications.

04-00:27:42
Alberts: Well, still the sum of them is better than either one alone. More useful to people. And we're also publishing at the same time a paper showing that in various countries around Asia the virus is only a few mutations away from getting this form that's so dangerous.

04-00:27:59
Hughes: Where were you, Bruce Alberts, leaning in this whole issue? Meaning publication versus redaction?

04-00:28:12
Alberts: I and Phil Campbell were arguing for redaction simply because we wanted to support this mechanism. We weren't making a judgment whether they made the right decision or not. We wanted to support the NSABB, which is the only mechanism we have. Before the NSABB, I was at the Academy in another episode where we were told not to publish a paper in *PNAS* [*Proceedings of the National Academy of Sciences*], which is the Academy's journal, on the danger of *botulinum* toxin in milk. And this was from biosecurity people. Subsequently security people were telling us not to publish genome sequences, all kinds of issues—basically overreacting, at least in the opinion of the scientific community. My fear still is if we get rid of the NSABB then we're back to square one with the security people at war with the scientists. This way they were working together.

If you go back to that *botulinum* toxins paper published in 2005, I wrote a long editorial about why we decided to publish despite the fact that the security people asked us not to. We were formally requested by the US government not to publish. It was an incredible amount of time that we all spent on this.

04-00:29:33
Hughes: But it keeps your brain alive, right?

04-00:29:38
Alberts: Well, that's right. I'm going to finish with *Science* in a year, so maybe my brain will be dying in a few years.

04-00:29:47
Hughes: Well, listen, I can't thank you enough for your time.

04-00:29:48
Alberts: Well, thank you.

04-00:29:49
Hughes: I hope you got some enjoyment out of this.

04-00:29:51
Alberts: Everything's worth it for Dan! [laughter]

[End of Interview]