



Photo by Bob Paz

DAVID BALTIMORE

(b. 1938)

INTERVIEWED BY
SARA LIPPINCOTT

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Subject area

Biology, microbiology, medicine, virology, Nobel Prize, Caltech administration

Abstract

Interview in three sessions, October-November 2009, with David Baltimore, Robert Andrews Millikan Professor of Biology and president emeritus of Caltech (1997-2006).

He recalls his childhood and early education in Great Neck, NY, his aptitude for science, and a summer at the Jackson Laboratory in Maine (1955), which confirmed his vocation. He discusses interest in molecular biology at Swarthmore (B.A. 1960); 1959 summer at Cold Spring Harbor with G. Streisinger; meeting S. Luria and C. Levinthal; graduate work at MIT and Rockefeller (PhD 1964); 1961 summer at Albert Einstein College of Medicine; postdoc at MIT with J. Darnell and Einstein with J. Hurwitz. Recalls his discovery of polio polymerase and demonstration that RNA chains initiate with a triphosphate.

In 1965 he is invited to join Salk Institute by R. Dulbecco; returns to MIT as associate professor in 1968. Recalls his 1970 discovery of reverse transcriptase and copublication with H. Temin; 1975 Asilomar conference on recombinant DNA; 1975 Nobel Prize with Temin and Dulbecco; founding of Whitehead Institute for Biomedical Research; National Academy of Sciences committee on national strategy for AIDS. Comments briefly on investigation of T. Imanishi-Kari for fraud and his return to MIT from Rockefeller University in 1994.

He discusses vetting process for Caltech presidency, his 1998 inauguration, and highlights of his presidency, including purchase of St. Luke's Medical Center, \$1.4 billion capital campaign, and building Broad Center for the Biological Sciences. Comments on Caltech architecture, including Cahill Center for Astronomy and Astrophysics; receiving National Medal of Science in 2000; L. Van Parijs dismissal from MIT and prior work in Baltimore's lab; and the prospects for human enhancement and understanding of consciousness.

Administrative information

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH DAVID BALTIMORE

BY SARA LIPPINCOTT

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with David Baltimore
Pasadena, California

by Sara Lippincott

Session 1	October 13, 2009
Session 2	November 19, 2009
Session 3	November 25, 2009

Begin Session 1

LIPPINCOTT: Let's just get through the early part of your life first. You were born in New York City in 1938.

BALTIMORE: I was.

LIPPINCOTT: Tell me a little bit about your family background—your parents. Were they native New Yorkers? Were they scientists?

BALTIMORE: They were native New Yorkers. My father [Richard Baltimore] grew up an orphan at age fourteen or so, and had one parent for most of his childhood—mostly, I think, due to tuberculosis. His sisters left school and went to work to ensure that he got through high school, and that's all he did.

LIPPINCOTT: He didn't go to college?

BALTIMORE: He didn't go to college. He was a thoughtful and intelligent and well-read man but had very little formal education.

LIPPINCOTT: Was that for economic reasons?

BALTIMORE: The family was just very poor. He actually did start some classes at one point—I never understood that. He's long gone, so I never will.

LIPPINCOTT: Were they in Manhattan?

BALTIMORE: They were in Manhattan or in Brooklyn—they moved around. They didn't have a stable place.

LIPPINCOTT: What did he do for a living?

BALTIMORE: He went into the garment business, and I don't know how he got into the garment business. At the beginning of the Depression, he actually had his own firm and worked in that all through the Depression. My parents married in, I think, 1930, and my mother also worked in the garment business. At one point, they told me that we were never wealthier than we were during the Depression.

LIPPINCOTT: How interesting!

BALTIMORE: Yes. Women's wear. He was in women's coats and suits—or cloaks and suits, as they called it then—so he did very well, because women continued to buy coats even through the Depression. Women in those days needed a new coat every spring and every fall, and people spent what we would consider a disproportionate part of their income on clothes. There wasn't much else to spend it on—there were no dish washers and all of those things in those days. So they did well.

LIPPINCOTT: Did you have siblings?

BALTIMORE: I have a brother [Robert S. Baltimore], who is now a physician-scientist at Yale University. He's been there, actually, for a long time. He's a little younger than I am.

LIPPINCOTT: When did you move to Great Neck?

BALTIMORE: We did not move to Great Neck until the early forties. I was born in New York City [Manhattan], but I was brought up in Queens—in Rego Park and Forest Hills. We were in Rego Park when I was in second grade, and my mother said, “These schools are just not good enough.” I haven’t told you about my mother yet; she was a very different sort of person from my father. So they up and moved to Great Neck. At that point, Great Neck was not what it became, which is a real Jewish enclave. They were pioneers in that regard. But the schools were known to be terrific.

LIPPINCOTT: And it was well off, was it?

BALTIMORE: It was well off. It was *The Great Gatsby*. [Laughter]

LIPPINCOTT: If you were seven, that would have been right after, or at the end of, the war—1945?

BALTIMORE: When they moved? No, they moved in 1943 or 1944. They moved during the war. My father was 4F for some reason, and wasn’t in the war.

LIPPINCOTT: That was lucky. So was my father. He had flat feet.

BALTIMORE: Yes, I think that may be the answer, in fact—flat feet.

My mother, Gertrude Baltimore was a very different sort of person. My mother graduated from NYU. Her parents, although not wealthy—she was on scholarships and whatever—had a stable home. Her father was a tailor. She went to Hunter High School, which was the high school for bright young women in those days; and then she went to NYU on scholarship.

LIPPINCOTT: What was she interested in? What was her—?

BALTIMORE: I think she graduated in English, or something in the humanities. And then in 1943 or thereabouts— My brother was born in ’42, so sometime shortly after my brother was born, she started taking classes at The New School.

LIPPINCOTT: Oh! That was around in those days?

BALTIMORE: Oh, yes. Well, this was the school in exile for the great German philosophers and psychologists; and her interest was in child psychology—experimental psychology, really. And so she took classes from the greats of psychology of the day. She knew Wolfgang Köhler, who was old by then—he was a founder of Gestalt psychology. Hans Wallach, Solomon Asch—these were the greatest of people, and they had all been given intellectual homes on the East Coast, either at The New School or at Swarthmore College. Wallach and Asch were both at Swarthmore—I took classes from Asch when I was at Swarthmore. That’s one of the reasons I went there—there were multiple reasons, but that’s certainly one of them. My mother got a master’s degree at that time in experimental psychology. She did everything for her PhD but write her thesis, and I’ve never known why she didn’t. She had a block in writing. Not that she didn’t write well—she wrote perfectly well—but she didn’t seem to want to write things up. She was not aggressive in pursuing her own position in academic psychology.

LIPPINCOTT: She wasn’t interested in practicing?

BALTIMORE: Oh, no, this was not about practicing. This was all experimental psychology. She was interested in the nature of perception—that was the big question for the Gestalt psychologists—and in other theoretical questions. She worked with animals some, but a lot with children, and developed, apparently, very sophisticated thinking about childhood development. I can’t tell you very much about it. But she had a mentor, Mary Henley, who was sort of the last of the Gestalt psychologists.

LIPPINCOTT: Did she want an academic career?

BALTIMORE: Yes, so she had one. She taught at The New School, once she had finished her training, for quite a number of years [1958-1966] and also at Sarah Lawrence [1965-1977], where she got tenure at age sixty-two.

LIPPINCOTT: Gee! There’s hope for us all!

BALTIMORE: Yes, right! [Laughter] And she was a beloved professor. I say that because I have run into her students occasionally and they spontaneously come up to me and tell me how wonderful working with her was. It was a very unusual program at Sarah Lawrence, where the professors were dons to the women—it was all women then. She was there when men first entered, but it was basically all women. She had a very close relationship with a couple of the students every year, and those are the ones I hear from—how much she affected their careers. Some of them are professional psychologists.

My father died somewhere around 1977. He was alive when I won the Nobel Prize [in physiology or medicine, 1975] but ill. My mother died in 1988—ironically, not too long before I moved to New York. They had an apartment there; they lived in Great Neck until my brother left for college and then, almost to the day, moved back into Manhattan, which is where their life really was. In Great Neck, they were among a group of sort of intellectual people who lived in Great Neck solely for the schools and who all moved back to Manhattan when their kids graduated. They wanted their kids to go to public schools. Some of them were wealthy enough that they could have sent their kids to private schools, but they believed in public schools, so they lived in Great Neck. Some of them, like my parents, weren't that well off and could not have afforded private school, which in those days was a tremendous luxury for the middle class. My parents were comfortable but they were never really very wealthy, and they made it very clear that we could not expect to live up to the standards of many of the people who lived around us.

LIPPINCOTT: How did they make that clear to you?

BALTIMORE: They told us! [Laughter]

LIPPINCOTT: Well, were you happy in Great Neck? Was it a good place to grow up?

BALTIMORE: Umm... [Pause] No, it was not a great place to grow up. It was a little sterile. It was *very* sterile. It was very safe; in that sense, it was a great place to grow up.

LIPPINCOTT: Suburban. Sidewalks.

BALTIMORE: Suburban, sidewalks, walking the dog. It was a nice community, and because my parents did have this group of people they were close to, many of whom lived within a short walk of us, we'd meet them in the evenings on the street. So it was a comfortable, very placid upbringing. There was a pool—Great Neck is broken up into little villages, and there was the local pool and tennis court and whatever, so you really didn't even have to go to camp. Although I did go to camp, for many years.

LIPPINCOTT: Just for the record, what camp did you go to?

BALTIMORE: I went to Camp Turkey Point.

LIPPINCOTT: Where was that?

BALTIMORE: Camp Turkey Point was on the Hudson River between Saugerties and Kingston. It was run by a wonderful couple, Henry and Celia Paley, who were great believers in progressive education and ran a camp that didn't have those—what are they?—color wars, they used to call them. Camps used to be very macho places, where people were taught to compete. That wasn't the style of the Paleys at all; it was cooperativity and the arts. So this was really a very nice place. I don't know how my parents found it, but, you know, many of the camps were run by the Jewish organizations that believed in— The Zionist organizations and the socialist organizations and camps that were really bright red, this was not. This was more in the tradition of, I guess, [John] Dewey and other thinkers about education—not about politics so much.

LIPPINCOTT: Well, tell me about your parents' politics. Were they just liberal, or were they over toward the socialist end of the spectrum?

BALTIMORE: They were sympathetic to socialism. My father, after all, was a capitalist, and he made no apologies about that, but they were believers in equality and what was in the socialist movements. They were proud later, they told me, that they had never joined the Communist Party, that they had seen it for the totalitarian wing that it was.

LIPPINCOTT: Were they religious people?

BALTIMORE: My father was religious. He was brought up as an Orthodox Jew and went, by choice, to Orthodox synagogues. My mother was a vehement atheist. Not vehement—she was never really vehement about anything. She was a committed atheist. But she was also committed to her family and to my father's right to have his religion, and we celebrated the major holidays, we fasted on Yom Kippur, and I walked with my father to the shul, which was a long walk from where we lived.

LIPPINCOTT: But you didn't go to the synagogue every week.

BALTIMORE: Well, I did, because I had a bar mitzvah and I had to be trained and all of that. But the day after my bar mitzvah, I left any thought of religion behind. I never really *had* any thought of religion; I was doing it because it was expected.

LIPPINCOTT: Cultural.

BALTIMORE: Cultural. Since that day, I've virtually never been to a shul, except for, you know, weddings and stuff.

LIPPINCOTT: How about Great Neck High School?

BALTIMORE: Great Neck High School was a famous high school in its day. It was the best public high school, perhaps, in America. Its great rival was Scarsdale High School.

LIPPINCOTT: Was it large?

BALTIMORE: Great Neck High School was fairly large. In fact, not long after I left, they opened Great Neck South and split it into two. I would have gone to South; my brother went to South. But I went to Great Neck North, which was a remarkable school of its day. They taught a fair amount of science. They taught it in a very rote and unenlightened way, but the fact that they taught it at all was a plus. In those days, we didn't have AP [advanced placement] courses and that kind of stuff; but they helped those of us who had obvious talent to move ahead a little. And

I remember a big thing was that I took some calculus when I was still in high school. Just a little—but today, you know, you can take AP calculus.

LIPPINCOTT: Was that when you first became interested in science, and what kind of science grabbed you, at the beginning?

BALTIMORE: Well, I don't know the answer to that. I've thought about that, and I've decided that I simply don't remember. [Laughter] I think the answer is "None." What I knew was that I was very good at it. I can remember one year in which I, just as a matter of protest, did all of the homework for mathematics—for I don't know, the year, or the semester, or something—in the upper corner of the paper. You know, a lined piece of paper has a little blue line running down it, so I did it all there. I simply wrote down all the answers, without having to do any calculations. It all came very easily to me.

LIPPINCOTT: That's mathematical. But—

BALTIMORE: That's right, and the science, similarly, was not hard. Understanding chemistry; and I think we had a biology course, but I don't remember what was in it. But we certainly had chemistry and physics. And because my mother was an academic psychologist, I took some interest in psychology and had read a little bit of the stuff that she cared about—particularly,... [Pause] Who was the great child psychologist at that time? Someone whom she was actually an exponent of and really sort of translated for people. Umm....

LIPPINCOTT: It will come to you.

BALTIMORE: Well, it may and it may not. Anyway, he was European, and he wrote in French. You don't hear about him anymore. At the time he was a pop figure, almost. He worked with young children. [Jean Piaget] He had a whole series of theories about how they created their view of the world. He was one of those names that everybody knows, like [Steven] Pinker. So I read a little bit of what was around the house, and then my mother walked in one day when I was a junior in high school and said, "You know, there's this program that I've heard about"—it was

on the bulletin board or something at The New School—“at the Jackson Lab, in Bar Harbor, Maine, and maybe you’d want to go and spend six weeks there this summer.”

LIPPINCOTT: At that time [1955], it was a big biochemical lab, wasn’t it?

BALTIMORE: Genetics. It is the home of mouse genetics—developed by C. C. Little, in the twenties.

LIPPINCOTT: Had you been interested in biology at that point?

BALTIMORE: Well, this is what I don’t know. I really don’t know whether that was arbitrary. That is, it was science and it sounded like fun, and I didn’t have any particular plans for the summer. And whether it was important to me that it was biology and not chemistry or something else, I really don’t know.

LIPPINCOTT: That’s interesting. So you don’t have a—?

BALTIMORE: I’m not one of these guys who came to biology because I was a bird watcher, or anything of the sort. No. In fact, the kind of science that I’ve done focuses on the chemistry of biology, not on the phenomenology of biology. So I ended up, for structural reasons, being a chemistry major at Swarthmore. And when I talk to the kids here at Caltech, I tell them, “The best background you can get for biology is chemistry,” because that’s the way I look at biology—as kind of a process.

LIPPINCOTT: Well, when you went up to the Jackson Lab, you met Howard Temin, is that right?

BALTIMORE: I did.

LIPPINCOTT: Was he a kid, too? You’ll pardon the expression.

BALTIMORE: Well, no, he was not. I was a high school student. This was part of the high school program which is housed in an old mansion, Highseas. Howard had just graduated from

college at that point; he was four years older than I was. He's dead now, of course; he died quite young [1994]. He was sort of the guru of the high school program. If you had a question, you went to Howard, and he always knew the answer. Howard knew everything. I mean, he was famous at Swarthmore—he went to Swarthmore—for knowing everything.

LIPPINCOTT: How many other high school students were there that summer with you?

BALTIMORE: It's a program that runs every summer. There are twenty-five or so students.

LIPPINCOTT: And it was a couple of months that you were there?

BALTIMORE: Yes, six or eight weeks.

LIPPINCOTT: What kind of work did you do there?

BALTIMORE: It was a structured program. There were lectures, from the staff and from visiting scientists, every day or two. And then there were three research projects that you did, with three of the staff scientists or summer visitors. They were little projects; they dealt with some aspect of genetics—coat-color mutants. You couldn't really *do* much genetics, because a mouse takes three months to breed, so you can't get many generations in a summer.

LIPPINCOTT: But they taught you techniques, I guess.

BALTIMORE: They taught us techniques—the techniques were simple. For instance, I measured lengths of bones from skeletons of mice, because the man I was working with was interested in seeing whether there were genetic differences of some sort—I don't remember exactly what he cared about. I used calipers and measured bone lengths and looked up some statistics on it and whatever. And I did something with the great Tibby Russell, who was one of the founders of the genetics of the blood system.

LIPPINCOTT: That's Elizabeth Russell?

BALTIMORE: That's Elizabeth Russell, known to everybody as Tibby. She was fabulous; she was sort of a mother figure to everybody and cared about people in a personal way. She was terrific.

LIPPINCOTT: Did you know anything about what [James D.] Watson and [Francis] Crick had done in '53 at that point?

BALTIMORE: Well, this was '55. When I asked Jim Watson, here at Caltech when I interviewed him [May 2003], what the response was to the discovery [of the structure of DNA] from the rest of the scientific community, he had one word: "Silence." The Watson and Crick discovery was not appreciated for the landmark that it was for quite a number of years. So in '55, when I was at the Jackson Lab, you would have had to have been a committed DNA biochemist or something to even be aware of what Watson and Crick had done.

LIPPINCOTT: Even after the little letter in *Nature* with the famous sentence in it?¹

BALTIMORE: That's right.

LIPPINCOTT: That's amazing.

BALTIMORE: That's right. I said, "Weren't people calling you to give seminars and stuff?" and he said, "No." The first person to really understand it was Max Delbrück, and Jim was asked to come here as a postdoc by Delbrück and was on the faculty here [as a senior research fellow] for a relatively short while, until he was called back to Harvard. He couldn't find girls here, so he was not happy. [Laughter] I'm quoting him. But Max understood it. And I think you can date the change—that kind of hockey-stick moment—to the Meselson-Stahl experiment [1958]. The Meselson-Stahl experiment was done here at Caltech by a research fellow and a postdoc: Matt Meselson and Franklin Stahl.

LIPPINCOTT: Could you just summarize it a little?

¹ "Molecular structure of nucleic acids," *Nature* 171: 4356, 737-8 (1953).

BALTIMORE: Yes. That experiment showed that when DNA duplicates, you open the helix and you duplicate the individual strands. There's a whole book about that experiment, which was published by this wonderful historian of science at Yale who died a few years ago, called *The Greatest Experiment*, or something like that.² And many people considered it the greatest experiment. When I invited a great geneticist, George Streisinger, to come to Swarthmore—I was the head of a little biology club and I invited George to come and give a lecture, because I'd worked with him at Cold Spring Harbor—George came and said, "I don't want to talk about my own work. The greatest experiment in biology has been done just recently, by Meselson and Stahl, and I want to explain that experiment." And he got up to the board and just simply spent an hour explaining that experiment.

LIPPINCOTT: So what they showed was what—

BALTIMORE: The big question was, If the helix is in fact wound around itself, the information is at the center—because the information is the base pairs. How do you get at the information? And the notion that you can take apart a chemical—

LIPPINCOTT: That it unzips?

BALTIMORE: That it unzips—was very hard for people to accept. Now, Watson and Crick accepted it very easily.

LIPPINCOTT: They saw it.

BALTIMORE: They saw it and they made a picture of it, but there was no experimental verification that the helix came apart. You know, in the history of science it's interesting how experiments that are not necessarily the most insightful somehow strike the community as the thing they want to know. If they know that, then they accept a whole realm of theory around it. And the Meselson-Stahl experiment was that. It gave people the courage to believe that the '53 paper had it right.

² Frederic Lawrence Holmes, *Meselson, Stahl, and the Replication of DNA: A History of "The Most Beautiful Experiment in Biology"* (New Haven: Yale University Press, 2001).

LIPPINCOTT: That's interesting.

BALTIMORE: Yes. It's a very very important thing, and Caltech was at the center of all of this.

LIPPINCOTT: So, back at the Jackson Lab, there was no—?

BALTIMORE: I don't know that anybody even mentioned it.

LIPPINCOTT: Were you taught anything about DNA, the nucleic acids themselves, at that point?

BALTIMORE: Not there, no. Not in high school.

LIPPINCOTT: And not at Jackson, either?

BALTIMORE: I don't remember hearing anything about that. I do remember a wonderful night that we spent with a man named Francisco Duran-Reynals. He believed that cancer was caused by a virus. In 1955 that was heresy, for all intents and purposes. And both Howard Temin and I sat there and listened to him, and I think there's no question but that neither of us ever forgot that.

LIPPINCOTT: That's interesting. Where was this Duran-Reynals from?

BALTIMORE: He was Spanish. He had a summer home in Bar Harbor.

LIPPINCOTT: Is that why he came over to the lab?

BALTIMORE: Yes. He was a professor at Yale. Had a very cute daughter. [Laughter] So he came over one night. He was asked, "Would you sit with the high school students and tell them about what you're thinking?" And so he did that.

LIPPINCOTT: Do you happen to know why he had that notion? Was he a virologist?

BALTIMORE: No, I don't remember the details or exactly how he fit into it. He was not a great research scientist, I don't think—at least not in cancer research. His name never turns up these days.

LIPPINCOTT: What was Temin like?

BALTIMORE: Oh, Howard was a very, very brilliant man—thought about everything, was aware of everything, loved intellectual life. He was a tall—not terribly tall—thin man his whole life. Constantly on edge; you just felt he was ready to explode at any moment.

LIPPINCOTT: Was he somebody you were particularly friendly with at the Jackson Lab?

BALTIMORE: I wasn't particularly friendly with him, in the sense that he was four years older than I was. At that point, that was the difference between seventeen and twenty or so. He was on his way *out* of college, so his conferees were the college students. We became friendlier many, many years later.

LIPPINCOTT: Well, by this time had you decided where you wanted to go to college?

BALTIMORE: No, I hadn't. This was between my junior and senior years in high school, and so that was the summer to begin to make that decision and to go visit in the fall and make applications.

LIPPINCOTT: What turned you toward Swarthmore?

BALTIMORE: I had already, I think, visited Swarthmore with my mother.

LIPPINCOTT: And she was interested because of the psychologists who were there?

BALTIMORE: She wanted to take me around to various schools and look at things, and there wasn't a lot of pressure to go to Swarthmore. But because she knew some of the senior members of the faculty there, she could give me an introduction. I remember meeting Hans Wallach at

that time, and he said, “What do you want to do?” and I said, “Well, I’m thinking about various things, like psychology.” He said, “Don’t do psychology.” [Laughter] “Learn physics and math and you can do the other sciences later.” So I had that introduction. Then Howard had been at Swarthmore, so this guy whom I admired enormously spoke well of Swarthmore. And then I had a girlfriend that summer who was going to Swarthmore that fall. She was one year ahead of me—Jan Tollman, who became a well-known scientist later. So I had to go down and visit her, and she was a little embarrassed to have a high school student visit her. There was another guy there, at that summer program, called Paul Trachtman, who was going to Swarthmore. Was he in my class at Swarthmore, or was he a year ahead? He came from Brooklyn. I don’t remember now whether he was already at Swarthmore or whether he was looking at Swarthmore. But there were a lot of coincidental interactions with people who were going to Swarthmore.

I knew I wanted to do science. At the end of that summer, there was no question in my mind about what I wanted to do for the rest of my life. And I’ve done it—as opposed to most people, who make decisions like that and then do something else. I fell in love with experimental science—experimental biology—and that’s all I wanted to think about or do.

So, why go to Swarthmore? And the answer was that I had a lot of other interests in my life. I loved literature. I loved being with people who cared about things that were different from what I cared about. I had a sort of hankering for Greenwich Village and whatever—which is to say that I cared about things beyond science.

LIPPINCOTT: Yes, things that were going on in the society.

BALTIMORE: In the world, in the society, right.

LIPPINCOTT: Did you think about NYU, then?

BALTIMORE: No. No. So I ex’d out MIT, where I could have gotten in easily—I had all the grades and everything else you need.

LIPPINCOTT: Why did you reject MIT? It’s near Boston.

BALTIMORE: I did not want a technical school. And of course I never considered Caltech because California was a foreign country.

LIPPINCOTT: Oz.

BALTIMORE: Yes. I just never conceived of it. Howard actually went to Caltech from Swarthmore, which was very brave of him [laughter], because he grew up in Philadelphia. He'd never been outside of the East, I don't think.

LIPPINCOTT: Where else did you apply?

BALTIMORE: I applied to Harvard, and I went and visited Harvard, and I didn't like it. I didn't like the atmosphere in the freshman dorms. I stayed with a high school friend who had been a year ahead of me, in a dorm there. I just thought people were not serious. I was pretty serious—I guess I still am. This just didn't seem right to me, and I didn't think that they challenged people enough. It was too easy to get through Harvard if you wanted to.

LIPPINCOTT: Well, the Biology Department wasn't big in molecular biology at that point.

BALTIMORE: No place was.

LIPPINCOTT: Except for Cambridge University.

BALTIMORE: That's right. When I went to *graduate* school, there was almost no place to go. I'll get to that. No, that was not the issue.

LIPPINCOTT: I do know that Watson was disgusted with Harvard.

BALTIMORE: That was much later. He only went to Harvard in '56, and he was perfectly happy with Harvard until the seventies, when he tried to do new things and move Harvard in new directions and they were immobile.

LIPPINCOTT: Well, I was in Harvard in '59, just working for the former chairman of the Biology Department there.

BALTIMORE: Who was that?

LIPPINCOTT: Frank Carpenter. Well, you wouldn't know about him, because he was a morphologist, a paleoentomologist. And I only say that about Watson because he was considered the bad boy of the department and all the grandees disliked him—because he had contempt for them.

BALTIMORE: Yes. He was disrespectful.

LIPPINCOTT: Yes, and he thought that they were not engaged in interesting or important work. They were still looking at wing venation—

BALTIMORE: Yes, but he was able, at Harvard, to bring in a cadre of molecular biologists and create molecular biology. So nothing was in his way. I mean, he had disdain for all those other people. And Ed [E. O.] Wilson, famously—but he had Wally [Walter] Gilbert, whom he attracted over from physics; he had Mark Ptashne, who came there as a faculty member; he had a number of others. So he created a school of molecular biology. What he couldn't do was then move that into mammalian biology, and his frustration was that he wanted to do cancer research; he saw that the future was in cancer research, and he could not convince his colleagues or the administration—particularly the administration—to make a commitment to a cancer center, to a new kind of mammalian biology, which we did at MIT. Jim was always jealous of what we were doing at MIT; he said it, at the opening of the MIT cancer center [1973]. He said, "You're doing the right thing. I was never able to do that at Harvard, and it's Harvard's loss, and you guys are in the forefront."

LIPPINCOTT: Is that why he went to Cold Spring Harbor?

BALTIMORE: Yes, that's why he went to Cold Spring Harbor. Because at Cold Spring Harbor he had the freedom to create what he wanted to create—and would have created it at Harvard, if they'd let him. And Harvard would be a different place today, in that kind of biology.

LIPPINCOTT: Anyway, so you decided you didn't want to go Harvard.

BALTIMORE: So I didn't want to go to Harvard. And, you know, you make these decisions on a whim. One day in the dormitory overnight is not the way to make a decision.

LIPPINCOTT: But that's what people do.

BALTIMORE: That's what people do. And there was a very strong positive attraction to Swarthmore. Beautiful campus—and I had all these people I knew who were going there or had been there.

LIPPINCOTT: It was co-ed?

BALTIMORE: It was co-ed from its inception in 18-whatever it was.

LIPPINCOTT: Was it Quaker?

BALTIMORE: Quaker. The Quakers set up three schools, one for women, one for men, and one co-ed: Byrn Mawr, Haverford, and Swarthmore.

LIPPINCOTT: How about the science there, at that time?

BALTIMORE: I figured the science would take care of itself. There was perfectly good science at Swarthmore but not outstanding, because they didn't have a research tradition. Although even in biology, they had a guy who ran biology—[Robert K.] Enders, who did a lot of research, especially in the summers. He went out to the Rocky Mountain Biological Laboratory in Colorado and sort of ran that program, I think. A number of the faculty ran research programs,

but they were very old-fashioned: embryology, descriptive biology, diatoms. None of it was molecular; none of it was biochemistry.

LIPPINCOTT: How about genetics?

BALTIMORE: There wasn't much formal genetics of any kind. It was not forward-looking, let's put it that way. Whereas Haverford and Bryn Mawr were much more forward-looking—particularly Haverford. It had a cadre of young faculty who had been trained in the last five years basically for that and who were creating the future. Now, these were all men who were comfortable going to a liberal arts school, an undergraduate school, without a graduate program. So they worked their whole careers with undergraduates. They were wonderful teachers. Some of them have made a reputation for themselves in science, but most of them were just better known as teachers; I got something from them. And I used the libraries at both Bryn Mawr and Haverford which were much richer than the library at Swarthmore in the areas I cared about. So I discovered molecular biology—discovered what was going on there.

LIPPINCOTT: And you taught it, actually, didn't you—in your senior year?

BALTIMORE: In my senior year I taught it.

LIPPINCOTT: That was unusual, was it not? To have an undergraduate teach?

BALTIMORE: Well, I didn't teach a formal course. It was a sort of evening course; it was the halt leading the blind. There was this little group of us who really saw that this was happening and wanted to be participants in this future. And I had gone to Cold Spring Harbor after my junior year, in the summer of 1959.

LIPPINCOTT: Let's talk about that. I read that you were in the first class of the Undergraduate Research Program at Cold Spring Harbor.

BALTIMORE: URP. Right.

LIPPINCOTT: How big a program was that? Were there quite a few students in it?

BALTIMORE: Oh, I don't remember, but it was maybe a dozen students.

LIPPINCOTT: You had to apply?

BALTIMORE: You had to apply—but this was a wonderful story: Here I am now, a junior in the honors program at Swarthmore, in biology. The honors program at Swarthmore was this fabulous program where you met once a week with the other students in the seminar for two years, and at the end of two years you had an exam on everything you'd done over those two years. And then it was decided whether you would graduate with honors, high honors, or highest honors—sort of like at Oxford. About half the students were in honors. The program is nowhere near as lovely as it was then because the world has changed. But this was remarkable. And then the people who examine you are outside examiners. Howard Temin is famous for his honors exam, among other things, because he walked in the room, sat down on the table in the room and said to the examiners, “You're the most prestigious group of people we've had at Swarthmore. I have some questions for *you*.” [Laughter] Another famous thing about Howard is that he had read everything in the Swarthmore library and they had to buy books to keep him busy.

LIPPINCOTT: Is that true?

BALTIMORE: I don't know if it's true or not.

LIPPINCOTT: Sounds apocryphal.

BALTIMORE: Yes, it sounds apocryphal, and there are certainly books in there he wouldn't have read. Anyway, the honors program was a fabulous program; it allowed you really to develop independent education. So we were able to take these seminars into directions where the faculty had no idea what we were talking about, because we were going off and reading journals in Haverford and Bryn Mawr, and reading review articles. And we would come in with questions, and these guys would look at us and say, “You know, we have no idea. Go out and read something more and decide for yourself what's right.”

LIPPINCOTT: Were you reading about bacteriophages then?

BALTIMORE: Yes, we were reading a lot about bacteriophage. We were reading a lot about cell biology. We were reading about everything. The great debates were going on about regulatory biology; the Jacob-Monod experiments were coming out then. There was a great naysayer, Sol Spiegelman, and I remember that I was unable to figure out whether to believe Spiegelman or to believe Jacob and Monod—because Spiegelman is actually a great writer and a very persuasive person and was given a lot of respect. He was in the review articles and so on. When I came to MIT as a first-year graduate student [1960], one of the first questions I asked one of the faculty was, “What am I supposed to believe?” And [laughter] they said, absolutely off-hand, “Spiegelman’s nuts. Don’t listen to a thing he says.” Now, there was nobody at Swarthmore who could tell me that. That was actually a great lesson to me in the value of mentorship by people who know what they’re talking about.

So, as I say, it was the halt leading the blind. And this group of us tried to educate each other; we all took the same seminars. Many of those people— Well, one of them is Jim Strauss’s [Bowles Professor of Biology, emeritus] wife, Ellen Strauss.

LIPPINCOTT: Don’t know her.

BALTIMORE: You see them at the opera all the time; they’re constantly there. They’ve seen *Siegfried* four times. [Laughter] And they travel around the world going to Ring cycles and stuff. Ellen was one of this group. She never had a faculty position; she married Jim and worked in his lab for her whole career and retired there. David Denhardt, who is a very important scientist; David Teller, who’s a faculty member at the University of Washington; a couple of others. So we had this little cadre of people, and that’s who I was quote-unquote teaching molecular biology to, because I had gone to Cold Spring Harbor that summer.

That’s what I was going to tell you about. I was taking a microbiology course, and I said to the professor, who was this wonderful man who worked on diatoms—but he knew how to let us learn by ourselves—

LIPPINCOTT: Was this Enders?

BALTIMORE: No, this wasn't Enders. This was another guy, whose name I can't remember. No, Enders was positively opposed to our going into this stuff.

LIPPINCOTT: Why?

BALTIMORE: "You can learn that stuff in graduate school," he said. "You should study the parts of a frog." He was really an impediment to what I wanted to do. I never took a course with him. And I got out of biology partly because of him, and graduated in chemistry because they let me do an experimental thesis my senior year; and the biologists wouldn't.

LIPPINCOTT: How interesting! The name of this other professor will come to you, and you can add it.

BALTIMORE: I don't know if it will or not. So, in the spring of my junior year, I'm taking this microbiology seminar; and I said to the professor, "Can we at least see what a bacteriophage plaque looks like? Can we make a lawn of bacteria, and plate bacteriophage?" He said, "I don't have any bacteria. I don't have any bacteriophage. But if you'll bring some, we'll do it."

LIPPINCOTT: A plaque—that's a lot of dead bacteria that have been killed by the phage?

BALTIMORE: Right. But they're beautiful, symmetric little plaques, and you use them to count the number of phages you have. That's why phage was an object of such interest: because you could quantitate the number of these things by the number of plaques on a lawn of bacteria.

LIPPINCOTT: And he didn't have any?

BALTIMORE: He didn't have any of the stuff. He knew that this was the thing to work on, but he didn't care about it.

LIPPINCOTT: There was no lab work connected with this seminar?

BALTIMORE: We did some other lab work, but not this—maybe not for this seminar. Anyway, I knew that Cold Spring Harbor was a place where they would have this, so I said, “If I go to Cold Spring Harbor and get phage and bacteria, couldn’t we do this?” He said, “Yes.” So at Easter vacation—this was just down the road from Great Neck. I went home, got in the car, drove out to Cold Spring Harbor—

LIPPINCOTT: Had you been there before?

BALTIMORE: I’d been there before to go to the fish hatchery with my father, but I didn’t know anything about it as a scientific place; except I’d been reading all these papers and so many of them came from Cold Spring Harbor, and it was clear that this was the center of the world. I was taking a cell biology course, I guess, and reading the work of a woman named Helen Gay. She was the one name I knew who was on the Cold Spring Harbor staff; because most of the people were there in the summers, and the famous things that went on were done by people who were actually professors elsewhere. Here I am in April, or whenever, and Helen Gay was the only name I knew. So I drove out there, and I said, “Can you tell me where I can find Helen Gay?” I may have called ahead, I don’t remember. So she met me, and we talked for a while, and then I said, “I would like to get this phage,” or whatever, and she said, “Well, that’s not work I do”—she was an electron microscopist who looked at chromosomes and stuff. I said I knew that, but was there somebody else on the staff—? Because, as I said, I knew the famous people who had been there, but here we were in April. She sent me to George Streisinger, who was on the staff. He was upstairs. And I walked into George’s office. George was the most self-effacing, most brilliant of men, and also quirky as could be.

LIPPINCOTT: Quirky how?

BALTIMORE: Well, for instance, at that point he was hooked on Coca-Cola; he would drink endless Coca-Colas. So I walked in, and the first thing he said to me was, “Would you like a Coca-Cola?”

LIPPINCOTT: He was a young man? He’d been a Caltech postdoc, I think, right?

BALTIMORE: That's right. He'd been a Caltech postdoc. He was probably in his thirties at that point and was one of the phage group—one of the least known of the phage group, because he absolutely wouldn't publish. Once he knew what he wanted to know, he was totally uninterested in writing it up, and his whole bibliography is twenty papers or something.

LIPPINCOTT: Do you know whom he worked with when he was at Caltech?

BALTIMORE: Max [Delbrück]. He was part of Max's group. And I can't tell you what he did because I'm not sure there's any record of what he did. But he knew everything, and he really understood phage—he understood the questions.

LIPPINCOTT: How did he react to you, coming in as a—?

BALTIMORE: He was very kind. Here's this kid walking in who doesn't know anything. He said, "Yes, I can give you phage," and he did. So I went back and we actually grew plaques at Swarthmore. He said, toward the end of our conversation, "What are you doing this summer?" I said I was going to Haskins Laboratories in New York. I had been at Haskins before, and I was going to work there for the summer.

LIPPINCOTT: Is that at Mount Sinai?

BALTIMORE: No. I had worked at Mount Sinai, and through that had heard about Haskins. Haskins was a small private laboratory named for Caryl Haskins, who was the head of the Carnegie Institution of Washington and who built this laboratory, which was focused a lot around training students. Many, many high school students went there—and college students. But in New York, many of the bright high school students would go there. And the faculty there was terrific—some really wonderful people, like Corky [Sheldon] Aaronson, did microbiology, parasitology. I think it was really focused on parasitology.

LIPPINCOTT: And you were planning to go there.

BALTIMORE: And I was planning to go there. I had an acceptance from, I guess, Corky Aaronson, who was working there—I can't remember. I said to George that I was going there. He said, "Well, we have a program here starting up. This will be the first year of it"—the URP program—"and I'm pretty sure I can get you a position in it, because we've just gotten the money for it and we haven't had much of a chance to advertise it." I almost fainted.

LIPPINCOTT: I bet! You must have been very turned on. [Laughter]

BALTIMORE: Oh! The whole idea of going to Cold Spring Harbor in the summer—to me, that was the mecca, because everybody wrote about it and it was well known. So I called whoever it was at Haskins and said, "I have this opportunity to work at Cold Spring Harbor," and they said, "Take it! That's an opportunity you shouldn't miss." So I went to Cold Spring Harbor.

LIPPINCOTT: How many weeks were you there?

BALTIMORE: Well, I was there from the beginning of the summer till the end of the summer. Probably three months.

LIPPINCOTT: Did you live at home or—?

BALTIMORE: No, I lived there.

LIPPINCOTT: And you worked with Streisinger?

BALTIMORE: Wait a minute! Did I commute to home? I went home a lot, I know, because it was a short drive and I had a car. But where would I have lived, if I lived there? I don't have an image of living on the campus, yet I think I must have.

LIPPINCOTT: They must have had some housing.

BALTIMORE: They had housing for the students, so wherever it was, I probably lived there.

LIPPINCOTT: You probably spent most of your time in the lab.

BALTIMORE: I spent all of my time in the lab. I did a little project, and at the end of the summer, George said—this was his form of publishing—he said, “I would like you to present your work to [Jun-ichi] Tomizawa and [Alfred D.] Hershey.” These were the great men whom George admired, who worked downstairs from me. Hershey later won the Nobel Prize [1969]. Tomizawa, who is not as well known, was a Japanese who worked a long time at NIH [National Institutes of Health] afterwards. Anyway, we went downstairs, and I presented the work, and they had a few questions, and they said, “That’s nice work.”

LIPPINCOTT: What was it on?

BALTIMORE: It was on the requirement for protein synthesis for the replication of incoming phage, I think; something like that. And I had some P³² experiments, and I had done some genetic experiments, and I had a convincing answer, which I’ve now forgotten. It was a completed piece of work; it could have been published in its day; it was certainly on a topic that people were interested in. But George didn’t care about that; all George cared about was, “What does Hershey think of it?”

LIPPINCOTT: You met Salvador Luria that summer?

BALTIMORE: I met Luria; I met [Cyrus] Levinthal—everybody came through. I met Max [Delbrück]; he came through.

LIPPINCOTT: Is that where you first met Delbrück?

BALTIMORE: Yes. And Luria said to me that summer, “Come to MIT for graduate school.”

LIPPINCOTT: Yes, he had just formed a program in microbiology there.

BALTIMORE: He had just formed the program there. He was looking for students, he and Levinthal. Luria was a microbiologist of course. Levinthal was a geneticist—later did a lot of work in protein structure; that’s what he was famous for. Died young.

LIPPINCOTT: What did you think of Luria? What was he like then?

BALTIMORE: Luria was just wonderful. He was constantly bubbling with enthusiasm, in his Italian accent. There was nothing imperious about him; he was just totally in love with what he was doing.

LIPPINCOTT: How did he come to know your work there?

BALTIMORE: Well, he knew George very well, and he was around, and he was looking for bright students. And George must have said to him, “You should convince David to go to MIT.” So I looked around in my senior year for where to apply to graduate school or whether to just accept the opportunity to go to MIT. One of the only places that seemed to offer anything was Rockefeller and, unfortunately, I think four students had gone from Swarthmore to Rockefeller the year before and had had a terrible time for a variety of reasons. It may have been mostly their own fault. And had left—well, one of them stayed and the others left. So I was worried about Rockefeller.

LIPPINCOTT: Who was at Rockefeller at that point, do you remember? I mean, Luria would have been the big draw at MIT, but—

BALTIMORE: Luria was a big draw at MIT—and Levinthal, who was known as one of the bright young guys in the business. And Alex Rich; he was more biophysical. Vernon Ingram, who discovered the mutation in sickle-cell anemia and was a world-famous English biochemist, was at MIT. They had a great biochemistry division—Jack [John M.] Buchanan and others. No, it wasn't just Luria. MIT really had done it. What Luria did was put them on the map, but they had already made investments in these other people, from about '55 on. Luria came there in '58-'59 on sabbatical and then stayed, was convinced to stay. He was the driving force to move into the more genetically-oriented and microbiologically-oriented work.

LIPPINCOTT: He had been at Indiana University?

BALTIMORE: No, I think he came from Illinois. He had been at Indiana and then Illinois, and then came to MIT.

LIPPINCOTT: So you looked at Rockefeller and you looked at MIT. Anywhere else?

BALTIMORE: I glanced at Harvard, but I just couldn't see what was going on there.

LIPPINCOTT: Nothing. [Laughter]

BALTIMORE: Well, no, they had some—

LIPPINCOTT: They had Watson there by that time, and I think Seymour Benzer was thinking of going there, but didn't.

BALTIMORE: Seymour thought about everything, at one point or another. Only when he found Caltech did he realize what he wanted to do.

LIPPINCOTT: Did you think about Caltech at that point?

BALTIMORE: I knew about Caltech—Howard [Temin] had gone there. I probably read as many papers from Caltech as from any other school. The stuff that was being published by Max's group—[Robert] Edgar, [Charles] Steinberg, [Frank] Stahl—was the basis of the course that Levinthal taught at MIT in genetics. But California was just inconceivable to me. I'd never been to California. I'd never been off the East Coast. It was just words—it had no reality.

LIPPINCOTT: You were an Easterner then.

BALTIMORE: I was an Easterner; I just didn't think about going to Caltech. So I went to MIT, and at MIT I made—which I'm sure I've described at great length—a decision to work on animal viruses.

LIPPINCOTT: And you worked in Cy Levinthal's lab?

BALTIMORE: I worked in Levinthal's lab doing some phage experiments: T-odd phages, for some reason.

LIPPINCOTT: And then you gave up phages, didn't you?

BALTIMORE: I said to myself, "You know, I could do a thesis in phage," but I really—and I think this goes back to the experience I had with mice at the Jackson Lab—really thought I wanted to do mammalian biology. I didn't want to do bacterial or phage biology; it seemed to me that those had—not run their course, but that the interest was going to move to mammalian biology. Because that's, after all—

LIPPINCOTT: The point.

BALTIMORE: That's what we care about, right. And the mouse was the model. But you couldn't *do* much in those days. This was 1960, and you couldn't do much molecular biology. You could do a little, but it was very difficult. So I thought, "Maybe animal viruses will do for mammalian biology what bacteriophages did for bacteria," and that that was the entry to it. I went to Levinthal and asked him, and he said, "Yes, I think that's an interesting question. I've been thinking about that question, too." Then I went to Luria, and he gave me the advice that I followed. He said, "I don't know whether you're right or wrong, but why don't you investigate it? I know a young guy at Albert Einstein [College of Medicine]. You could go work with him for the summer." We're now in February or March or something.

LIPPINCOTT: So that would be the summer of '61, when you went to Albert Einstein?

BALTIMORE: Right. I went to Phil Marcus's lab there, and Luria arranged for me to take the animal virus course at Cold Spring Harbor. That was something you could do, as a starting graduate student, in those days; those courses later became only for much more advanced people. But somebody at Cold Spring Harbor was smart enough to realize that this was looking to the future—to have an animal virus course.

LIPPINCOTT: What viruses did you study? Poliovirus?

BALTIMORE: Polio and its relatives; influenza; the viruses that cause cancer. Harry Rubin came from Caltech to help us with the cancer virus work because that was a specialty at Caltech.

LIPPINCOTT: Oh. But did they even know what a cancer virus was back in those days?

BALTIMORE: Oh, yes. Cancer-inducing viruses were first discovered by Peyton Rous in 1911.

LIPPINCOTT: Oh, that's right—Rous sarcoma virus.

BALTIMORE: Rous sarcoma virus.

LIPPINCOTT: So that was Richard Franklin's course?

BALTIMORE: Richard Franklin and Ed Simon taught the course. And from the experience that I had had with Phil Marcus and then working with Franklin and Simon and the other students in that course, I said, "This field is ripe for attack, and Dick Franklin is the guy who really knows what's going on." So I convinced Luria to recommend me to move to Rockefeller, where Richard was an assistant professor. Now, Luria having gone out on a limb to get me to MIT, it's pretty remarkable that he personally recommended me to Detlev Bronk, who was the president at Rockefeller and who accepted every student personally.

LIPPINCOTT: Luria must have been disappointed to lose you.

BALTIMORE: He said he was. I think he was. He helped me get back to MIT, later on. No, it was all very cordial, and he understood exactly what I was saying. There was one animal virologist coming to MIT that fall, and I met him at Cold Spring Harbor—Jim Darnell. And he doesn't really remember this; I've talked to him about it. I asked him, maybe not very precisely, "Will there be a chance to work in your lab when you come to MIT?" And he started telling me about all the people he was committed to and that it wasn't clear, but that "We'll talk about it when I get there," or something. So I didn't feel like I had a clear path at MIT to do animal virology. I didn't know Jim—I later worked with him but didn't know him at that time. And I

did know Franklin, and he and I really got on wonderfully. I understood the questions he was interested in, and he could help me understand questions I was interested in.

LIPPINCOTT: Is he still with us?

BALTIMORE: He is still with us, but he is long retired from the Biozentrum in Basel, Switzerland. He lives in Basel and travels Europe going to opera houses.

LIPPINCOTT: [Laughter] Well, that's nice! So at Rockefeller you began working with someone named Fritz Lipmann?

BALTIMORE: I did work with Fritz Lipmann. He's not just "somebody named Fritz Lipmann." Fritz Lipmann was one of the seminal biochemists of the twentieth century—a remarkable man. My friend Jimmy [James H.] Schwartz said of Fritz Lipmann that he was plugged into the universe. It wasn't clear that he understood things analytically, but he sure got them right. There was also a little bit of a language barrier there. He was a German refugee.

No, I worked with Richard, and my work became more and more biochemical. There were people in Lipmann's lab who knew biochemistry and whom I was friendly with, because Rockefeller really encouraged you to get to know people outside your own lab in a variety of ways. Jimmy Schwartz was one of them, whom I just mentioned, who was working with Lipmann; and through him I met a postdoc in Lipmann's lab—Dan Nathans, who later won the Nobel Prize [1978] for his work. Dan sort of took me under his wing and taught me just the manipulations—how to do biochemistry—so I could then adapt that to the work that I was interested in, which was working with extracts of virus-infected cells and looking for new enzymes. And I discovered the polio polymerase doing that—with all this technical help from Lipmann's lab, but the actual experiments were things I did myself.

LIPPINCOTT: Where did you live when you were at Rockefeller?

BALTIMORE: In the dorm. They had built a really beautiful dorm; they were single rooms, with shared bathrooms. The guy across the bathroom from me was Harvey Lodish, whom I later worked very closely with at MIT and who is now one of the honored people in cell biology.

LIPPINCOTT: So your PhD was from Rockefeller?

BALTIMORE: My PhD [1964] was from Rockefeller. I worked two years at Rockefeller.

LIPPINCOTT: You finished your work in '63, and you went back to MIT as a postdoc?

BALTIMORE: That's right—as a postdoc with Jim Darnell. And then after a year, he moved to Albert Einstein. So I had to move somewhere, and I wanted to learn biochemistry. I knew biochemistry, but I wanted more professional training in biochemistry, so I went to Jerry [Jerard] Hurwitz's lab at Albert Einstein. It was a little bit coincidental that Jim and I both went to Einstein, although I did continue to collaborate with his lab a little bit there—but I mostly worked in Jerry's lab and got just terrific training in how to do biochemistry. Back when I made this discovery of polio polymerase, the first thing I did was to show that the virus inhibited cellular RNA synthesis and then developed its own RNA synthesis. The experiments on the cellular RNA synthesis were the first biochemical experiments I did; I would publish them in *PNAS*. So we went to write them up, and Richard [Franklin] had no experience writing this stuff up, and I had no experience, of course, so Richard said, "Why don't we go down and talk to Jerry Hurwitz? He's the great biochemist"—he was then working at NYU—"and he will be able to help us formulate this work in a professional way." So we went to Jerry, and Jerry said, "Oh, this is awful!" When we plumbed the depths of what was awful, it was not the work that was awful, it was the way we had presented it. It was totally unprofessional.

LIPPINCOTT: You mean it wouldn't have done as a paper?

BALTIMORE: It would not have done as a paper, right. It was not in the right units. There was nothing wrong with it; it just wouldn't tell professionals the things they wanted to know. Because we didn't *know* what the professionals would want to know. But Jerry knew, because he had been trained by [Arthur] Kornberg. So Jerry set us straight, and then we published the paper.³

³ Baltimore, D., & R. M. Franklin, "The effect of Mengovirus infection on the activity of the DNA-dependent RNA polymerase of L-cells," *Proc. Nat. Acad. Sci.* 48: 1383-90 (1962).

LIPPINCOTT: That wouldn't have been your first paper, would it?

BALTIMORE: I don't think it was my first paper; but you know, it just might be.

LIPPINCOTT: And it was thanks to his tutoring you in all the fine details of how to present data and so forth?

BALTIMORE: Right. So now I went to Jerry as a postdoc, and I did some interesting experiments there. I did one very important experiment there.

LIPPINCOTT: On what?

BALTIMORE: On how RNA polymerase initiates chains of RNA. I demonstrated that it initiated with a triphosphate, which wasn't known.⁴ But I'm not the first author on the paper, because I had basically scooped everybody in the lab, and they all wanted to take credit for it. They were furious with me, because I had got a little bit of stuff from one guy and a little bit of stuff from another guy and put them both in the same test tube and there was the answer. They should have done that experiment well before I did it.

LIPPINCOTT: But it was something that it occurred to you to do.

BALTIMORE: Yes. So anyway, I'm an author on the paper, but I'm not the first one, and the world never knew that I discovered it, but I did. And then I worked on trying to do the same thing to DNA polymerase, and I couldn't get it to work. And you know why? Because it doesn't happen. [Laughter] It doesn't initiate with a triphosphate; it initiates in a different way; and I finally showed that it initiated in a different way.

LIPPINCOTT: So then we come to the Salk Institute—and I just wanted to ask you if you're getting tired, because—

⁴ Maitra, U. et al., "Identification of nucleoside triphosphate ends on RNA formed in RNA polymerase reaction," *Biochem & Biophys. Res. Comm.*, 18:5-6, 801+ (1965).

BALTIMORE: No, I'm not tired. I could talk about myself forever. [Laughter]

LIPPINCOTT: Oh, that's wonderful!

BALTIMORE: I look at these kinds of things—which I've done in various venues, actually—as a substitute for writing an autobiography. And this thing [points to recorder] seems to have an infinite capacity, so—

LIPPINCOTT: Yes, it does. It's not like the old tape machine, where you had to watch the tape. Renato Dulbecco—did he bring you to Salk, and how did you know him?

BALTIMORE: Well, I think he showed up at Cold Spring Harbor. He was a phage biologist originally; he knew what I had been doing. When Richard Franklin and I published a whole series of papers that really set the basis for biochemical analysis of RNA viruses, the world had never heard of Baltimore and the world had never heard of Franklin. And we would get inquiries: "Which one of you is the senior member of this team?" Because Richard was a young assistant professor and had never done this kind of work before. He didn't do that kind of work; I did it. But he was my coauthor on all of this, and that's fine, and I give him all the credit in the world for helping me be the success that I became. But people knew the papers of Baltimore and Franklin—everybody did, in the animal virus field. So Renato certainly knew the work that I'd done. So Renato shows up in New York to give a series of lectures, maybe at Rockefeller. I'm at Albert Einstein, and he calls ahead and says, "I want to talk to you." He was at Caltech at the time, and he told me about the Salk Institute development—that this was going to be built, that he was going there, and would I want to come and take a piece of his laboratory and start my own independent work in his laboratory. He would arrange for me to get funding and a position. It sounded like heaven. By this time, I was ready to think about going to California.

LIPPINCOTT: This was an entirely new enterprise, wasn't it?

BALTIMORE: It was an entirely new institution; it hadn't been built yet. It was just being formulated, and it was really Szilard who was the man behind it.

LIPPINCOTT: Leo Szilard? The physicist?

BALTIMORE: Leo Szilard, yes. Leo had become an adviser to [Jonas] Salk. Leo did a lot of biology; he knew biology very well. He knew talent, and he knew organizations, so Leo advised Jonas, and Jonas looked at the people who Leo thought were the right people to get. So he got Dulbecco, he got Ed [Edwin] Lennox, he got Mel [Melvin] Cohn—all these very quantitative people in a very good biological sense. Extraordinary people to start the institute with.

LIPPINCOTT: And they were all to look at—what?

BALTIMORE: Two of them were immunologists. Dulbecco was the animal virologist and cancer biologist. There were one or two people, I'm forgetting, who were part of that initial group. Anyway, so Renato said, "Come," but he also said, "Don't make a decision now, because I'm not yet a hundred percent sure that this is all going to happen."

LIPPINCOTT: Nothing had been built yet; this was just an idea. This was to be in La Jolla?

BALTIMORE: La Jolla, yes. They built temporary labs, but the permanent labs hadn't been built. And they were trying to attract various people, and they were getting these people I mentioned. Jonas was a little difficult. He had his own ideas. Jonas was a sort of dreamy person who thought of himself as a philosopher and wrote many books about philosophy.

LIPPINCOTT: But he found a polio vaccine.

BALTIMORE: He designed the polio vaccine, right. But he was an MD—that's the only research he ever did, as far as I know.

LIPPINCOTT: Yes, he wasn't what you'd call a microbiologist.

BALTIMORE: Oh, no, no. He was a doc, working with others.

LIPPINCOTT: Did you meet him when—?

BALTIMORE: Oh, yes, I met him many times. When I went to Salk, he was there all the time.

LIPPINCOTT: Did he involve himself in the work of the labs?

BALTIMORE: He had a lab. It did very pedestrian things. Yes, he tried to continue being a scientist, but he never did anything else important. And then the finances were complicated, in what the National Foundation [for Infantile Paralysis] wanted to do. This was all money from the March of Dimes that built the Salk Institute—it was a certain gift to Jonas from the March of Dimes for having discovered the vaccine.

So Renato wasn't sure it was going to happen, and we went back and forth through that whole spring with him saying, "It's going to happen! Come!" and then "Oh, I'm not sure!"

LIPPINCOTT: This would be the spring of 1965?

BALTIMORE: Yes, spring of '65. Actually, this must have started in the fall of '64 and carried through to '65, because I finally left and went to Salk in April 1965, so it must have evolved over the winter of '64-'65. Finally Renato said, "It *is* going to happen. I'm moving. Come join me." And I said, "Fine," and I did. We packed up our car and drove out.

LIPPINCOTT: You were married at the time.

BALTIMORE: I was married at the time.

LIPPINCOTT: And you moved to La Jolla?

BALTIMORE: First to Solana Beach and then to La Jolla. Drove across the country in a snowstorm in Colorado, had to buy chains to get over the mountains.

LIPPINCOTT: Oh, great—well, that's the way to come to California, because it's an important metaphysical journey that you made.

BALTIMORE: It is, absolutely—and it was. I had been to California once or maybe twice before to give seminars, but it was a new world for me.

LIPPINCOTT: Did you like living in La Jolla?

BALTIMORE: I loved it! We lived on La Jolla Shores Drive, on the way down to the beach.

LIPPINCOTT: Yes, I know where that is. OK, so there you worked with Marc Girard, and that's where Alice—?

BALTIMORE: Marc came out and joined me. Marc had come as a postdoc to Jim Darnell's lab [at MIT] and moved to Einstein with Jim, and he and I had worked very closely in Jim's lab. We were good friends. I remember I greeted him when he got off the plane from France having never been in the United States. He's French; he was recommended to Jim by Andre Lwoff. His only degree was a veterinary degree, but he'd gotten interested in viruses, worked a little with Lwoff. Lwoff said, "Go to the United States, learn what you need to learn, and come back to the Pasteur Institute." Which is what he did.

LIPPINCOTT: A lot of veterinarians did animal virology, didn't they?

BALTIMORE: Only a few. Harry Rubin was one. Marc was one. Very few veterinarians do science. It's a great background for science, but the schools are not oriented that way, and the people who go into the field are not oriented that way.

Anyway, Marc came out in 1963; and a couple of days after he arrived at MIT, I took him down to the machines to get lunch—there were sandwiches and sodas and whatever in vending machines. And Marc said, "How sophisticated! You have beer!" It was root beer. [Laughter] But he had no idea what root beer was. So I helped Marc get oriented towards American life. Marc is a terrific guy. And having spent two years here, he said he'd love to spend his third postdoc year in California, so he came and worked with me as a postdoc.

LIPPINCOTT: It was a year's appointment?

BALTIMORE: It was year's appointment, and then he went back to Pasteur. He was my first trainee.

LIPPINCOTT: How about Alice Huang?

BALTIMORE: Alice came a year later; she was my third trainee, I guess—second postdoc.

LIPPINCOTT: Where did she come from? Where did she get her PhD?

BALTIMORE: She got her PhD at Johns Hopkins with a man named Robert Wagner, a virologist. And she introduced, into his lab, work on a virus called vesicular stomatitis virus, VSV, and she later introduced me to that. She was recommended to me by Dan Nathans. She had contacted me and asked if I would be interested. I didn't know anything about her, I didn't know anything about Wagner; and all I knew at Hopkins was Dan Nathans, because I had known him from Rockefeller. So I called Dan, and I said, "Do you know anything about this woman who's working down the hall from you?" He said—he either said, or he did a little research and then said—"She looks very good, and she's beautiful. Take her."

LIPPINCOTT: She went to Wellesley.

BALTIMORE: She went to Wellesley, that's right. Two years. She left Wellesley after two years to go to Hopkins for an MD program that they had started for kids who had finished their sophomore year of college. In those days, they were trying to move up the MD—that's since pretty well died. She really rues the fact that she didn't spend four years at Wellesley.

LIPPINCOTT: Well, I don't think the science—with the exception of astronomy, which was pretty good—was very good at Wellesley when I was there. I wasn't myself—

BALTIMORE: You know, for an undergraduate that doesn't matter. The reason she's unhappy about not having spent four years at Wellesley is because she missed the chance— They have a good music course, a good art course—

LIPPINCOTT: The music course was terrific!

BALTIMORE: Yes. So she really feels she slighted her education by moving it so fast.

LIPPINCOTT: Well, I can see that. It's definitely liberal arts.

BALTIMORE: She wanted to go to medical school; she knew what she wanted to do. And then she finally gave up medicine and went into research full time.

LIPPINCOTT: Yes. So, at La Jolla, there were anti-war rallies that you—?

BALTIMORE: Oh, yes. I was there from 1965 to 1968. Vietnam was exploding. There were some very radical groups around San Diego. There were old Trotskyite groups that were still—

LIPPINCOTT: San Diego was very right wing, was it not?

BALTIMORE: San Diego was very right wing, and it had this underground of old lefties. Actually, so did Caltech. Pasadena was very Republican and very right wing, but we had this kind of cell here, as it turned out.

LIPPINCOTT: [J. R.] Oppenheimer?

BALTIMORE: Well, it wasn't Oppenheimer; it was the people at JPL. Frank Molina. That's what tainted Tsien [Hsue-shen]. That's a great story. There was a great book about it.⁵

LIPPINCOTT: Did Alice go to these anti-war rallies with you?

BALTIMORE: She did, but she was less— I mean, she was certainly committed to it, you know. Everybody was committed to the idea that the Vietnam War was idiocy and that we had to do something to stop it. Our commitment was not because I was in any danger of being drafted; I was too old.

⁵ Iris Chang, *Thread of the Silkworm* (New York: Basic Books, 1995).

LIPPINCOTT: No, it was just your feelings about it.

BALTIMORE: That's right.

LIPPINCOTT: There's a story I read in a couple of places about an art exhibition they had at Salk, and they censored some of the artwork, and that's what tipped it for you. You got angry, did you?

BALTIMORE: I did. I was married at that point to an artist, to Sandra. And she got to know this guy Hugh Duckworth, who was a sort of surfer character and an artist in La Jolla, and we arranged to put up a show of his work and hers at Salk. But he took joy in desecrating the flag. It was not that he was so political, it was almost more....

LIPPINCOTT: Bohemian?

BALTIMORE: Bohemian, right. And so it offended people and got in the newspapers.

LIPPINCOTT: Whom did it offend at Salk?

BALTIMORE: Well, the president, for one.

LIPPINCOTT: Who was he?

BALTIMORE: Augustus Kinzel—I think that's who it was.

LIPPINCOTT: What about Jonas Salk himself. Did he care about this, or—?

BALTIMORE: I don't remember Jonas getting involved in it.

LIPPINCOTT: It was just the administration.

BALTIMORE: Yes. So they insisted on taking down some of the art; and I said, "If this place is so sensitive to the right wing, it's not the place for me." Anyway, I was a little tired of— You

know, new institutions, whatever they are, are fragile places that are constantly coming apart around the corners, because they just don't have in place the history and the people and the community, the style, the memory—all of that. And that's exciting, in a way, because it means that anything goes, and there's a tremendous opportunity to create the style of the institution. We did that, and that was fun. But at the same time, it's wearing, because you can't ignore the pressures of it. At least I couldn't; I kept getting myself involved in more than I should have. And so for me to go back to MIT— First of all, it got me out of this situation; my marriage was coming apart at that point, and then I took up with Alice afterwards.

LIPPINCOTT: Yes. January '68 is when you moved back to MIT.

BALTIMORE: Right. But I spent the last three months in France, with Marc at Pasteur and with Boris Ephrussi in Gif-sur-Yvette, and then I came back to Salk, packed up, and left, and Alice and I drove across the country.

LIPPINCOTT: Was Dulbecco sorry to see you go? He became director there, didn't he?

BALTIMORE: Yes, he did. We're going to have lunch with him in a few weeks, during the Biology [Division] retreat, which is in San Diego. He's in his mid-nineties. Renato is failing now, I think, so I really want to see him.

LIPPINCOTT: Is he in a retirement community there?

BALTIMORE: No. He married his technician at Caltech, left his old Italian wife. I mean, she wasn't old; she was the woman he had married and who bore his first brood of children. I don't know what happened to her; she lived around here for a long time. He married Maureen, who was thirty years younger, twenty-five years younger, or something. They're living in La Jolla; they've been living in the same house for a very long time. They have a wonderful marriage, and she's just a fabulous woman. They have one child, who's now out of college.

LIPPINCOTT: Well, this may be a good place to stop. [Recording ends]

DAVID BALTIMORE

SESSION 2

November 19, 2009

LIPPINCOTT: We left off just as you came back to MIT from Salk in January 1968, and you came back as an associate professor. Then you and Alice Huang married in October?

BALTIMORE: Yes.

LIPPINCOTT: Just to get the personal business down—you have one daughter, Teak.

BALTIMORE: Actually, her name is Lauren Rachel Baltimore. But in utero we started calling her “the kid,” and that became “TK,” and that became “Teak.”

LIPPINCOTT: Oh, “TK,” to come. That’s the editorial term.

BALTIMORE: I know. I see it all the time, actually. When she was growing up, we continued to call her Teak, and she loved the name. So when she was of an age to make decisions, she said she wanted to be Teak and spell it TK—Just capital T, capital K—which is what she’s known as.

LIPPINCOTT: Oh really? Because I’ve seen it in print as—

BALTIMORE: Like the wood. Yes, when we spell it out so people will know how to say it, it gets spelled like the wood, but she signs herself “TK.”

LIPPINCOTT: Is she a scientist?

BALTIMORE: No. She works for Advance Publications.

LIPPINCOTT: Oh, that’s Si Newhouse, who owns *The New Yorker*.

BALTIMORE: Right.

LIPPINCOTT: Is she in New York?

BALTIMORE: She's in New York but she reverse commutes to Jersey City, because she works in the Web presence—in the newspapers, actually, of Advance Publications.

LIPPINCOTT: That's Donald Newhouse, I think.

BALTIMORE: Yes, I think that's right. And that is headquartered in Journal Square, in New Jersey. So she takes the PATH out. She lives in the Village.

LIPPINCOTT: Oh, I used to live in the Village. Where does she live?

BALTIMORE: Twelfth Street between University and Broadway.

LIPPINCOTT: Oh, my. Yes, I have some happy memories of the neighborhood.

BALTIMORE: We actually own the apartment. When we moved here, we decided that since we were going to be living in the President's House, we really should keep an investment in real estate, so we bought an apartment in New York, a very nice—

LIPPINCOTT: Twelfth and University? You were near what used to be Bradley's.

BALTIMORE: Bradley's. Now, that's before my time, I think.

LIPPINCOTT: It was a piano jazz place—sort of the downtown Elaine's, on University between Tenth and Eleventh.

BALTIMORE: Oh, yes, there used to be a lot of music on University. There really isn't anymore.

LIPPINCOTT: The Cookery.

BALTIMORE: The Cookery—that's all gone. And that great bar—one of the great bars of New York.....

LIPPINCOTT: The Cedar Tavern?

BALTIMORE: The Cedar Tavern closed, and they're not going to reopen it. So there's nothing left of that era, but it's still a great place to live. I was just there; we came back last night.

LIPPINCOTT: So she's happy there.

BALTIMORE: She loves it. She doesn't want to think about ever leaving New York City. She went to Yale, and then she and her classmates all moved to New York; and many of them are still there. Some of them have families now, and they're living in the suburbs, or they've moved away.

LIPPINCOTT: I know. Well, that's good, because a lot of people grow up without ever living in New York and wish they had, and their lives are ruined as a consequence. [Laughter] Well, OK. So you're back at MIT, and then in 1970 you published your discovery of reverse transcriptase.

BALTIMORE: Yes.

LIPPINCOTT: Along with Howard Temin. He had made the same finding independently, is that right?

BALTIMORE: Right.

LIPPINCOTT: And your papers were published in the same issue of *Nature*.⁶

BALTIMORE: Yes.

⁶ Baltimore, D., "Viral RNA-dependent DNA Polymerase: RNA-dependent DNA Polymerase in Virions of RNA Tumour Viruses," *Nature* 226: 1209-11 (1970).
Temin, H. M., and S. Mizutani, "RNA-dependent DNA Polymerase in Virions of Rous Sarcoma Virus," *Nature* 226: 1211-13 (1970).

LIPPINCOTT: Did you have a lot of back-and-forth with him while you were working on this? Or, were you aware of what he was doing—let me put it that way?

BALTIMORE: No. It deserves a little more discussion. Howard Temin was a Caltech graduate student and worked here in Renato Dulbecco's laboratory with a man named Harry Rubin, who was Renato's postdoc. But Renato gave him a lot of freedom—as he gave me when I was at Salk—to develop his own program. Harry had been interested in Rous sarcoma virus. So Howard joined Harry, and they developed the first quantitative in-vitro assay for cancerous transformation, which was recognized in the field as one of the great discoveries of cancer research. It put cancer research on a quantitative basis. Harry was a veterinarian doing a postdoc and Howard was a student. But, of course, Howard was in one of the most exciting scientific atmospheres that existed in America at that time—at Caltech, with the school of Delbrück being very evident and involving some wonderful young scientists.

LIPPINCOTT: This was in the middle sixties?

BALTIMORE: This was in the late fifties. This was when the Meselson-Stahl experiment was done here. Howard came here in '55, and Meselson was one of his roommates. Howard was a unique person, who had his own desires in science. Understanding cancer-inducing viruses became his passion—and there wasn't anybody else. The way that he began to think about the problem—at the time he was a student here—became the dominant issue in his life. Because he recognized—and I think the experiment was key—he recognized that something very odd was going on when a cancer-inducing virus infected a cell. What was odd about it was that the phenotype of a cancer cell—which is to grow indefinitely, and it also had some physical characteristics that were pretty obvious—was a permanent change in the cell. So you were infected with this virus, and the cell became a different cell and stayed that way forever and caused cancer if you put it in an animal. And he said, "But wait! Viruses, particularly RNA viruses"—he knew it was an RNA virus—"produce transient effects on cells. RNA is a transient molecule, so it must imprint itself on DNA." He couldn't imagine how, but he was quite sure that DNA was involved, and the simplest way to imagine it would be that the RNA molecule of the virus was copied into a DNA molecule—because RNA and DNA are fundamentally the same kind of molecule, although in the cell they're differentiated in their functions. This notion

appears in Howard's PhD thesis, in 1959. Howard then went off to the University of Wisconsin as an assistant professor, rose to be a full professor, stayed there his whole career. Over the ten years between 1960 and 1970, he tried to find a killer experiment that would prove he was right. And he couldn't find it—a definitive experiment. He worked with drugs, he worked with radiation; he worked with radioactivity. He did everything we knew how to do at the time—hybridization. And none of it was convincing to nonbelievers in the scientific community, and there were almost no believers, except for Howard. Nobody thought that RNA would be copied into DNA. That notion was— It wasn't exactly anathema, but Crick had said, famously, that the central dogma of biology was that DNA made RNA made protein, and it still *is* the central dogma of biology. The fact that RNA can be copied into DNA, which we now know so very well, doesn't change the fact that the flow of information in a cell, be it a person or an animal or a bacterium, is from DNA to RNA to protein. So that was ingrained in people's minds. This was still early days in molecular biological thinking, so dogmas had an important role; they guided people's thinking. That's what people thought about.

Howard became increasingly convinced that RNA made DNA, but he couldn't find a way to prove it. He published a number of papers on it; he talked about it at meetings. He also did other work, but in fact he was viewed as a bit of a heretic in this thing because he was so persistently trying to find a way to prove it.

So in 1969 I began to see a way to test it. I was aware of what he was doing. I was in virology; I taught virology. I didn't do anything with cancer-inducing viruses, but I was interested in the whole issue of cancer and viruses, actually from way back. Through all of this period, although I had known Howard at the Jackson Lab—I think we talked about that—I didn't see Howard much. He was stuck in Wisconsin; he wasn't invited many places, because the big thing he was doing was something that most people just didn't believe in, and they didn't want to hear any more about it; and he was pretty monomaniacal in his approach to it. And my own career was going very well; I was working on poliovirus all that time and became moderately well known for it. So, no, Howard and I didn't have much communication through this period. I remember once seeing him—and only once in that time—and seeing his performance, if you wish, about this issue. It was probably around 1967, in Issaquah, Washington.

I did see, in early 1970, that there was a way to test this notion. I had to get some virus, and I got the virus—that's a long story, which I've told in another place—and then I did the experiments. I'm proud to say I did them with my own hands.

LIPPINCOTT: And you found this enzyme.

BALTIMORE: And I found the enzyme, and the enzyme clearly copied RNA to DNA, and it provided the piece of evidence that I knew would convince the scientific community that Howard had been right all along. So when I had the experiments to the point where I was absolutely convinced they were right, the first thing I did was to call Howard and tell him about it. And he told me that he was doing the same experiments and had actually talked about them at a meeting. So I said, "Well, look, we're ready to submit this. Are you?" And he said, "Well, we're almost there. We'll get there right away." And so we decided to submit them back-to-back to *Nature*. I had absolutely no qualms about that because I knew that Howard had spent a decade toiling to make this clear, and that I never would have done the experiments had Howard not developed the notions. Although that's a little hard to know, because now we were ten years later, and it was beginning to become clear to many people that this was not totally crazy. If you look around that time, there's some stuff in the literature that was much more positive about Howard's idea than had been true all through that decade. And after I did the experiments and ran into some people in the scientific community, they told me that they were actually working on the problem at the time. Like [J. Michael] Bishop and a guy name John Bader at NIH. It was just the way I approached the problem and my own history in doing that kind of experiment—the kind of experiment that was involved was something I'd been doing for ten years.

LIPPINCOTT: The enzyme was named by some journalist, wasn't it? Reverse transcriptase?

BALTIMORE: That's right. We called it the RNA-dependent DNA polymerase, being very scientific about it. A mouthful! And the journalists at *Nature* renamed it "reverse transcriptase" in their news column. I think it was John Maddox who did that, but I'm not sure.

LIPPINCOTT: Well, it's a good name because that's what it does.

BALTIMORE: Yes, it's a good name.

LIPPINCOTT: So then you got to be a full professor at MIT, in 1972.

BALTIMORE: Once I did that experiment, and people looked at the response to it, it suddenly became— I mean, papers began flowing from all sides that built on what we had done.

LIPPINCOTT: It was pretty fast to your Nobel. Only five years.

BALTIMORE: Yes, well, that's why. And it was a lot faster to the realization that I would win a Nobel—but nobody expected it to come that fast. Least of all me. [Laughter]

LIPPINCOTT: People began to talk about that, I guess.

BALTIMORE: Yes, people began to talk about it, so I was rapidly promoted to full professor and made a member of the National Academy [of Sciences] and all of that, so that people weren't embarrassed by the Nobel Prize coming before they had done this.

LIPPINCOTT: I want to talk about that prize. But before that, in 1975, I'd like you to talk about the Asilomar conference on recombinant DNA that you had a hand in organizing. Did that come up because of all the brouhaha in Cambridge, Massachusetts?

BALTIMORE: It's linked to that, but it didn't come up because of it. The Asilomar conference came before that. In 1973, at a Gordon conference in New Hampshire, [Herbert] Boyer and [Stanley] Cohen—I think it was Boyer—described their experiments linking two pieces of DNA and showing that they would be replicated as a hybrid, as a chimera. And it was clear to everybody at that meeting that a new era was opening in biology. But they also recognized that there were some potentially dangerous things you could do with this new technology.

LIPPINCOTT: Dangerous, in that you didn't know what the consequences would be?

BALTIMORE: Or maybe you could guess the consequences, and *they* were. I mean, for instance, putting an antibiotic-resistance gene into a pathogenic bacterium so that it would resist antibiotic treatment. That's an obviously disastrous thing to do, and that's the kind of thing that became doable. And then there were all of the fantasy things that might happen, because once you sort of let loose— We didn't have any real experience with what these chimeric molecules could do, so anybody's imagination was as good as everybody else's imagination. And so they all began imagining strange and disastrous scenarios—autoimmune diseases—and you just name it, it was suggested. But I get ahead of the story. So, in '73, the Gordon conference participants, many of them, wrote a letter to the National Academy of Sciences saying, "This is a wonderful new technology. It's going to revolutionize biology. But we see a potential for danger here, and we think that there ought to be serious consideration of how to move forward with this technology. It shouldn't just happen."

LIPPINCOTT: They wanted rules?

BALTIMORE: Well, if I remember correctly, it was pretty vague. Maxine Singer, one of the organizers of the Asilomar conference, was an old friend—she's actually a Swarthmore graduate, although older than I am. She and Dieter Söll were the two co-chairs of that Gordon conference. They signed the letter to the National Academy. She contacted Paul Berg, who was her good friend—I don't think he was at that meeting—and said, "We've got to do something to respond to this." Now, Paul and I and others had been involved in an earlier Asilomar meeting, because there had been a similar kind of issue, around the safety of virus work; so we had held a meeting at Asilomar to talk about the safety of, particularly, cancer viruses and work on SV40. But that meeting was totally focused on lab-safety issues—whether you could get cancer in the lab and things of that sort. So there was a precedent. So they got in touch with me, and I said yes, I thought this was a real issue—I didn't know anything about it. So I invited them and some other people, including Jim Watson, to come to a meeting at the cancer center at MIT—

LIPPINCOTT: And this was '74?

BALTIMORE: This was in April '74. So they all came, and we agreed that we would publish a public letter saying, first of all, there should be a moratorium on certain kinds of experiments.

The experiment I described to you—putting antibiotic-resistance genes into a bacterium—and making hybrid viruses of various sorts, putting virus genes willy-nilly into different places. We had a little bit of a hierarchy of what might be really dangerous.

LIPPINCOTT: And you wanted to stop all such things.

BALTIMORE: We just asked for a voluntary moratorium in the scientific community prefatory to having a meeting—which turned out to be the Asilomar meeting—in which the community could consider these experiments and come up with a safe way forward. So that was the origin of Asilomar. We did it at Asilomar because Paul said, “That’s a great place to meet.”

LIPPINCOTT: Well, what about the Cambridge City Council?

BALTIMORE: That’s later. See, this issue was not publicly terribly interesting *until* we held the Asilomar conference. We agreed there should be press there, but we insisted that the press agree that they would write nothing until after the meeting was over, so that there wouldn’t be reporting on day-to-day events, and they agreed to that and held to that. So right after the meeting, there was a spate of publicity about the fact that the meeting had taken place, and it was good in the sense that because we insisted that they wait until the end, they could start with the end result, which was a call for a continued moratorium and an NIH committee to grade experiments and a way forward on this graded experimental basis.

LIPPINCOTT: Were these scare stories, in any sense?

BALTIMORE: They were a little bit scare stories. Sure. It’s sort of inevitable that they would be. But they were pretty responsible. David Perlman at the *San Francisco Chronicle*. He’s still writing stories—wonderful guy. And I can’t remember whether the *New York Times* had someone there or not—maybe Harold Schmeck. I’ve forgotten who was there from the press.

LIPPINCOTT: But you think they were by-and-large responsible stories?

BALTIMORE: Absolutely. But there *was* a scare aspect to them. Well, we were scared! So that scared the Cambridge City Council, and they found out that Harvard was going to start these experiments and build a laboratory—a safe laboratory. This must be '76 or '77 [July 1976—ed.]. I know I was called back from another Gordon conference in New Hampshire to come to the City Council meeting in July or August. So the politicians in Cambridge decided that they would make this an issue, and they drove Tom Maniatis, who was the scientist who wanted to start this work, out of Harvard. He went to Cold Spring Harbor and then to Caltech. He was on the Caltech faculty [1977-1981] until everything calmed down in Cambridge, and then he went back to Harvard. I was with him yesterday; he just moved to Columbia.

LIPPINCOTT: Well, eventually they stepped down from this confrontational position, is that right?

BALTIMORE: No. What they did was to appoint a citizens' committee. And let me tell you, this was just citizens—I mean, the guy who delivered oil, and a nurse, and whatever—to advise them on whether the NIH guidelines were a safe umbrella with which to go forward. And these people did a magnificent job. They listened to everybody. They recognized who the kooks were. They understood, as best they could but not badly, the nature of the issues, and they finally said, “The NIH guidelines are the way to go.” With some modifications to give a little more local control. But effectively they defused the problem; and at that point the politicians had no place else to go.

LIPPINCOTT: Well, that's a very happy story!

BALTIMORE: It *is* a happy story. It's a very important story, in that way.

LIPPINCOTT: Well, in the meantime, you had gone to Stockholm. I'd like you to recall some of that. You and Howard Temin and Renato Dulbecco shared the prize for physiology or medicine in 1975. What was it like in Stockholm? Did your whole family come with you? Not the baby, I guess.

BALTIMORE: Well, my mother and father came. My father had a heart attack and was in the hospital when the Nobel Prize was announced. It's a funny story, because he was in a private

room and they didn't turn on the phone in a private room until after, say, eight o'clock in the morning. He got up early and he turned on the television set, and he learned about it from the television, sometime between seven and eight—we got to him not long after. And they put him back in Intensive Care [laughter] because they were worried about him. He was not well. He never fully regained his energy, and at the time of the Nobel celebrations it was never clear to us how much he remembered of it, but he was there. My mother was fine, she enjoyed it. My brother came with his family. He had one child at that time, and she came; she was probably four. But Teak was only one year old. We couldn't take her—there was no purpose in taking her.

LIPPINCOTT: No. [Laughter] She wouldn't have remembered it. Did you meet many of the other laureates there?

BALTIMORE: Well, this was 1975, and the Nobel prizes started to be given in 1900, so this was the seventy-fifth anniversary. And what they have been doing is that at auspicious times they invite back all previous Nobel winners. So, yes, I met my fellow laureates as well as a very large number of the then-living Nobel laureates.

LIPPINCOTT: Do you remember anybody who impressed you particularly?

BALTIMORE: Well, there was a moment that I'll never forget. As part of the ceremony, you give a lecture to your professional colleagues. So physiology or medicine laureates give lectures to the Karolinska Institute faculty, which is the great research institute of Stockholm. So I prepared a lecture—it's something you prepare very carefully—and sat in the front row and was called to the dais, and turned around, and realized that all of my heroes in biology were lined up in the row behind mine.

LIPPINCOTT: How marvelous! Like whom, for instance?

BALTIMORE: Jacob, Monod, Watson, Crick—those are the main ones.

LIPPINCOTT: It must have been hard to talk in front of them, because of your emotions, I suppose.

BALTIMORE: It was hard. It was *just* remarkable.

LIPPINCOTT: But you managed to open your mouth and make a sound after a while.

BALTIMORE: Well, luckily, I'd written it out, so at least my opening paragraph— The opening paragraph of that talk, if you've never read it, has been quoted widely over the years.

LIPPINCOTT: I think I looked at it on the Nobel website. That's a terrific website. They have some film clips of people from the thirties—like P. A. M. Dirac.

BALTIMORE: Well, Dirac was there. I remember meeting Dirac; he lived a very long time. He only died ten or fifteen years ago. There's just a new biography of Dirac out—supposed to be very good.

LIPPINCOTT: Yes, *The Strangest Man*.

BALTIMORE: He *was* a strange man. [Laughter]

LIPPINCOTT: You know what's funny about that? This is totally incidental, but I also interviewed Val Telegdi, a year or so ago, and brought up Dirac. And Telegdi looked at me and said, "He was the strangest man I ever met." Well, that's what almost everybody says about him. [Laughter] That's why they called his biography that. There's a film clip of him—I think it was in '33 that he won, with Heisenberg—a short clip of him signing in, and he just looks at the camera like this, and he goes [makes rueful *moue*]. [Laughter] It's beautiful! Because he never would say much.

BALTIMORE: Right.

LIPPINCOTT: OK. Renato Dulbecco was given the prize for some slightly different work—not in reverse transcriptase.

BALTIMORE: His work was on DNA viruses. He showed the integration of DNA viruses into the cell. So it was the same sort of issue, the permanent change in the cell induced by a cancer-inducing virus.

LIPPINCOTT: Everybody who gets the Nobel says their life changes immeasurably. Did yours, because of the award?

BALTIMORE: Well, lots of things changed. First of all, the pressures on your time change; suddenly, everybody wants you to be everywhere, and that hasn't stopped to this day. I turn down, easily once a week, an invitation to an international symposium or something like that.

LIPPINCOTT: You were just getting into immunology at that point at MIT. Was this interfering with your research?

BALTIMORE: It did. I had taken a sabbatical in '75 and gone to New York to spend a year at the Rockefeller University, in Jim Darnell's lab. I heard about the prize when I was in New York.

LIPPINCOTT: I was going to ask you, did they call you up in the middle of the night? How did that work?

BALTIMORE: I didn't know it was going to be announced; I had *no* idea that the prize would be announced; I just didn't pay any attention to it; it just didn't seem relevant to *me*. But Alice, my wife, was in Copenhagen at a leukemia meeting. We had a very young child at that point, so I was home with the child, and she was—we had a nanny, also—so Alice was off at the meeting. She was in a symposium that morning in Copenhagen, and the chair of the session was George Klein, who was at that point the central figure in cancer research in Sweden and a member of the committee that awarded the Nobel; and he knew that it was going to be announced at what I believe was twelve o'clock, or twelve-thirty, their time. He was chairing the session that Alice was in. George was famous—is famous to this day—for being able to summarize a session and

kind of pull out the interesting things. So he starts summarizing, and he goes on and on and on and on. And Alice says she could not figure out why he was going on like that. And he finally said, “Look, I want to make an announcement, but it’s premature. On the other hand, I’ve got nothing more to say, so I’m just going to do it. [Laughter] And at, I guess, eleven-thirty, he announces that the Nobel Prize is going to me and Howard and Renato.

LIPPINCOTT: And Alice was there on the dais with him?

BALTIMORE: Yes. So Alice runs to the nearest phone to call me. Calls me, wakes me up at—it was six-thirty in the morning, I think, in New York—and says, “First of all, nothing’s wrong.” [Laughter] Because in those days you didn’t make international calls lightly; and we never called back and forth unless there was something important, an emergency or whatever. She said, “Nothing’s wrong.” And then she said—you’d have to ask her what she said; I don’t remember at all—but she basically said, “Now, I don’t want this to spoil your life, but this is going to be announced,” and so on, and we talked for a little while. I hung up. It was now five or ten minutes to seven, and the prize was to be announced at seven. The nanny was up with Teak, and I was lying in bed, and it suddenly occurred to me that there was nothing happening. And I wondered, Could it possibly be that that was a dream? [Laughter] So I went out and asked Norah, the nanny, whether she had heard the phone ring, and Norah said yes, she had heard the phone ring, so I knew the whole thing was real. And then about two minutes later, all hell broke loose. Reporters started calling. It took a while for the Swedes to find out I was in New York. Everybody has that problem; they’re somewhere else and the Swedes try to find them. The *New York Times*, having discovered I was in New York, came over to interview me, and the next morning in the *New York Times* there was the first picture that anybody can ever remember of a baby on the front page of the *Times*. [Laughter] Because I picked up Teak and I was holding her—although all you can see is the back of her head. That was the photo they used.

LIPPINCOTT: Oh, that’s nice—for her, too.

BALTIMORE: When she graduated from college, a friend of hers, who had graduated a year earlier and was working at the AP or something in New York, went into the archives and found

all the pictures that had never been published which the photographer had taken, and got some of them printed and gave them to Teak as a graduation present.

LIPPINCOTT: Great present! OK, so maybe we should move on to talk about the Whitehead Institute, which you founded in 1982. What was the impetus behind that? Did that come from higher up at MIT, or was it your idea, or—?

BALTIMORE: Oh, no, no. In 1980, in August, I got a call from Joshua Lederberg, and Josh said—

LIPPINCOTT: Where was he, at the time?

BALTIMORE: He was at Rockefeller University. And Josh said, “I work with this guy Jack [Edwin C.] Whitehead, and Mr. Whitehead has been trying to develop an institute. He’s got \$135 million he’s put aside to do this—

LIPPINCOTT: For biomedical research?

BALTIMORE: Biomedical research. “And there are a couple of people who give him advice, and we think it might be valuable for you to come and talk with him.” So I got on my horse and went down to New York and met with Jack Whitehead and Josh and—I can’t remember who else was in the room. He [Whitehead] asked me a lot of questions like, “What would you do to develop such a thing?”

LIPPINCOTT: What was his interest? He wasn’t a scientist, was he—just a very rich man?

BALTIMORE: No. He was an interesting man. Jack Whitehead left college in the thirties after six months or something, joined with his father, and formed a company to make scientific instruments, and that company was Technicon. It was Whitehead and Weisskopf—his father was Weisskopf. His mother, who had actually raised him because they were divorced, had taken the name “Whitehead”—anglicized it and made it less obviously Jewish. But they got together and formed this little company. When I was a graduate student, I used Technicon equipment.

They made the only fraction collectors for chromatography in the world, and they were handmade machines—I can see it. Then, in the early fifties, I think, he came across technology that was developed by a guy in Cleveland, Leonard Skeggs, to measure the concentration of analytes and enzymes and things in blood in an automated fashion. Skeggs had done this because he was a physiologist and was tired of running samples by hand. He was interested in rabbit physiology. I think it was mostly coronary things; you measured coronary enzymes and whatever. He was tired of measuring them by hand and getting scattered data that were hard to interpret; he wanted something really precise. So he realized that you could put reagents into a system automatically by flowing them down a Teflon tube and separating the units of material by air bubbles, because an air bubble would effectively clear everything in its path in a Teflon tube. And that discovery—the discovery of the bubble—was what Jack licensed from Leonard Skeggs, who remained his friend throughout his life; Leonard was on the original board of the Whitehead Institute.

So Jack turned that into a business, and by the time I met him, which is in 1980, he had built a business selling automated clinical-analysis equipment to the world. The Technicon AutoAnalyzer was *the* instrument used in every clinical lab in the world, and they had gone into computerization of it and really made it a multichannel big deal. These were selling for hundreds of thousands of dollars. So Jack was a very rich man, because he had developed this company, and when he took the company public, in 1970 or something, he suddenly found himself a paper billionaire.

LIPPINCOTT: What do you mean by “paper”?

BALTIMORE: Well, because he owned most of the stock in the company—but it was stock, so if the stock went down, your wealth went down. But he had a lot of money. He hired a tax adviser to advise him about this. The tax adviser said, “You have a huge problem. If you die, the estate taxes will be so enormous that they will have to sell the company.” He said, “Wait a minute! I want to leave the company to my kids.” And the tax man said, “No, you can’t do that.” There was, however, a loophole, and the loophole had been opened by Howard Hughes and is the basis of the Howard Hughes Medical Institute. Which was that if you started an institute of medical research at a hospital, then you could have the institute own the company, control the company,

and the profits from the company have to go to the institute's needs. So Jack said, "Fine. I'll form an institute and I'll appoint, to head the foundation that controls the institute, my friends"—which was what Howard Hughes did—"or my kids, and we'll keep control of the company, and profits can go to that, that's fine." Because there are a lot of ways of getting the money out of it.

LIPPINCOTT: So that was his motivation.

BALTIMORE: That was his motivation. And he did that. He formed the institute at Duke, and that was the original Whitehead Institute. And it had gone badly, because Jack was a very controlling person, and he assumed that he could run everything himself—

LIPPINCOTT: Did he want to select the faculty?

BALTIMORE: He wanted to select the faculty; he wanted to do it all. Duke bought into this thing—because there was a lot of money involved—and didn't put in place academic safeguards. So they were at loggerheads. And finally Duke said, "We can't have it anymore." So it reverted back to Jack. Meanwhile, Revlon had bought Technicon [May 1980], so Jack went from having a problem to having money—no problem left. But the idea had captured him, and he began to formulate it not as a tax dodge but as giving back to the community that had made him so wealthy, because what he had done had come out of the research community from the very beginning. And he was very close to and really loved the research community—loved his connections to Rockefeller and the scientists there. That's how he did these fraction collectors—because they needed them. So by the time I met him he had money, not a tax problem, and he had made a decision: "I'm going to give the institute \$35 million to build and equip a building and \$100 million endowment." So that's where the number came from. He had put together a committee of elders—Josh Lederberg; Gus [Gustav] Nossal, from Australia; and some others—to help him find a venue for this thing. But he now knew a lot more: He knew that medical research, clinical research, was very expensive and generally very far downstream and not terribly efficient in affecting the treatment of people. But basic research was a very different story; it cost a lot less to do any individual thing, and when you made a discovery in basic research, the effects on medicine were profound. So he had been convinced, through his association with these other people as well as his basic intelligence—he was a smart man, very

smart—that he should look to basic research and not to clinical research. He had originally avoided the Cambridge/Harvard area.

LIPPINCOTT: Why?

BALTIMORE: Because he was afraid he'd be overshadowed. One of the reasons he'd gone to Duke was that he saw Duke as a sort of out-of-the-way place—it was more so then than it is now—where he could have a bigger profile. And in fact he had come through Cambridge when he was first trying to site his institute, and I had actually presented to him—because there was supposed to be a Harvard-MIT thing—but he said, “No, I don't want to be up in Cambridge.” So he changed his mind; he was now willing to talk about Cambridge. He had refocused his institute away from medicine to basic research, and he'd also learned that in the academic world a donor cannot control what goes on, much as he wanted to. So that was the whole background to my meeting with him. I didn't know they were thinking of me to direct this.

LIPPINCOTT: But you must have been a very attractive prospect to him.

BALTIMORE: I was attractive to him in a couple of ways. But one important thing—which he told me later—was that he had asked a lot of people this question: “How do you form such an institute; what do you do?” And everybody had told him about their own work and how they would use their own work as a basis for the project. And I, contrary to that, had thought about the problem and decided what I considered to be the direction of contemporary research that would be the best basis for building an institution—which was developmental biology, which I didn't do. Well, I did it, but I was not a core developmental biologist.

LIPPINCOTT: Do you mean embryology?

BALTIMORE: Well, embryology, the development of the embryo, the signaling that goes on. This was a big problem in biology—how do the egg and the sperm generate an organism? Everything in biology was relevant to it. Intracellular signaling in cancer was a developmental problem—whatever you wanted. It was a good rubric for an institution, because it wasn't particularly restricted, and yet it was recognizable as a direction of science that was strong and

contemporary. So that's what I presented to him. He said I was the only one who ever came in with an idea that wasn't about their own research and he liked that. I guess I was moderately persuasive, so a couple of days later he called me up and said, "Would you consider directing such an institute?" And I then—

LIPPINCOTT: With the idea that it would be at MIT?

BALTIMORE: No, the site of it was entirely open at that moment, and he had had some discussions with other institutions: Stanford, Rockefeller, Harvard—certainly not MIT. He hardly knew MIT did biology.

So I, after thinking about it for a while, said to him that, no, I would not agree to be director. What I would agree to was to be his consultant in the formation of the institute, and if I could build a structure that was attractive to me, then I would be director. And he agreed to that. So then I started the process of visiting these other institutions, talking to people at MIT—Jack loved to kid me later, saying the institute could have been anywhere, as long as it was MIT.

LIPPINCOTT: Whom did you talk to at MIT about this—the head of the Biology Department?

BALTIMORE: I talked to the head of biology, Maury Fox. I talked to the president, Paul Gray. I talked to the board chairman, who was Howard Johnson, who had been president. And I talked to Josh at Rockefeller. And I talked with the dean at Harvard Medical School, who said "Look, no problem with this; we can make it work." This is now a very long discussion. Jack had very particular requirements, and the basic form of these requirements was that the institute be independent, have its own board, and be able to—as Jack liked to say—make its own mistakes.

LIPPINCOTT: And hire its own faculty?

BALTIMORE: Hire its own faculty.

LIPPINCOTT: Without interference from the parent?

BALTIMORE: Well, so, now we have to negotiate. And it frightened people at most schools. It frightened Stanford. It frightened Rockefeller. Harvard Medical School said, “Don’t worry. We do this all the time.” All their hospitals are independent; Dana Farber’s independent. So I felt in the back of my mind that the fall-back position was going to be Harvard Medical School. But I loved MIT—still love it—and I really, if possible, wanted it sited at MIT, because I felt that the quality controls that we could put in place at MIT would make it a success. But that would require that all appointments be jointly Whitehead-MIT appointments. So we had to negotiate a structure in which that could be true, but Jack was adamant about independence. So we had to interpret “independence” in a way that kept Jack happy and MIT happy.

LIPPINCOTT: That must have been difficult.

BALTIMORE: It was. And then the press got wind of it, and they just— It was a terrible time; that’s a whole story in itself. But I finally found a middle way and negotiated with both sides—actually, with many sides—and put in place a structure.

LIPPINCOTT: And you agreed to be director.

BALTIMORE: And then I agreed to be director. This was 1982.

LIPPINCOTT: And up until 1990, when you became president of Rockefeller University, you were the director. In the interim, I wanted to talk about the NAS [National Academy of Sciences] committee on a national strategy for AIDS, which you co-chaired. This was in ’86. How did you get involved in the AIDS epidemic?

BALTIMORE: I got involved in the AIDS epidemic because I had discovered reverse transcriptase, and the AIDS virus is a virus with reverse transcriptase and was actually discovered by analyzing it for reverse-transcriptase activity. In ’82-’83, both in France and the United States—

LIPPINCOTT: Yes, that’s [Robert] Gallo and [Luc] Montagnier.

BALTIMORE: And that's another story. The virus was discovered, and it was clearly a retrovirus; but it was a new kind of retrovirus, nothing we'd ever seen before. So I was aware of all that, and actually had even suggested to Gallo to look for it once, before he found it. So I knew that world, and I knew about the controversies.

LIPPINCOTT: You weren't doing any work on it?

BALTIMORE: No, I wasn't doing any work on it. My lab had moved largely into immunology by that time.

LIPPINCOTT: Did you move over into the Whitehead Institute?

BALTIMORE: Yes, I moved to Whitehead in '84, when it opened. I had a poliovirus group still and kept that through my whole career, but I had moved mostly to immunology. I also had a cancer virus group that was working on the Abelson virus, and that actually fed into the immunology, because Abelson virus transforms immune cells; so that was a core part of it. But I was thinking mostly about immunology.

Walter Rosenblith was a Viennese who had come to the United States and worked in communications, information theory, and things of that sort at MIT. He had risen to be provost at MIT—was provost under [Jerome] Wiesner. Wiesner and Rosenblith built a lot of modern MIT. And I remember that I was at some event, and Walter, who was foreign secretary of the National Academy, came over to me and said, "The National Academy wants to start a committee to think about the national response to the AIDS epidemic, and they would like you to be co-chair." This is 1985; it was when Reagan was president, and there was this homophobia that penetrated the whole government and they didn't want to hear about AIDS. There was just a huge frustration in the scientific community because we knew this was a problem we could work on—a problem we could solve, though it turned out to be harder than anybody imagined. We wanted the government to institute a serious program of research, and many of us had been outspoken about that and had been just totally frustrated by the administration, which didn't want to do it. So when Walter asked me to do this, I said, "It's the last thing in the world I need!" On the other hand, I'm sort of born into it; I can't get away from it. So Shelly [Sheldon M.] Wolff

and I co-chaired that. Shelly was at Tufts; he was a clinical infectious-disease guy, and I came from basic science.

LIPPINCOTT: What did the NAS charge this committee with doing?

BALTIMORE: Developing a national strategy to respond to the AIDS epidemic. To make a judgment about how serious it was, give some sort of prognosis for what was going to happen in the world, and what is it that we needed in order to minimize the devastation. And we wrote a report. It was—may still be to this day—the best-selling report the National Academy’s ever had. It was done as a joint NAS-IOM [Institute of Medicine] report. The IOM and NAS had gotten together to sponsor this report and had used their own money for it, which gave us total independence. We didn’t have to answer to anybody. Usually they get government support, but they knew in this case that they didn’t want the government controlling the report. So we wrote this report, called *Confronting AIDS*, which came out in ’86, and we called in there for a \$1 billion research program. Now, \$1 billion in those days was big money; nobody ever talked billions. And it was my doing. One day in committee discussion— We met under a very tight schedule. We had a great group of people; Howard Temin was one of them. And I came into one of the latter committee meetings, and I said, “Look, we have to recommend a number for a research program, and I’m going to suggest that that be \$1 billion, because that’s a nice round number.” There was no way to *derive* the number. You could say, you know, you needed \$100 million for this, and \$100 million for that, but fundamentally you might as well add it all up to \$1 billion. And everybody agreed to that, so we called for \$1 billion, and of course that was the headline. We had a \$1 billion research program on the books within about three years.

LIPPINCOTT: Was this federal money?

BALTIMORE: This was federal money, yes. The federal government actually did respond to that. So the report was widely read.

LIPPINCOTT: The work was spread over different institutions, I guess?

BALTIMORE: No, it was largely NIH, some CDC [Centers for Disease Control and Prevention]. There are twenty-three institutes in NIH. It was in three or four of those institutes. The major program was in NIAID, the National Institute of Allergy and Infectious Diseases, but there were programs in the Cancer Institute and other institutes.

LIPPINCOTT: Were they mostly devoted to looking for ways to make a vaccine?

BALTIMORE: No. This was before we had drugs, so—

LIPPINCOTT: Oh, like AZT?

BALTIMORE: Yes, that's right. I don't remember when AZT came. We needed to characterize drug targets, we needed to make drugs. We needed to make a vaccine—vaccine was a central issue. And we needed to find out more about the properties of the virus. We needed a research program that was going to characterize every little nook and cranny of the virus, so we could find out where it was vulnerable. And that's what's happened.

LIPPINCOTT: Do you want to talk about the prospects now for an AIDS vaccine, just a little bit?

BALTIMORE: An AIDS vaccine is—I have always said, and I said it in '86 and have said it continually since then—that an AIDS vaccine is at least ten years away.

LIPPINCOTT: Sounds like what they say about fusion.

BALTIMORE: Exactly. It's very similar to that, but that's, like, twenty years away. Basically ten years is as far as anybody can look, and that means that there is no vaccine on the horizon. But it *may* not be ten years anymore, because we have made a little bit of progress. There was just a trial which had, maybe, a positive signal in it. I mean, just on the borderline of positive, and people will argue about it.

LIPPINCOTT: Is the difficulty that the virus is so mutable?

BALTIMORE: A difficulty is that the virus is so mutable. But the virus has also found a variety of ways to make itself invisible to the immune system; and so the immune system, wonderful as it is, simply cannot mount an effective response to HIV, and that's why it kills. Since the immune system can't mount an effective response, making a vaccine involves doing something which doesn't have a natural counterpart. And that's new. All vaccines that we know of take advantage of the natural properties of the immune system. We're going to have to do something unnatural. I've recognized that for fifteen years, but [pause] people are beginning to realize it now, I must say, and there are people thinking differently about the problem for the first time.

LIPPINCOTT: You don't work on it specifically, here at Caltech?

BALTIMORE: Yes, I do. Actually I have a big program. One of the things that we said loud and clear in this report was that it is the responsibility of every member of the scientific community who has anything to offer to this program to make it part of their research effort. So I took that to heart and opened an HIV research unit in my laboratory, and I've had it ever since.

LIPPINCOTT: Around this time, you got embroiled in the [Thereza] Imanishi-Kari problem. Which I don't think we need to go into, because Dan Kevles quite exhausted that subject.⁷ [Laughter] And it turned out well—for you and for Imanishi-Kari.

BALTIMORE: She remains a tenured professor at Tufts University, doing immunology.

LIPPINCOTT: That's good. I read Kevles's book, obviously. It was fascinating to me—the lack of understanding on the part of some of those politicians about what a research paper is. It's not the final word on anything—it's like an invitation for people to come in and fool around and take things in a different direction.

BALTIMORE: The way I describe it is that it's part of an internal conversation in the scientific community, and it has to be seen that way. And it was very hard to get that through. But when you say that it was hard to get politicians to understand that—they willfully did not *want* to

⁷ Daniel J. Kevles, *The Baltimore Case: A Trial of Politics, Science, and Character* (New York: W. W. Norton, 1998).

understand that. Because if they had understood that, then they couldn't make hay out of it, and they had decided that this was going to be their poster child. Specifically, John Dingell [D.-Mich.].

LIPPINCOTT: Do you think Dingell is a villain?

BALTIMORE: Absolutely. I have no doubt. In spite of the fact that he has positive characteristics.

LIPPINCOTT: Yes, he's done some good, but I guess he's garden-variety—

BALTIMORE: He was, at that time, pure evil. He was interested *only* in his personal aggrandizement—how much power could he get over the budget of the federal government; and he would use anything to increase that power.

LIPPINCOTT: Well, it was kind of a sexy topic for him, wasn't it? So that caused you difficulties at Rockefeller.

BALTIMORE: It did.

LIPPINCOTT: And your presidency ended in 1991. OK, then you went back to MIT?

BALTIMORE: Well, no, I stayed on at Rockefeller for a couple of years, but not as president. I had a research laboratory—I'd always had a research laboratory. I expanded it some and stayed there because my wife then had a nice position in New York. We enjoyed being in New York very much.

LIPPINCOTT: Where was she?

BALTIMORE: She was at NYU; she was dean for science. So I didn't particularly want to leave New York. But I then got an offer to come back to MIT, in 1994.

LIPPINCOTT: What prompted you specifically, do you remember, to leave New York and go back to MIT?

BALTIMORE: Well, first of all, as I said, I loved MIT. I appreciated that the president of MIT had called me, days after I resigned the presidency of Rockefeller, and said, “Look, I know that you still have a position here.” I’d never resigned my position at MIT—and that was not a conscious thing; it was sort of a convenience. First of all, many people, when they leave a university, take a two-year terminal leave. It doesn’t cost the university anything, and it provides you with a chance to rethink the decision if you need to. We just hired back here at Caltech, within the last year, three major scientific figures, all of whom had left and were in their two-year period and decided to come back.

LIPPINCOTT: Who were they?

BALTIMORE: Peter Bossaerts. He’s a professor of economics but a very scientific guy. He’d gone to Lausanne. And Dianne Newman [professor of biology and geobiology] and her husband Jonas Peters, who’s a chemist. Both of them had come here as assistant professors; they’d fallen in love here and married and had a child, and they left to go to MIT, which is where Jonas had gotten his PhD. They had terrific offers and they thought it was a good idea—and they didn’t like it. And they came back.

LIPPINCOTT: Too cold. [Laughter]

BALTIMORE: No, not that. They just didn’t develop a comfortable social structure to their lives.

LIPPINCOTT: I guess Caltech was happy to see them again.

BALTIMORE: Caltech was very happy to see them again.

LIPPINCOTT: So you go back to MIT—

BALTIMORE: I still had an appointment; I also had kept a lab at MIT during my first year as president of Rockefeller, just as a way of unraveling, because, you know, there were people who didn't want to come to New York, and it was easier for me to focus my attention when I was at Rockefeller on Rockefeller, not on my lab. I kept contact with the lab by going back to MIT occasionally and seeing the people in the lab, and I did a lot of phoning. So there was a reason to keep people. I had never dropped it, so they didn't have to reappoint me.

LIPPINCOTT: Did you teach, by the way, when you went back to MIT?

BALTIMORE: I began to teach, yes, because I went back as a professor. So I started teaching, and I hadn't taught much for quite a number of years because I'd been involved in running Whitehead.

LIPPINCOTT: So, in 1996 came the final vindication of Imanishi-Kari, and yourself. It coincides with Caltech's search for a new president. That's going to be the meat of our interview; and I wonder whether we should take another session to start that. I want to talk about Kip Thorne [Feynman Professor of Theoretical Physics and head of the faculty search committee] coming to see you, but we'll need some time for that, so this might be a good time to stop. [Recording ends]

DAVID BALTIMORE

SESSION 3

November 25, 2009

LIPPINCOTT: We got you up to 1996 last time. And you had just barely surfaced from the Imanishi-Kari case when the Caltech presidential search committee and Kip Thorne approached you about the presidency of Caltech. What was the timing there? Do you remember when he came to see you and what the circumstances were?

BALTIMORE: No, actually I don't remember a lot. Let's see if I can reconstruct that.

LIPPINCOTT: Was Thorne the one who approached you first?

BALTIMORE: I believe so. I believe he called.

LIPPINCOTT: Did you know him?

BALTIMORE: No.

LIPPINCOTT: He's a physicist, so you wouldn't have had any truck with him.

BALTIMORE: [Laughter] Right. I think I sort of knew about him, and maybe had met him. In fact, I had been interviewed for the presidency of Caltech in the previous round. It would have been ten years earlier.

LIPPINCOTT: When they got [Thomas E.] Everhart [Caltech president, 1987-1997]?

BALTIMORE: When they got Everhart. And I had said no at the time. For some reason I was on the West Coast, in San Diego, and we met in San Diego—something like that. I met with a couple of members of that search committee.

LIPPINCOTT: Were they trustees, do you remember, or faculty? Because there were two separate committees.

BALTIMORE: Well, they were faculty. There are two separate committees, but the trustees' committee doesn't do any of the searching, although they may enter into interviews, I suppose, with the faculty. But the faculty does all the research, which is a remarkable process. There's no other school I know of where the trustees trust the faculty so completely to bring them the right candidates. And I said no, I really didn't want to, at that point, take on Caltech. But people remembered that interview, because I guess I was fairly candid about some things. [Laughter] I've heard about it since. And then when Kip called—I think it was Kip who called—it was a total surprise because I wasn't looking for another job. I'd pretty well decided that I wasn't going to run anything anymore.

LIPPINCOTT: Was this because of your battering?

BALTIMORE: Yes, the experience around Rockefeller and the Imanishi-Kari situation really left me feeling— First of all, it seemed unlikely that anybody would want me, given the trouble that I had gotten into.

LIPPINCOTT: Well, the publicity, some of which was not very flattering—

BALTIMORE: That's right. That's right. So I guess I, without thinking about it terribly hard— because I had never defined myself around my administrative activities—I defined myself around the science I was doing. And MIT had very kindly given me the opportunity to continue my science in a quite effective situation. So that seemed like a good life.

LIPPINCOTT: I've just remembered, now, that when I looked at Mel [Melvin I.] Simon's [Biaggini Professor of Biological Sciences, emeritus] oral history, I think he said he got in touch with you. Did he bring this up with you? I think he says something about that in his oral history. Does that ring any bells?

BALTIMORE: No, it doesn't. I don't think he was on the search committee, because David Anderson was on that committee from Biology. So I think Mel probably wouldn't have been, because they generally choose one person from each division. And I did talk to David about it. It's even possible that he was the one who first contacted me. That's the only other possibility.

LIPPINCOTT: Did you know him well, or had you ever—?

BALTIMORE: Not well. But I knew him because of contacts within the scientific community. And he had also gone to Rockefeller, although a lot later than I did. And he was—and is still—one of the brightest young lights in molecular biology.

LIPPINCOTT: Do you remember when he or Kip—either one—came up to Cambridge to talk to you? Or did you meet them someplace else? [Pause] I'm taxing your memory.

BALTIMORE: You're taxing my memory. You are.

LIPPINCOTT: [Laughter] I'm sorry.

BALTIMORE: You would think it would be ingrained in my memory, but—

LIPPINCOTT: Well, it's thirteen or fourteen years ago.

BALTIMORE: It is, but still it was pretty important, but I don't remember. I don't remember. I think not. I think I did not meet with them until I came out to California to talk to the whole search committee. And that was very funny, because Kip was famous for having run this search under the highest of security.

LIPPINCOTT: It was all off-site, right? You didn't meet on the campus?

BALTIMORE: No. We in fact met in the place that Kip calculated was the least likely place to run into anybody in Los Angeles.

LIPPINCOTT: Where was that?

BALTIMORE: This was—what's the name of that hotel, on the Westside, with little cabins—the Bel-Air! So he brought Alice and me out to the Bel-Air, and I was interviewed there by the committee. It's the only time in my life I've ever stayed at the Bel-Air Hotel; it was very elegant. There must have been a previous conversation, but exactly how it took place is what I can't remember.

LIPPINCOTT: But you were obviously interested, because you came out to—

BALTIMORE: Well, I was intrigued, and, yes, I was interested enough to do that. I think I was pretty clear that I wasn't making any commitment; they were very clear they weren't making any commitment. And I still, even then, didn't believe that I would end up being their choice. Because it just seemed so unlikely.

LIPPINCOTT: Well, you would have been the first biologist to be the president of Caltech, I believe.

BALTIMORE: Yes, I was the first biologist, but that didn't seem so unlikely, because biology was in the air. Everybody was talking about the twenty-first century as the century of biology. And biology had become, already, notably interdisciplinary in its activities, so for a school like Caltech— And they told me they had passed a biological science initiative and were in the process of that while they were talking to me. So there was no question about Caltech's commitment to biology and about the fact that biology was driving the thinking of so much of science—in physics and chemistry and engineering and computer science, everywhere.

LIPPINCOTT: Were these people on the search committee your allies, more-or-less, in the Imanishi-Kari case? That didn't bother them?

BALTIMORE: Well, they looked into it very carefully. It is said that Kip talked to everybody whom I had ever had any contact with. [Laughter] Because I heard back from some of them that they had been contacted. He was going very deep; it was estimated that he talked to a hundred people. He did a lot of it himself—and I think he was taking a huge chance in approaching me because of the Imanishi-Kari incident. I don't know if he was more careful than he would have

been about somebody else, but he was certainly careful. And I wanted him to be. That was really the only reason why I was willing to entertain the idea, because I wanted to be absolutely sure that they knew what they were getting into. Of course, Dan Kevles [Koepfli Professor of the Humanities, 1986-2001] was here, and Dan knew more about me than I know about myself. [Laughter].

LIPPINCOTT: He'd already interviewed you for his book.

BALTIMORE: Well, he'd already *written* his book, I think. Remember, the *New Yorker* article came out before the resolution.⁸ Then it was resolved, and then later on his book came out.

LIPPINCOTT: So Kip talked to Dan, too, probably. Or maybe not, if you say Kip held it closely.

BALTIMORE: Oh, but I think Kip must have talked to Dan. And Dan was chairman of the faculty at that point, so he had actually appointed Kip. No, I'm quite sure he talked to Dan.

LIPPINCOTT: Do you know who any of the other candidates were?

BALTIMORE: I've since heard about a couple of them.

LIPPINCOTT: Were they all biologists?

BALTIMORE: Well, the ones I've heard about are biologists. I don't know the full list. I once asked Kip to tell me who the finalists were because I didn't want to be in the position of talking to them and not knowing they had been finalists. He told me at the time, and I've now forgotten most of them. [Laughter] You can ask Kip.

LIPPINCOTT: OK. Well, how did these conversations go, with the search committee at the Bel-Air Hotel?

BALTIMORE: I guess they went well because they asked me back.

⁸ "The Assault on David Baltimore," *The New Yorker*, May 27, 1996, pp. 94-109.

LIPPINCOTT: You don't have any visual recollections, or—?

BALTIMORE: Well, it was a very thoughtful conversation. They were a terrific group. And Kip of course was the clear driving force, and he was fabulous—he *is* fabulous—in the depth of his understanding of Caltech, the kinds of considerations that he brought up. We talked about everything from the role of the presidency, the value system that I care about, the—

LIPPINCOTT: I think Mel Simon says that you wanted a lab, also, if you were to come as president. Is that right?

BALTIMORE: It is right, but I didn't make that decision right off. I don't think that came up in the first conversations at all, and probably not when I came out for— Because I finally came out to meet all the— I see: When I met Mel was when I met all the division chairs. I didn't do that the first time I came out. And Mel was the Biology Division chair, and that's why he knew about [my wanting] the lab.

LIPPINCOTT: When was that second meeting? The first one must have been late in '96 because I have the date of your appointment, which was May 13, 1997. So, maybe, this was—

BALTIMORE: I think it may have been in January of '97 that I came out the first time, and the second time was probably in March or so.

LIPPINCOTT: Was that second meeting off campus too?

BALTIMORE: It was at the Huntington Hotel—what is now the Langham—because I was now going to meet a lot of the local people. Now, that second meeting may not have been in March, it may have been later than that. At this point, they were as much interesting *me* as they were vetting me. In fact, I think at that point they had said that if I was comfortable with it—and I can't remember, but I think it was all my decision at that point.

LIPPINCOTT: And you met all the division chairmen?

BALTIMORE: Yes, and Kip's statement to them was, "Don't hold back anything. I don't want David to come here and say that we didn't tell him whatever."

LIPPINCOTT: Whatever problems there were.

BALTIMORE: Whatever problems, responsibilities, perceptions were around, he wanted them laid out. So they were, as far as I know, very honest. And when I came, it was true, there were no surprises that I could lay on the head of anybody else. [Laughter] You know, when you aggregate it all, of course there are surprises, but—

LIPPINCOTT: Steve [Steven E.] Koonin was provost at that time?

BALTIMORE: Steve was provost, and I met with him. I even met with Tom [Everhart], I think, if I remember correctly.

LIPPINCOTT: And you kept Koonin as your provost.

BALTIMORE: I did.

LIPPINCOTT: I guess he was pretty useful to you.

BALTIMORE: Well, it was a terrific opportunity to have somebody with deep knowledge of Caltech working closely with me; because I knew nothing about Caltech other than what I'd been told. I had not ever spent time on the campus beyond giving a couple of lectures and coming out here for Norman Davidson's eightieth birthday, so I'd been here three or four times.

LIPPINCOTT: Yes, and people important to you, like Dulbecco and Temin were Caltech people.

BALTIMORE: Right. I knew all of the background of Caltech and the mystique of Caltech. In fact, I can tell and do tell even to this day, about the role of Caltech in modern biology, which is not appreciated on the campus, even. How deep it was. Because Caltech was generally viewed—still is viewed, on the outside—as a physics school, as an engineering school, but not as

a biology school, in spite of the fact that from the time that [Thomas Hunt] Morgan came here in 1928, Caltech was the center of genetic biology almost in the world.

LIPPINCOTT: Yes. [George Wells] Beadle and people like that.

BALTIMORE: Beadle, and Delbrück.

LIPPINCOTT: And Norman Horowitz.

BALTIMORE: And Norman Horowitz, and others. It was an extraordinary place—first in non-molecular genetics and then, with the structure of DNA, in molecular genetics.

LIPPINCOTT: And Lee [Leroy E.] Hood and his machines.

BALTIMORE: Yes. That moves you away from pure genetics to something more biochemical. His first machines were protein-sequencing machines, which were very important, both from a genetic point of view and from a physiologic point of view.

LIPPINCOTT: So that must have excited you. You weren't coming to a biological wasteland from MIT.

BALTIMORE: Oh, no! I felt like I was coming to the place that really held the flame of biology. I was a little disappointed, in a way, that some of those characteristics—the very central role of Caltech in biology—were very much in the past.

LIPPINCOTT: Do you think it fell down a bit?

BALTIMORE: Well, it certainly fell down a lot, but it's not entirely Caltech's fault. At the time when Caltech was the central place in biology, there was almost no one else doing it—doing molecular biology.

LIPPINCOTT: Is this back in the days of Beadle you're talking about?

BALTIMORE: No, it's in the days of Delbrück, in phage genetics, where Delbrück had gathered around him Steinberg, Stahl, Meselson—although Meselson was actually working with [Linus] Pauling—and Bob Edgar and others, Bill Wood—it was an extraordinary group of people.

LIPPINCOTT: That kind of work was going on at Cold Spring Harbor, too.

BALTIMORE: It was, but Cold Spring Harbor wasn't a university. And Cold Spring Harbor actually played its largest role in the summer program, in training people. There were a couple of very, very great core people there—Al Hershey, notably—but it wasn't the same training school that Caltech was.

LIPPINCOTT: Yes. Well, in 1996, I also want to mention the NIH's AIDS vaccine research committee, which you consented to chair while you were also contemplating the Caltech presidency. Did you think you could do both of those things?

BALTIMORE: Yes. The advisory committee for NIH was a few-times-a-year trip to Washington. It was part of my continual traipsing back and forth across the country, which I did, and which I continue to do, at a minimum, once a month.

LIPPINCOTT: Was it essentially just promoting research?

BALTIMORE: No. We were a committee to advise the NIH vaccine program on vaccine strategies, and we had people from around the country who together were probably the strongest group of people thinking about the AIDS vaccine in the country. Now, we had our frustrations—a major frustration being that we really couldn't affect policy the way we would have liked to, because they didn't want to hear from us.

LIPPINCOTT: Who didn't?

BALTIMORE: The people who actually ran policy in Washington.

LIPPINCOTT: OK. So on May 13th you were appointed Caltech's president, and then your inauguration was—

BALTIMORE: And that was an incredible moment.

LIPPINCOTT: The inauguration?

BALTIMORE: No, when I was appointed; because I had agreed sometime before then, and they set up that date, and I was out here, and they held a faculty meeting to introduce me. And when I walked into the Hall of Associates, where the faculty meeting was, and was introduced, no one except the committee and the division chairs knew who was going to walk through the door!

LIPPINCOTT: [Laughter] That must have been exciting! Was there a gasp, or what?

BALTIMORE: Well, you know, there was applause, and everybody was very welcoming; they were very welcoming. Caltech was enormously welcoming. But Kip had really kept a clamp on the whole thing.

LIPPINCOTT: That's amazing.

BALTIMORE: Yes, it really is. To the extent that he had limousines with tinted windows, so you couldn't see in. One time I came out, my daughter came with me and Alice; and Teak wanted to use a computer because she wanted to keep connections. She lives her life through computers. So somebody said that they would get her a computer over at the campus and took her over there. But I was not to appear on the campus; and they said to her, "If anybody asks you what your name is, your name is Huang"—which is Alice's name—"not Baltimore."

LIPPINCOTT: [Laughter] That's funny. OK. I was at your inauguration. I remember it—a very nice day.

BALTIMORE: A very nice day. It was actually blessed that it was a nice day, because that was the El Niño winter, and we had had rain before, and we had rain after, but that one day was gorgeous.

LIPPINCOTT: Well, there you go.

BALTIMORE: An omen, right.

LIPPINCOTT: Do you remember the commencement—maybe you weren't here. They used to sing the *Hallelujah Chorus*, and when they got to "And he shall reign for ever and ever," it started raining. [Laughter] Wonderful! OK. Maxine Singer spoke at your inauguration; she's a friend of yours.

BALTIMORE: She's a friend of mine.

LIPPINCOTT: Were you the one who asked her to speak?

BALTIMORE: Yes—whether I asked her directly or somebody else literally asked her I don't remember, but it was my choice.

LIPPINCOTT: She was at the Carnegie Institution at the time?

BALTIMORE: She was at the Carnegie Institution. She knew me from when I was an undergraduate at Swarthmore. She had gone to Swarthmore and came back for a visit—she's significantly older than I am—but came back for a visit during the time I was an undergraduate and remembered me from there. And then I had seen her on and off; she'd been very much involved in the Asilomar conference.

LIPPINCOTT: Right. It was a good talk, I remember.

BALTIMORE: It was a great talk—a lot of it was about [George Ellery] Hale. I was already feeling that this was Hale's university—although everybody calls it Millikan's university, and

Judy's book calls it that.⁹ So I did a lot more reading about Hale after that and discovered how deeply Hale was involved. This is Hale's university.

LIPPINCOTT: Yes, I guess that's right. And there are some Hales left over here in Pasadena.

BALTIMORE: There are occasional Hales around, and I've run into them. And I tried to work with the Huntington Library to save the Hale observatory [Hale Solar Laboratory].

LIPPINCOTT: Yes, I know that observatory. It's on Holladay Road.

BALTIMORE: It's on Holladay Road and backs right onto Huntington property.

LIPPINCOTT: Isn't it still there?

BALTIMORE: It is still there. It was finally bought by private people, who have maintained—actually I think improved—the telescope. It's a separate little building on the property.

LIPPINCOTT: I've been in it.

BALTIMORE: Yes, so have I.

LIPPINCOTT: It's beautiful.

BALTIMORE: It is. It's a wonderful, wonderful place. It was being run as the headquarters for the Mount Wilson Observatory. [Robert] Jastrow, who ran the Mount Wilson Observatory, had his lowland headquarters there, but I don't know whether that's still true.

LIPPINCOTT: It's not being used as an observatory?

BALTIMORE: No, it's not being used as an observatory.

⁹ Judith R. Goodstein, *Millikan's School: A History of the California Institute of Technology* (New York: W. W. Norton, 1991).

LIPPINCOTT: It looked like a library to me, when I was there.

BALTIMORE: Yes, it was much more a library, and it had some meeting rooms and stuff in the first floor. But then it goes way down deep. That's where the telescope is. You have to bring the light down and then reflect it up and focus it. There was a piece about the telescope on campus that Hale built in Robinson [Henry M. Robinson Laboratory of Astrophysics], and they're now going to use that as a water-storage place to air-condition Robinson. It's worth reading; it's in the last, or second to last, *E&S* ["Subterranean Homesick Clues," by Michael Rogers and Heidi Aspaturian, *Caltech News*, Volume 43, No. 2—ed.].

LIPPINCOTT: I'll look at it. OK, so now you're launched into the presidency; and there are a few things to talk about there. One thing Caltech did was buy St. Luke's Medical Center [2003]. Was that your idea?

BALTIMORE: It was. St. Luke's was being closed by Tenet [Healthcare Corporation], and I was very concerned for the long-term health of Caltech and for the ability of Caltech to do new and interesting things. But it needed space.

LIPPINCOTT: For biology?

BALTIMORE: No, not particularly for biology—not for anything in particular. It's just that the amount of space on campus is limited. It's not zero; there's still a fair amount of space on the northern part of the campus for expansion, but that'll run out. We're landlocked here; it's very hard to extend our profile. And so I spent time, over the whole time I was president, thinking about that problem. There was a time when I was talking with the Polytechnic School about perhaps trading them out of their space there and taking that over.

LIPPINCOTT: Off Arden Road? Or off Wilson?

BALTIMORE: Well, the whole Polytechnic School. They could have moved over to the Ambassador property, which was available, and which the Maranatha School ultimately took. So there was a lot of discussion about that at the board level of Poly and our board level, but it

never came to pass for a variety of reasons, including the difficulty of actually doing much building on that campus, because of zoning issues, and where it was, and whatever. And I'm not sure we could have ever put together the resources to help them do that. And then St. Luke's came up, and it seemed like a tremendous opportunity. So we contacted, with the help of Wally [Walter L.] Weisman who was the board vice chair, the president of Tenet and asked him whether he'd consider such a thing. Well, he was actually very positive about the idea.

LIPPINCOTT: They'd already had the idea of selling it?

BALTIMORE: Yes, they had announced that they were leaving and closing it, so there was no question that it was up for sale. The city didn't want to see the property become a real estate development, which it could have—and I suppose may yet. So I arranged to purchase it for \$12 million, which was a steal at the time. And they were being very good about it; it *was* a steal. It wasn't clear who else was bidding for it. So we bought it, and we did put one program up there—the 30-meter telescope design program—and we outfitted the old cloister there. Because there was also a little chapel—a lovely room—and there was a religious group that lived there, in little rooms.

LIPPINCOTT: Like an abbey, or a monastery?

BALTIMORE: Yes, it was like a monastery, but it was women, I think. And that was separate from the hospital and lent itself best to adaptation to the needs of the 30-meter telescope project. So the project moved up, and it's still there, as far as I know; I don't actually know what's going on up there now. Then we looked at what it would take to recondition the hospital building for various sorts of projects that various people had in mind, and it turned out to be just hugely expensive.

LIPPINCOTT: To make labs there?

BALTIMORE: To make anything there, because of the need to deal with the whole structure. You couldn't just take a piece of it and do something; you had to really work on the whole structure, because it needed electrical work, plumbing, and all that sort of stuff. So the up-front

cost was going to be enormous, and we didn't have any way to pay for it, or any thing that was compelling to do there. I imagined that various things might happen, but none of those really did happen. There was not the kind of need for it that I thought there might be in the short run, but my real interest in this was in the long run, not the short run.

LIPPINCOTT: Right, and there it was, and you had to see if—

BALTIMORE: Right, and if we didn't do it then, we were never going to do it. Over time, it became a little bit of a liability, because just maintaining it had a real cost. You had to heat things and whatever else—keep things in shape.

LIPPINCOTT: Mow the lawn.

BALTIMORE: Yes. The major use for it—and it more or less paid for itself that way—was filming. Because here was a hospital; and television and movies are full of hospital stuff, and so they did a lot of filming there and that made us a lot of money. But in the process of really trying to deal with the financial problems of Caltech, we ultimately decided to sell it—and did. And in the end we made a fortune, because we sold it before the economic downturn. This happened while Jean-Lou [Chameau] was president [1997-present]. They found a buyer, and the buyer paid a lot more than I paid, so it was a huge financial gain for Caltech, but it was at the cost of going back to not having any expansion space. And I tried to insist that the gained money be put aside for purchasing land around us, because we do have a lot of buildings, particularly to the north of us, beyond Del Mar. There are a lot of apartments and whatever there, and we own a fair amount, and we can continue to aggregate property there. The only thing we've built there is the Financial Services Building, which is just across Del Mar from the campus. But we had talked about building a child-care center.

LIPPINCOTT: There's a small one already, on the other side of the street, I think.

BALTIMORE: Yes.

LIPPINCOTT: Where did the 30-meter telescope designers go after they had to vacate St. Luke's?

BALTIMORE: I don't believe they vacated it, because I don't think anything has happened to that property. I think developers bought it with a plan in mind, but it was a long-term plan, and I don't actually know what's happened. But I do know that—initially, at least—they said, “Well, we'll just leave the 30-meter telescope project going.” There's also an office building there, which has another Caltech project in it—a geology program. And that I don't know anything about. I should find out; you've piqued my curiosity.

LIPPINCOTT: In the meantime, you did have a lab of your own while you were president.

BALTIMORE: I did.

LIPPINCOTT: How could you manage that?

BALTIMORE: Well, when people asked me, “How could you manage that?” I said it was my hobby, and that absolutely stopped any questions. Because everybody feels that a CEO should have a hobby. So they thought, “Oh, that's very appropriate!”

LIPPINCOTT: Like golf.

BALTIMORE: Like golf, but I don't play golf. I have some little hobbies; I fish, and I do some things like that, but—

LIPPINCOTT: Did you spend time every day in the lab?

BALTIMORE: No, not every day, by any means.

LIPPINCOTT: Was it devoted to immunology?

BALTIMORE: Yes, I focused the lab entirely on immunology and things that relate to immunology, and I downsized tremendously from what I had been doing at MIT. But I kept a functioning lab going, and we made, actually, some quite interesting discoveries during that time.

LIPPINCOTT: Did you bring people from MIT to your lab?

BALTIMORE: I brought some people from MIT and hired some people *de novo*.

LIPPINCOTT: What about the new Cahill Center for Astronomy and Astrophysics? Were you behind that?

BALTIMORE: Right. Let's cover two things—well, but it's all one—which is fund-raising.

LIPPINCOTT: Oh, right, there was the capital campaign to raise \$1.4 billion. Weisman was the chairman of that?

BALTIMORE: Yes. When I came here, as I said, they had already announced the biological sciences initiative, which was a sort of test campaign, from a financial point of view, and was an attempt to shore up the facilities for biology, and the range of biology, on the campus. It was consciously an integrative activity—to help engineering get into biology, to help other people get into biology.

LIPPINCOTT: Were they trying to get neuroscience?

BALTIMORE: No, it was not focused on neuroscience, which already was an existing and very strong program that was very interdisciplinary. There was a sense to use that as a model, but the initiative didn't focus much on neuro itself; it focused on developing the genetic technologies, and developmental biology, and questions like that—network thinking, synthetic biology, the new kinds of approaches to biology. There was a commitment to build a new building and to hire a number of faculty; and ultimately it was a \$100-million effort. So that was my first responsibility when I came here—to carry out the biological sciences initiative. And I certainly could do that from the point of view of explaining what was going on, but I had to find the donors and the people who would be interested in supporting that. So that occupied a lot of my time and energy. It introduced me to Eli Broad.

LIPPINCOTT: That's when you met Broad?

BALTIMORE: Well, he was a member of the Board of Trustees, so I'd met him, but I got to know him better through trying to interest him in building what became the Broad building [the Broad Center for the Biological Sciences]. You know, there were a lot of fits and starts in that before we found the right path and the right level of interest. But we built the building.

LIPPINCOTT: The architecture's very unlike what it's next to.

BALTIMORE: Right.

LIPPINCOTT: It looks a little like MIT. [Laughter]

BALTIMORE: No, no, no. MIT at that point had, actually, terrible architecture.

LIPPINCOTT: Now it has some very funny-looking thing by [Frank] Gehry.

BALTIMORE: Well, now it does, but it didn't then. You're obviously not a lover of contemporary architecture. [Laughter]

So Eli said to me, in our final negotiation over his funding the building, "One of the requirements is that we get a notable architect." And I said, "That's fine; I think that's what the campus needs anyway." The campus had great architecture until the 1950s, and then almost everything built after that is just functional—or eclectic, like the Beckman Auditorium.

LIPPINCOTT: Oh, yes—that's awful, I think.

BALTIMORE: Well, it was Edward Durell Stone, who was the last great architect—but Stone at that point was very influenced by, and had just been to, Morocco, so he built us a little Moroccan temple. It's an odd thing to have on our campus. And then we had the big buildings—well, Millikan Library, which everybody dislikes and which was sort of foisted on us by the donor—and then the big buildings that flank the court.... [Pause]

LIPPINCOTT: It's called the Court of Man.

BALTIMORE: Yes, it's called the Court of Man, for some reason. Which are big and sort of—just wrong. [Laughter] But anyway, they do the job, and the space is terrific, and we get a lot of work done. But if you look at the engineering buildings—Keck [W. M. Keck Engineering Laboratories] and Spalding [Eudora Hull Spalding Laboratory of Engineering]—they're all very uninspired buildings, and certainly uninspired by the history of Caltech architecture as well as by the contemporary trends of architecture. There was an attempt to go back to build in the style of the original, of [Bertram] Goodhue and [Myron] Hunt, and, actually, I inherited a master plan which explicitly said we would do that. And the first building that was built according to that plan—which I had nothing to do with—was the one that Gordon Moore supported, the Moore building [the Gordon and Betty Moore Laboratory of Engineering]. The Moore building has, in the front of it, an arcade, very much reflecting the arcade that is in the courtyard between the biology and the physics buildings. So I said, "Look, from my point of view, that's the wrong thing to do. You don't try to go back and re-create history. Architecture is a moving influence, and if you want to get good architecture, you've got to move with the influence of the contemporary architects. And if you want to get throwbacks, well, you can build them, and there they are, and they'll do the job. But in that particular case— You know, the arcades are the most wasteful thing you can imagine, in terms of space utilization. The same thing is true—although it's a building I really like—of Avery House. So I said, "It is time to break with this," and I had to convince people, and I had to convince the Pasadena City Council. They had an architecture group we had to convince, and I had to appear before them personally and argue for the building, but I was able to get them—

LIPPINCOTT: Did you choose the architect?

BALTIMORE: Well, I was involved in choosing the architect, along with a trustee committee that Eli was centrally involved in. It had to be somebody Eli would appreciate. And I don't think, from a donor's viewpoint, that's a bad requirement—particularly somebody like Eli, who's very aware of the currents in contemporary architecture and really loves architecture. I will never forget the session we had in which we interviewed four or five of the world's great architects and finally chose Jim Freed to do the building.

LIPPINCOTT: Who else did you interview?

BALTIMORE: We interviewed the firm of Richard Meier—actually, in the person of somebody else; it wasn't Meier himself who came. And—what was the name? An Italian, a local guy, who was a very good architect. And....[Pause]

LIPPINCOTT: Koolhaas?

BALTIMORE: No, Koolhaas was not considered for that building. He was considered for another building, and that's a different story.

LIPPINCOTT: Oh, the Annenberg Center [the Walter and Leonore Annenberg Center for Information Science and Technology].

BALTIMORE: The Annenberg Center. Let me see if I can remember who else. Names, names, names. It's well documented, but I can't reconstruct it right now.

LIPPINCOTT: When you see the transcript, you can pencil it in [One was Robert Stern from Yale—D. Baltimore]. Was Broad happy with the building?

BALTIMORE: I think Broad was very happy with the building. Along the way to build the building, what we needed was going to cost more than we had, so we started downsizing a lot of the concepts for the building.

LIPPINCOTT: Was this before the capital campaign?

BALTIMORE: Oh, yes, this was well before the capital campaign. Broad finally, actually, came up with more money so that we could put marble—travertine—on the exterior and the steel panels on the other side, because otherwise the building was just not going to be the great building it could be. I think it's one of the most beautiful buildings not only on the campus but in all of this area.

LIPPINCOTT: What's done in there? Your own lab is in Braun Laboratories?

BALTIMORE: My lab is in Braun. The major things [in the Broad Center] are structural biology. The structural biologists we had on campus moved there, and new ones whom we've hired have gone in there—one of them does cryoelectron microscopy, which gives you a special resolution ability to look at three-dimensional structure. He's great. He was a young hire that we made because we had the building and the commitment. Ultimately, the Moore gift—which we haven't gotten to yet, and which was probably the most important thing I did—funded the buying of equipment for the Broad building; because although we put aside space for these things, we didn't have the money to do that. That was the cryoelectron microscope and the magnetic resonance imaging facility that's in the basement; we needed another \$20 million to do that, and we got it from the Moore gift; that's been as important to the campus as anything we've done. So there's a lot of structural work being done there, and there's a lot of engineering interface work being done there—nanoengineering. They're doing some work using DNA structures in that building. And then there's some developmental biology there and some other things.

LIPPINCOTT: How did the capital campaign get started?

BALTIMORE: As time went on, it became clearer and clearer to me that Caltech's financial model would not support the long-term development of the school and that we needed more money in our endowment and we needed a real shot in the arm to get our programs in gear—all of our science programs. In talking about that with the then head of finance and the head of development, we decided that we just had to have a capital campaign. That was about three years into my being here, something like that.

LIPPINCOTT: Yes. The kickoff was in 2002, so you must have been talking about it before that.

BALTIMORE: Right. There was a trustee meeting in Hawaii where we sat down and really talked this thing through—

LIPPINCOTT: And arrived at this \$1.4 billion figure?

BALTIMORE: No, we didn't arrive at that right away. We knew it had to be big, but there was an outstanding question, and that was, What would Gordon Moore do? Now, we haven't talked

about this, but when I was being considered for the presidency, one of the meetings that I had, which was most important, was with the trustees. I came out here I think separately for that meeting. Because ultimately it was the trustees' committee that was going to choose the president, from a list that was given to them. And I'm told that I was on the top of the list that they got and that they really weren't sure about any of the other candidates—so they didn't know what they'd do if they didn't convince me. And when I came out and talked with them, it was clear that Gordon Moore was prepared to make a big gift.

LIPPINCOTT: Was that contingent on your accepting the presidency?

BALTIMORE: No, it wasn't contingent on it, but *if* I accepted the presidency, he was prepared—I don't know if it was only me—but he was prepared to make a big gift in the future. And “big” was [pause] big. We didn't know how much it was.

LIPPINCOTT: Had you met him before?

BALTIMORE: No, I had not met him. So when I came here, I began to try to figure out when this gift would materialize and what it would look like, and nothing was forthcoming for a couple of years. But I talked a lot with Gordon, and he was pretty open about things. One of the things that became clear was that without a campaign we probably weren't going to convince Gordon—or we didn't know *how* we were going to convince Gordon, let me put it that way. I don't want to say that he was renegeing on anything, but it just wasn't clear what the path forward was. So one of the subtexts of the campaign was to provide a structure through which Gordon could make the gift which he had said he would make. Meanwhile, much to my horror, the value of stock in Intel was falling. And Gordon, who had been worth at one point—I can't remember whether it was before or after I came—\$23 billion or some number like that, saw his net worth falling and falling and falling. And that worried us, of course. It meant that some of the numbers that were in our heads were never going to materialize.

LIPPINCOTT: It wasn't just Intel, was it? Was the whole market going down?

BALTIMORE: It was Intel in particular. Yes, the whole high-tech world came apart in '99 and 2000, that's true. And his wealth was almost entirely in Intel; he had never diversified. I had once, just in an attempt to jump-start this— I must say, I worked very closely with Wally Weisman in all of this. Gordon was the chairman of the Board of Trustees and Wally was the vice chairman. I presented Gordon with a plan for a \$2 billion infusion into Caltech, and he was not interested at that point in doing that; so he more or less rejected that plan, so we needed Plan B. And the campaign was Plan B.

So Wally, who was now going to be chairman of the campaign and was my ally in all of this, was trying to figure out how to get to Gordon, to make clear to Gordon that he had to make a commitment to his gift before we structured the campaign because it was going to determine the size of the campaign and everything else that we did. Wally one day opened his mail and found a letter from Gordon saying that “I will commit \$300 million from my foundation”— which he had just then established—“and an equivalent amount of money in Intel stock.”

LIPPINCOTT: So, \$600 million total?

BALTIMORE: Six hundred million total. Actually, he didn't say how much. He said in the letter that he would give a \$300 million draw on his foundation over ten years and a certain amount of Intel stock—I've forgotten what the number was—which would come in over time. The stock gift—he consciously said in the letter and had said in our discussion—was a way of shifting the uncertainty of the value of the stock from him to us, because he was giving it to us in share numbers, not in dollar denomination. That was a thing I understood. I understood from his point of view why he was doing that, because then he could define this gift, whereas he couldn't define it otherwise, because it was a shifting resource. So *I* calculated its value.

LIPPINCOTT: You looked into the future?

BALTIMORE: I looked into the future. I made a very conservative guess—I thought I was being conservative; in the end, actually, it was less than I thought. I think the stock was twenty-seven at the time, and I said “Well, let's figure we could get thirty for it”—because the stock was way down at that point—and sized it on thirty. So I called Gordon and I said, “Gordon, we have to announce this gift.” He said, “Do we really have to announce it?” I said, “Yes, Gordon, we have

to announce it.” Gordon’s humility is unbelievable; he didn’t want to announce it, not out of any embarrassment—he just didn’t think it was important, when it was the most important thing that had happened to Caltech. It was the largest single gift ever given to an academic institution. It was going to be the kickoff to the campaign, so I *had* to announce it—there was no question—and I had to announce a number. I couldn’t tell everybody in the world about the complications of it, because they don’t want to hear it. They want to hear a number. So I said to Gordon, “I estimate it at \$600 million. Can I use that number?” And he said, “Yes.” So we announced that, and it became the basis of the campaign. And so we figured that we could raise \$800 million on top of that, and that’s where the \$1.4 billion came from.

LIPPINCOTT: And you did?

BALTIMORE: And we did. So, as part of this, we had to have a building plan, because in fact the big money was going to come in for buildings. It always does in these campaigns—except for Gordon, who gave money that largely ended up in the endowment. And one of those buildings was an astrophysics building—I’m answering your earlier question.

LIPPINCOTT: Yes, you’re right. Well, had the astrophysics people been feeling pinched? Were they in Downs [George W. Downs Laboratory of Physics] or something like that?

BALTIMORE: They were strewn around: They were in Robinson; they were in Downs; they were in various places. They’d never had the building they wanted. In 1965, the then president [Lee DuBridge] agreed to build them a building. It was in the previous campaign, and they never raised the money for it, but they had committed to building a building for the astrophysicists. This thing had been hanging on and hanging on and hanging on. So I said, “This is going to be something we do in this campaign—give the astrophysicists the building they need.” So we found Mr. [Charles H.] Cahill—actually, Tom [Thomas A.] Tombrello [chair of the Division of Physics, Mathematics, and Astronomy 1998-2008] did a terrific job in raising a lot of the money—and we built the building.

LIPPINCOTT: What did they need a lot of space for? Isn’t it just cerebral stuff?

BALTIMORE: It's more to bring them together into one place. That's, if anything, the most important thing about it—and for meeting space and other things; there's very little experimental space. There's some space for building equipment and things.

LIPPINCOTT: But they all go to the Keck [Telescopes] if they need equipment, is that not right?

BALTIMORE: You mean actual telescopes—the telescopes are elsewhere.

LIPPINCOTT: I was just wondering why they needed a whole lot of space, but you say it was just to bring them together.

BALTIMORE: Yes; it wasn't actually going to be a net gain of space; it was to bring them together.

LIPPINCOTT: Were they doing more hiring in astrophysics then?

BALTIMORE: Not a lot. It wasn't a big expansion, but they had, over the years, hired people and they'd put them here and they'd put them there and they'd put them all over—strew them around other physicists.

LIPPINCOTT: That's a very strange-looking building, too.

BALTIMORE: So, again, I committed us. And this time I did it—and actually with Mr. Cahill not being particularly happy about it—to get a first-rate architect. And again we had a competition and we looked at a variety of architects and chose Thom Mayne.

LIPPINCOTT: You say Mr. Cahill wasn't happy?

BALTIMORE: No. He found it all very strange.

LIPPINCOTT: Who is Mr. Cahill? I don't know anything about him.

BALTIMORE: I don't remember. He actually was not a Caltech person, but he appreciated astrophysics and he wanted to do this.

LIPPINCOTT: So he was a layman, and he was devoted to what they were doing?

BALTIMORE: That's right. He had made money in real estate and elsewhere, and the gift came in very strange bits and pieces of real estate and money and—

LIPPINCOTT: Are you fond of the design?

BALTIMORE: I think it's terrific. I think it's just extraordinary. I said to myself, "This is going to be the premier spot on the campus to build a building." Because it's on California, more people would see it.

LIPPINCOTT: Very visible.

BALTIMORE: Very visible. The Broad building is very visible on Wilson, but there isn't that much traffic on Wilson, whereas there's huge traffic on California. So this was going to be sort of the defining image of Caltech, and I thought it ought to be really special. Now, in the end—

LIPPINCOTT: It's the color that's startling.

BALTIMORE: Yes, it is, actually. But the color really is meant to reflect the Mediterranean colors of the old buildings—the tiles on the roofs of the South Houses. And it's not exactly the same color as that; it's not as red; it's a little rustier. But—

LIPPINCOTT: Maybe it'll fade with time.

BALTIMORE: No, I don't think it will fade with time.

LIPPINCOTT: [Laughter] Before we get off this subject, can we talk a little bit about the Richard Serra controversy?

BALTIMORE: Oh, yes. Again— I mean, I have a real interest in contemporary art.

LIPPINCOTT: I know.

BALTIMORE: I love it; I have a collection of my own, which used to grace the walls of the President's House. Richard Serra is the greatest living sculptor. You like him or you don't like him, but he is the greatest living sculptor. When we built the Broad building, we agreed to keep that lawn free of further building, and in fact, in the master plan, we traded some space into there, so that we would have the opportunity to build a campus center building elsewhere, but we would keep that lawn open. And that was partly homage to Arnold Beckman, because although Arnold at that point was very old and he himself wouldn't have thought much about it, his daughter, who was a member of the board, was tending his image, and she didn't want to see—we didn't want to see—the Beckman building, which sits on that lawn, blocked off. Not because it's great architecture. It's actually terrible architecture. But—

LIPPINCOTT: Were you thinking of the lawn maybe as a site for some kind of public art?

BALTIMORE: Right. So that lawn now looks ridiculous. And we knew it would look ridiculous, because it doesn't have a tree on it, it doesn't have— Well, now it has the set-up for the construction, but it will go back to being grass.

LIPPINCOTT: Oh, will it? It looked to me like they were going to build something on it.

BALTIMORE: No, they're not building anything on it; it's all support for the Annenberg building and the new chemistry building [the Warren and Katharine Schlinger Laboratory for Chemistry]. And it desperately needs something. I thought it would be a great site for a sculpture. Now, I imagined the sculpture being on a corner near the Broad building. But when I got Richard Serra to come out and look at it, and I got Eli to agree to pay some of the money for it— It would cost us a little bit more than that, and I never did figure out how I'd get that money, but I was convinced I could get it.

LIPPINCOTT: So you were the one who approached Serra?

BALTIMORE: Yes.

LIPPINCOTT: Did you like him?

BALTIMORE: Oh, he's an *awful* man! Oh, he's terrible!

LIPPINCOTT: In what way?

BALTIMORE: He is egotistical. He is bombastic. He is crude. He's just the greatest living sculptor. But he's not nice, and he was not helpful. He designed something which would take the whole lawn.

LIPPINCOTT: Was this one of his walls?

BALTIMORE: No, it was not a wall. It was a series of levels that came up. He looked at the site; the site actually falls, so he was going to build these various levels into the contours of the site. It was an interesting piece. It wasn't the work he was doing at the moment; it harkened back to work he had done at Storm King in northern New York State, on the Hudson. He had done this in Storm King, and it was somewhat related to what he had done at Storm King, but that was many years before. I was a little surprised by that, but that's what he wanted to do, and I couldn't convince him to do anything else.

LIPPINCOTT: Did you try?

BALTIMORE: I did, at various times, try—in particular, when it became clear that the opposition in the school to it was enormous.

LIPPINCOTT: I remember what I thought was the very funny thing the students did, with the couch and the lamp— [Laughter]

BALTIMORE: The students were actually pretty good about it, but they were also adamant and they were prepared to make a lot of trouble.

LIPPINCOTT: Well, they liked to play Frisbee on that lawn, for one thing.

BALTIMORE: That's what they said. [Laughter] I've checked. There was very little activity that had ever been on that lawn, and many people knew it. They played much more on the Court of Man than they did [in front of Beckman]. No, it was all an excuse to be Luddites about contemporary art.

LIPPINCOTT: That's so like kids, to be that way.

BALTIMORE: Right. Some of the faculty members were helpful, and some were not very helpful.

LIPPINCOTT: What about the community? Were there objections there?

BALTIMORE: No, it was all inside. So I finally decided that it was not worth the effort—the internal disruption. There were more important things to worry about than that, and so I gave up on it. Which I felt very sad about. And had I not given up on it, then they couldn't have used it for the set-up for these buildings, and we wouldn't have that eyesore, which we've had for years now, sitting on the campus.

LIPPINCOTT: You mean the construction stuff?

BALTIMORE: Yes.

LIPPINCOTT: When is that going to be over?

BALTIMORE: Well, I guess when the chemistry building is finished—probably in six months. The Annenberg building is open. I haven't been there, but—

LIPPINCOTT: That was the one that Rem Koolhaas was going to do, and then you dropped him.

BALTIMORE: Right. I mean, he just— First of all, he split off his American activities from his business and put them under the control of a man named Joshua Ramos.

LIPPINCOTT: You mean his architectural commissions in this country?

BALTIMORE: Yes, well, the office in New York, which Ramos ran, he then split off; and Ramos was going to run it. So we ended up having a contract with Ramos instead of with Koolhaas, although Koolhaas said he would continue to be interested and come by and whatever. Then, the plan for the building, which was fascinating and which I think would have made it one of the great buildings on campus, had a serious flaw, which was the planned roof of the building. Again, we had terrible trouble because the cost of building just skyrocketed in the late nineties, and so all the time we were planning these buildings, the cost of the buildings kept getting higher and higher and higher, and the amount of money we had didn't change. So we had to keep downsizing the buildings in order to make them fit within the financial constraint. That happened to the Cahill building and that happened to the Annenberg building. They said they could do that, but they had to put this stuff on the roof, as a roof, and we could not get comfort that that was going to last and that anybody really understood what that material was. They were never precise enough about it. So we finally fought and fought and fought with them, and fired them. This was Jean-Lou, finally, who made that decision. But then we had to get a new architect. They got a new architect, and they have a building there that's much more ordinary—but serves the purpose.

LIPPINCOTT: OK. To get off the buildings—I do want to talk a little bit about your getting the National Medal of Science from President Clinton in March of 2000. You went to the White House. Was that fun?

BALTIMORE: Oh, yes. That was extraordinary.

LIPPINCOTT: Who else was there? Weren't there other honorees at the time?

BALTIMORE: Oh, yes, there's a whole cadre of them, and I can't remember who all of them were; some of them I knew; most of them I didn't. It was also just at the time when the genome was going to be announced, so there was a lot of brouhaha about that.

LIPPINCOTT: Was there a connection?

BALTIMORE: No, there wasn't any connection.

LIPPINCOTT: Were you surprised to have been selected to get the medal?

BALTIMORE: I hadn't been thinking about it at all. I was completely surprised and very gratified. I didn't know I'd been nominated; I didn't know anything about it.

LIPPINCOTT: How about Clinton—did you get a chance to talk to him?

BALTIMORE: I did. And I guess I had met him once before that. I spent significant time with him; and it sort of laid the foundations for his coming out to Caltech, which he did in the last year of his presidency and gave the only speech he ever gave about science and technology [January 21, 2000].

LIPPINCOTT: Well, he was more scientifically savvy than George Bush, certainly.

BALTIMORE: Oh, certainly more than George Bush. But he wanted to have [Vice President Al] Gore as his spokesman in the areas of science and technology, so up until the time that Gore left the White House to campaign for the presidency, Clinton never really spoke about science and technology, although he had done a little bit with AIDS.

LIPPINCOTT: Who was his science adviser—do you remember?

BALTIMORE: Yes, he had two science advisers—[John H.] Gibbons in the first term and Neal Lane in the second term. And it was Neal who first contacted me and said that Clinton wanted to give this speech, and could we arrange it at Caltech; and I said yes, I could, and did.

LIPPINCOTT: And that was a national speech?

BALTIMORE: Oh, yes, that was a big deal. It was really very exciting to have him here. I still remain casual friends with him.

LIPPINCOTT: OK. In the meantime, in 2005 your lab got a huge \$14 million grant from the Bill and Melinda Gates Foundation. Your proposal was titled, “Engineering Immunity against HIV and Other Dangerous Pathogens.” That’s what you’re doing now. How’s that going, then?

BALTIMORE: That is going very well. We just had a review of that with a group of scientists who came in two weeks ago, and they were very complimentary about our progress. And the Gates Foundation—because we are coming to the end of that five years—is talking to us about continuing the work.

LIPPINCOTT: Well, that’s good. I did want to talk to you about stem cell research and gene therapy and so forth. There was an incident in 1999 where a young man [Jesse Gelsinger] died at a gene therapy trial at the University of Pennsylvania, because—I guess it was the vector, not the gene—he was allergic to it. What was your involvement in the public reaction to that?

BALTIMORE: I didn’t, in particular, have—

LIPPINCOTT: You didn’t stand up and defend gene therapy?

BALTIMORE: Well, I *had* defended gene therapy. When I discovered the reverse transcriptase in 1970, I saw immediately that it enabled gene therapy.

LIPPINCOTT: And that there would have to be human trials?

BALTIMORE: Oh, I didn’t know what the path to its becoming a therapy was going to be—the regulatory path. But conceptually, it was clearly there, and I’d been following that ever since; and much of our program in engineering immunity is basically a gene therapy program. So I’m deeply involved in gene therapy—not using the kind of vectors that [Dr. James M.] Wilson used in that Gelsinger situation, an adenovirus. We’re using a different kind of virus with its own problems, but not that.

LIPPINCOTT: Is this the kind of thing that can be tested on human beings at this point?

BALTIMORE: One of the things that we focused on in this review was, How do you now take what we've been doing in the laboratory and move it to humans?

LIPPINCOTT: You've been doing it in mice?

BALTIMORE: We've been doing work in mice entirely. Humanized mice—mice with a human immune system. And so we are now setting about that plan, and that's our focus.

LIPPINCOTT: Any particular diseases you're—?

BALTIMORE: Well, it's HIV.

LIPPINCOTT: OK. Let's see, we're coming up to the end of your presidency. And it was maybe a little bit blighted by this Luc Van Parijs guy—

BALTIMORE: Yes. You know, that's been blown out of all proportion. It really had nothing to do with anything. That was unfortunate. Luc was a postdoc with me here, did some very nice work, much of which we're building on. In fact, the whole engineering immunity program takes off from stuff that he was central in starting. He was a very good scientist.

LIPPINCOTT: What happened? He was tripped up after he left you, when he went back to MIT.

BALTIMORE: That's right. In retrospect, he had been cutting some corners along the way—either not paying attention or consciously—using figures more than once to make different points.

LIPPINCOTT: In papers. This would be old graphs?

BALTIMORE: Right. And they were slightly modified, but it was perfectly obvious it was happening and had been happening for some time. It happened when he was in my lab, and it happened before he was in my lab.

LIPPINCOTT: But you had no idea that—?

BALTIMORE: I had no idea what was going on. And what's more, when you look at the details of it, it didn't matter. It wasn't about *important* issues he was doing this. It was about *unimportant* issues—a control here... [Pause] You know, saying it that way may actually explain what was going on, which is that when he had to do things that were unimportant, he would cut corners. When he did things that were important, he didn't cut corners—as far as anybody knows, and he's not been accused of it. And what happened at MIT was that he started cutting corners about things that were important.

LIPPINCOTT: And some of his colleagues noticed it.

BALTIMORE: People in the lab noticed it. He was claiming things that had never been done, and they knew that they had never been done; and so they blew the whistle on him.

LIPPINCOTT: Well, that's a terrible shame.

BALTIMORE: Yes, it is a terrible shame.

LIPPINCOTT: Tragic. What happened to him? Is he out of science?

BALTIMORE: He's out of science—he's been working in the investment area. As I say, he's very smart. But from my point of view, it was an unfortunate but hardly serious issue.

LIPPINCOTT: But you asked that his work be investigated after it came out in the news.

BALTIMORE: Oh, right, absolutely. I mean, I had to. It had nothing to do with the news. As soon as I heard from MIT that they had strong reason to believe that he had— I was called by somebody I knew there, and he told me—he couldn't tell me the details of it, but he said there is no question that he [Van Parijs] was faking data on a significant scale. So I wanted to know if we had a problem, and I looked over the work we'd done here. But, interestingly, one of the people who is still in my lab worked very closely with him, and she said, "I never saw anything to suggest it—anything of this sort." And she actually created a lot of the figures in the papers that we published with his name.

LIPPINCOTT: One of the papers had to be corrected, isn't that right?

BALTIMORE: We've since retracted two of the papers because of the problem, but not because there was anything wrong with them—and in fact the retraction says that.

LIPPINCOTT: I understand what you're saying—it's more or less *pro forma*. A couple of nonessential figures were just not right.

BALTIMORE: Right. But what he did at MIT was egregious; I mean, he invented a whole series of experiments that had never been done, as far as I can tell. It's never been formally, publicly said what happened.

LIPPINCOTT: OK. Well, you resigned your presidency in October 2005. Now, this was a couple of years ahead of time. You would have normally gone on to 2007, isn't that right?

BALTIMORE: I had said that I would probably be here for ten years. I was feeling that I had accomplished much of what I had wanted to accomplish, and that I probably wasn't going to start any new big initiative of any sort, because I was going to wind down. At that point, I said, "Why am I going to stay on as president, in that case?" So I decided to resign after nine years rather than ten years, but you have to announce it a year beforehand to give the school a chance to find a new president.

LIPPINCOTT: So you continued.

BALTIMORE: So I continued, and I *said* I would continue, because I wasn't under any real personal pressure to do something else, and I had actually committed to this grant and other things—

LIPPINCOTT: To the Gates Foundation.

BALTIMORE: So I was going to spend more time in my lab, but I could continue to be president until they had a new president, and I did that. It's a very funny time. Because you have to announce much of a year ahead of time, every president ends up with this one year of limbo.

LIPPINCOTT: You felt lame-duckish.

BALTIMORE: Lame-duckish, but since I had other things to do, it didn't bother me. It's not as though I couldn't find anything to do with my time.

LIPPINCOTT: Yes. You're happy now in your lab, I guess.

BALTIMORE: I am.

LIPPINCOTT: OK. Just to wind up, I'd like you to comment on what you think the prospects are for our participation in our own evolution. Do you have any ideas about that? Given our expertise in moving genes around and fooling with them.

BALTIMORE: We have put a pretty strong firewall in place around germ-line genetic engineering—actually changing our inheritance. I think the scientific community is in agreement that we don't want to do that, that we don't feel like we're smart enough to do that.

LIPPINCOTT: I think they call it human enhancement therapy.

BALTIMORE: No, you've got to understand the difference between two things: germ-line genetic manipulation and somatic genetic manipulation. Somatic genetic manipulation is about you as an individual. We can change your genes in particular cells, in many cells, in a few cells, and we can have a therapeutic outcome which could be beneficial to you. But you won't pass that on to your children, because it doesn't change the germ line, it only changes somatic tissue. So we make a distinction between those two, and really nobody has proposed even—except the Raëlians, who are crazy people—

LIPPINCOTT: Who are Raëlians?

BALTIMORE: Oh, don't worry about them, they're crazy. It's a religious cult. [Pause] But no reputable scientists would want to change genetic inheritance. And we've said we're even happy to accept a congressional ban on it, if Congress feels it has to do that. Because what it does is enable us to do other things—other kinds of genetic manipulations that don't involve the germ line, and then the public can feel comforted, if they can, that we're not going to start breeding genetic monsters that breed true in any way.

LIPPINCOTT: I mentioned to you before that when you were interviewed by Charlie Rose in 2002, he asked you what subject you were most curious about, and you said, "Consciousness"—the mind-body problem—which there hasn't been a lot of work done at Caltech on.

BALTIMORE: Oh, well, Christof Koch [Troendle Professor of Cognitive and Behavioral Biology and professor of computation and neural systems]—

LIPPINCOTT: Except for Koch, yes. But that's pretty much it, isn't it?

BALTIMORE: No, actually David Anderson [Benzer Professor of Biology] has worked with him some and is trying to define how you would get some sort of a readout in rodents and mice so that you could actually do experiments. And I think everybody in neuroscience is aware of the problem and would love to contribute to it, if they found themselves able. And someday, somebody's going to be able. And it's going to come out of the blue, in all likelihood. When you say, "In 1945, the end of World War II, what were the great outstanding problems in biology?"—the great outstanding problems were how genes work and how development occurs. Those are the two big issues in biology. Watson and Crick set us on the path to understanding the first, and various people—Jacob and Monod, and a few others—set us on the path to understanding development, which we're still on. That one's harder, because it's much more complex—the genetic problem is DNA. And the third problem you would have identified at that time would have been consciousness. And consciousness remains as the outstanding problem on which we have *no* demonstrable progress, because it involves understanding the highest levels of brain function and we're still working on the lower levels.

LIPPINCOTT: It's how to get something abstract out of something physical.

BALTIMORE: Well, not just abstract; it's compelling to the individual. It's your inner life, as an individual, and you can actually demonstrate that it has tremendous power of decision making and representation. But what *is* it?

LIPPINCOTT: What is it? [Laughter] Well, it bleeds over into philosophy. You could have what they call a zombie, someone who you wouldn't be able to tell was a real person or not. But a zombie has no self awareness. That's the most significant—

BALTIMORE: Yes, the zombie state—things that happen without self awareness. How you drive when you're talking on a cell phone. [Laughter]

LIPPINCOTT: Well, thanks very much. It's been a terrific interview. Do you have anything you'd like to add about Caltech? You're still happy here; you intend to stay here for a while?

BALTIMORE: I'm still happy here; I intend to stay here for a while.

LIPPINCOTT: Glad to hear it. [Recording ends]