INTERVIEWER: This is the 150th anniversary interview with David Baltimore. Let me start by asking, where you were born, and where did you grow up?

BALTIMORE: I was born in New York City. I grew up largely in Great Neck, New York. I went to Great Neck High School, and Swarthmore College.

INTERVIEWER: Tell me about your family.

BALTIMORE: My family were sort of middle class people, coming from an actually quite poor background in New York. My father never went to college. My mother went to college, and graduated from NYU. She was a great scholar in her time-- a great student-- and went back to school. She graduated about 1930, and went back to school in 1943, about-- that's when my brother was born. She became an experimental psychologist. She taught at The New School for Social Research in New York, became a tenured professor at Sarah Lawrence at age 62.

INTERVIEWER: Good for her. Were there any particular influences or events in your childhood that you think helped to shape your career path?

BALTIMORE: Absolutely. In my junior and senior years of high school, my mother had told me about and I applied for and was accepted at a program at the Jackson Memorial Laboratory in Bar Harbor, Maine. High school students came and learned a little about research and genetics, and lived together as this group of 27 students or something. I discovered research science that summer, and I never looked back.

INTERVIEWER: Had you been interested in the sciences up to that point?

BALTIMORE: Well, I was a good science and math student. My mother recognized that. I didn't have any passionate interest in a particular kind of science. I had this sense of accomplishment of having really positive feedback from doing very well on the courses, and understanding it very easily, but this summer just transformed me.

INTERVIEWER: What do you think it was about research that kind of flipped that switch?

BALTIMORE: I think it was the ability to discover things that nobody else knew. It's easy, actually, to do that. When the faculty there presented me with certain opportunities to study this, that, or the other thing, and I did that, and I found an answer-- it wasn't terribly important, I can't even tell you much about it now, but it was something that no one else knew-- that just captured me as a way of life, and I've been doing it ever since.

INTERVIEWER: What made you decide to go to Swarthmore?

BALTIMORE: A mixture of things. The connection to the school of psychology, to my mother. I was involved in the Gestalt School, which was centered at Swarthmore and the New School-- this school in exile, from Germany. She introduced me to faculty there, and I knew them. I had also, at that summer program, met a number of people who were going to Swarthmore, one of whom I later shared the Nobel Prize with, Howard Temin.

INTERVIEWER: You were fated to cross paths.

BALTIMORE: We seem to have been fated.

INTERVIEWER: You started out majoring in biology and then you switched to chemistry. Can you tell me a little bit about why that happened?

BALTIMORE: I switched to chemistry for logistical reasons. First of all, I had a chemistry minor anyway, and enjoyed chemistry. The chemistry department at Swarthmore had some younger faculty and faculty who really understood the desire to do research, whereas the biological faculty was-- somebody recently said Swarthmore at that time had a great 19th century biology department-- much more observational and interested in embryological problems and developmental problems, but not in molecular biology or biochemistry. That's what I cared about, so I found my soul mates in chemistry, and they said, why don't you switch in the chemistry, and then you can do an experimental thesis. It's all about doing experiments, and I did an experimental thesis at the University of Pennsylvania, actually, from the connection which they set up for me.

INTERVIEWER: What was the nature of that?

BALTIMORE: I was studying the equilibrium of ATP hydrolysis to either AMP or to ADP. The issue was, what was the energy differential between those two reactions?

INTERVIEWER: OK. Not surprisingly, that doesn't mean a lot to me, but --.

BALTIMORE: You asked.

INTERVIEWER: Yes, I did. Did you have, in college, influential mentors?

BALTIMORE: Well, the professor of chemistry, Gilbert Haight was a wonderful mentor, and Pete Thompson, a younger professor of chemistry was, but aside from that, not really.

INTERVIEWER: OK. George Streisinger at all?

BALTIMORE: Well, he wasn't at Swarthmore.

INTERVIEWER: Oh, he was later.

BALTIMORE: He was at Cold Spring Harbor. Between my junior and senior years of college, I went to Cold Spring Harbor, and that played another central role in my life, partly because of George, whom I worked with that summer, and just from the exposure to the greatest biologists living, who all came to Cold Spring Harbor in the summer.

INTERVIEWER: Tell me a little bit more about what you did there.

BALTIMORE: I was part of what was called the Undergraduate Research Participation Program-- URPP-- and this was the first class of URPPs at Cold Spring Harbor. It was funded by the National Science Foundation. I grew up, as I said, in Great Neck. Great Neck is not far from Cold Spring Harbor, and so one April vacation where I was home, I went out to Cold Spring Harbor, and made contact with a couple of faculty there whose work I'd read about. I wanted to ask them to come to Swarthmore as speakers, and I wanted to get some materials that we could play with in the laboratory at Swarthmore.

One of those turned out to be George Streisinger. I had not heard of him before, but somebody directed me to him. George said, well, if you'd like to, you could come here for the summer with as an URPP. I said, I would like to-- and I had some other plans for the summer, and when they heard that I had the opportunity to go to Cold Spring Harbor, they said go-- so I went, and it was an incredible summer. It was just because Cold Spring Harbor was the center of this very nascent field in molecular biology. We're now in 1959.

INTERVIEWER: You seem to wind up-- your timing's good.

BALTIMORE: My timing's good? I don't know. I lived through the right times.

INTERVIEWER: So what made you think about MIT for graduate school?

BALTIMORE: In that summer, two people: Salvador Luria and Cy Levinthal, both relatively new faculty at MIT, were-- as I said last night-- trolling for graduate students to start a program of graduate education in molecular biology. They saw me-- maybe they talked to George, I don't know-- and said, would you like to come to MIT? Basically, I didn't apply anywhere else. I just came to MIT, because I'd been invited.

There weren't many places to go to get an education molecular biology in 1960. You could go to Rockefeller, which is where I ended up, but didn't really want to go directly there. You could go to Caltech, but I was an eastern person, and the thought of going to California was-- I mean, I might as well go to Japan. I just had never been there. My parents had never been there. It wasn't part of a world I knew anything about. I probably would have enjoyed going to Caltech, actually, and that's where Howard Temin had gone. Air travel wasn't so easy then. When you went to California, you were in California. What do I know from palm trees?

INTERVIEWER: So, just around the time jets were starting.

BALTIMORE: Yeah.

INTERVIEWER: Do you remember when you got to MIT, what your first impressions were?

BALTIMORE: What were my first impressions? Well, I was only here for a year as a graduate student, and then I went to Rockefeller. During that year, a number of the faculty were really very helpful to me in thinking through what I wanted to focus my attention on. I decided what I wanted to focus on was animal viruses.

I went to Cy Levinthal and said, I'm thinking that maybe animal viruses could be a probe of animals cells, the way bacterial viruses had already been a probe of bacterial physiology and molecular biology. He said he thought that was an interesting idea. He'd been thinking about that, but he had no idea what to do about it. I went to Salvador Luria with the same question, and Luria, who had written about animal viruses in his textbook in virology, and was one of the very few people in the phage world who understood that there were other kinds viruses, and that they could be useful to think about, he set me up to work the following summer-- it's always in the summer when things happen-- at Albert Einstein with a man named Phil Marcus and to take the animal virus course at Cold Spring Harbor.

I went back out to Cold Spring Harbor in August of 1961-- no, 1960. Yes. No-- 1961. It has to be 1961. I took the animal virus course, and there I met a guy whom I decided I wanted to work with-- Richard Franklin-- and he was a professor at Rockefeller, so I scrambled to get admitted to the Rockefeller program. Luckily, there was a guy who had dropped out of the program, so they had one slot open, because it was a very small program-- it was all onesies-- but this guy decided he wanted to play cello, as I remember, and not do biology.

There was a slot, and Luria helped me get into it. It was just the most wonderful thing that he could have done, because he had worked hard to get me to MIT, and it was being very successful, but he recognized that I had found what I wanted to do. So I went to Rockefeller, and I did my thesis, actually, in two years at Rockefeller. I walked into the most incredible scientific opportunities.

INTERVIEWER: Was the Rockefeller Institute different? Did it have a different feel than MIT?

BALTIMORE: Oh, totally different. MIT is a big teaching institution; Rockefeller is a research institution that had a few boutique graduate students, and we were just delightfully pampered. We had the opportunity live in the middle of New York. By that time, my parents lived in Manhattan, so it was great. I was sort of cross down from them.

INTERVIEWER: So let me go back to MIT. In the year that you were there, were you able to sort of characterize the culture or the students, or the faculty in any way? Did you have any impressions of what it was like as an institution back then?

BALTIMORE: Well, you know, as a first year graduate student, you don't really know a whole lot about the institution. MIT was very siloed, so I knew about biology and about the biology people. I didn't even know much about chemistry, and certainly didn't know much about what other things that went on around, although sometimes I would wander the halls and find people playing computer games. That was amazing to me, because I didn't know anything about either computers or games, or that kind of game. At two in the morning, they'd be playing these games in the halls of the physics department.

INTERVIEWER: You came back to--

BALTIMORE: Let me say that MIT at that time was just on a cusp of going from a school that had no real biology-- they had sanitary engineering, it had physical developments like the electron microscope, and it had some neurobiology. Frank Schmidt had done that, but it had-- over the period from about 1955 to 1960-- brought in faculty in biochemistry, biophysics, molecular biology, and was really shaping itself for the future. It was terrific.

Alex Rich, who's still here, Vernon Ingram, whom I think has passed away, Salvador Luria, who has passed away, and others-- were really creating a unique department here. It was unique in the sense that it didn't have the antecedents of zoology and botany that so many other schools had, so it didn't have that baggage to worry about. On the other hand, it also had absolutely no organismal material to work with, so it was all biophysics, biochemistry, and molecular biology. Molecular biology is hard to define, but we'll just leave that alone.

The faculty was very interested in the development of the students, because the students were all people who were pioneers. There wasn't an established curriculum. Nobody even knew what they oughta do. I ended up a biophysics major, just because that seemed like stuff I ought to know. I passed my prelims in the first year-- I mean, the year that I was here-- in biophysics. That was just convenience, I guess.

INTERVIEWER: This might be a good time for you talk a little bit about where the first Nobel Prizes at MIT were.

BALTIMORE: The first Nobel Prizes at MIT received were in economics and in biology. I think the literal first may have been in economics-- Samuelson, Modigliani, and Solo all won the prize-- and in biology, Luria. Well, I won it, but that gets us to a later time.

I don't remember exactly when Luria's prize was-- probably in the 1970s-- and his was the first, but there were no other Nobel Prizes at MIT. There were no prizes in physics or chemistry-- things that you might have thought would have been the first places to be recognized-- because MIT really was set up as an engineering school. Everything else was sort of ancillary to engineering until after World War II, when there started to be a real thrust in basic science-- as near as I can tell it, anyway-- at MIT.

I have a real mirror to that, which is Caltech. Caltech was set up to be a school, where it was also an engineering school, but the engineering was set up to be a derivative from the science, and so Hale, who really just really set the template for Caltech, said we have to have strong physics, strong mathematics, strong biology, even-- strong chemistry, and strong biology-- from the very beginning, and that they are going to feed into the engineering disciplines, rather than the other where around. Caltech was never a sort of building a bridge engineering school, but Caltech started winning Nobel Prizes in the 1930s.

I think the first prize was about 1930-- well, it was early in the 1930s-- and Morgan, who won the Nobel Prize in biology for his work in genetics, was then at Caltech. Carl Anderson was the first one to win a prize for work actually done at Caltech, and that was in the 1930s. It was really a very different mindset between MIT and Caltech. Hale, I might say, came from Caltech-- came from MIT and got his degree here-- and was once invited back to be president and said no, I want to be in Pasadena. I want to build a school on somewhat different principles here and then stole a chemistry professor from MIT, Noyes.

INTERVIEWER: After you finished your doctorate, you came back to MIT.

BALTIMORE: That's right.

INTERVIEWER: Tell me a little bit about that.

BALTIMORE: So as I said, I did my thesis in two years. Rockefeller said, we have a three year residence requirement, but there's clearly nothing else you need to do. Anyway, my professor had left Rockefeller to go to Colorado, where he got a nice job. They said, why don't you just be on leave.

Rockefeller was really very focused on its students, and they had a program where if you wanted to go somewhere for a year, you could do that, so I came back to MIT. Meanwhile, MIT had hired Jim Darnell, and Darnell was probably the most forward looking person working with animals viruses in the United States. I wanted to learn the technology that he was applying to these problems. I wanted to work with him, and with the whole atmosphere he had created here, which was really very special.

So I came for a year. Actually, I just came as a postdoc. I'd planned to spend a usual couple of years as a postdoc. I hadn't really made a scheme of it.

Jim left after I was here a year, and went to Albert Einstein, and so my reason for being here dissolved. I only spent that year between-- 1963, 1964-- here, and then I also went to Albert Einstein, but not with Jim. I went with to work with a biochemist, Jerry Hurwitz, because I really felt I needed some rigorous training in biochemistry. I did, and I probably never would have won the Nobel Prize without it.

INTERVIEWER: After that, you wound up going to the Salk Institute?

BALTIMORE: I did. I was at Einstein, and Renato Dulbecco came through. He was giving some lectures, I think, at Rockefeller, actually. He sort of knew what I had been doing in animal viruses. By this time, I was fairly widely published in the field, and my mentor wasn't any better known than I was. We were seen as sort of two young Turks. He called me, and he said, we're going to set up-- he was at Caltech at the time-- I have made a commitment to go to the new Salk Institute, which is going to be formed in La Jolla, and I would love to have you come as a junior faculty member in my unit.

It wasn't really departments in the usual sense, but he had space. He could do anything he wanted with it, and he was offering me some of that space to do my programming and working in RNA viruses. Then ensued this wonderful uncertainty about whether the Salk Institute was actually going to exist or not.

I would get these notes from Dulbecco, or calls from Dulbecco saying, don't make any plans, because I'm not positive it's going to happen. We're having a meeting next week, and I'll get back to you. So, the meeting occurs next week, and he gets back to me, and says looks like it's going to happen. Start making plans. A couple weeks later-- grr, we're running into trouble. This went back and forth, until finally he said, yes, it's going to happen. I got in the car, and came out to California. By that time, I had once before been to California. I had flown out there to give a lecture at Caltech.

INTERVIEWER: Not so much of a culture shock.

BALTIMORE: Well, it was a culture shock. It was enormous culture shock, particularly in this sort of unformed world of La Jolla at the time. They were just starting up the university and the Salk Institute was brand new. We were working in these temporary buildings. When I arrived at the Salk Institute, they had to build labs for me in the basement of this temporary building while the main buildings was going up. They had to take a rattlesnake out of the lab in order to get me in there. This was the Wild West.

INTERVIEWER: You were there for three years, and then you wound up coming back to MIT. What was the draw?

BALTIMORE: The MIT yo-yo. The draw was that I loved MIT. I had loved my two years here as a graduate student and postdoc, and it was all about Salvador Luria. While I was at Salk-- and I'd picked up virus work again after my postdoc, and I was publishing some interesting stuff-- a professor at Harvard became aware of it, Bernie Davis. Bernie said, would you come and look at a job at Harvard. I said, I'll come and give a seminar, and so I did.

Luria heard about it, and Luria than engineered an offer to me on the basis of what people knew about me-- I think I came and gave a little seminar-- because he didn't want me to go Harvard. He said, if you're going to go anywhere, you should come to MIT. In the end, Bernie Davis didn't offer me a job, because I didn't show enough enthusiasm for teaching to satisfy him. It's an interesting story.

So I had this offer from MIT. It's a sort a long story, what made me make the decision, but anyway, I decided I want to leave Salk. I pulled this offer out of my drawer, and called up Salvo, and said, I'm coming. In those days, we didn't negotiate for anything-- just kind of assumed. When I came back as a postdoc, I was actually supported on a training grant of Luria's. Luria just watched over me the whole time, and so I felt very comfortable that I was coming back to a situation where I'll be treated well, and I'll get what I need to do what I want to do.

INTERVIEWER: The importance of mentors.

BALTIMORE: I said last night in the tribute to Luria that he's my father in some very real ways, and I try to be a dutiful son.

INTERVIEWER: Can you walk me through in a kind of summary fashion your research after you got here?

BALTIMORE: Yes, I can. I came here having started as a graduate student working on-- we'll call it polio virus. I was actually working on a relative of polio, but then I switched to working with polio itself. It's an RNA virus. It has a very particular biology because it is an RNA virus, and there was very, very little known about the biochemistry of our RNA virus infections. That's why I say I had walked into a totally open situation. I had made a lot of discoveries about it, and there were all lots of fascinating things about it.

When I arrived back here, it was to continue to work on polio virus. I set up the lab to do that, and I got graduate students and postdocs. Things were going very well, and one of the people in my lab I married-- Alice Huang-- and she had actually come to my lab first at the Salk Institute, and we had come east together. It was, by that time, no surprise that we got married. She had, for her thesis, worked on a very different kind of RNA virus called the vesicular stomatitis virus. She'd made very important discoveries about it, and then had originally come to work with me in order to develop her skills in various new approaches to viruses that we were using on polio.

So she was working on polio, and that was the only object of focus of my lab. She said one day, you know it might be fun to pick up VSV again, this virus that she worked on, and by this time, I had been working on polio for much of 10 years, and so the idea of doing something new was a good idea. Anyway, I had various interests in viruses, and so I could have a look at a new virus, and see how it's how related to the virus I'd worked on.

We started growing it, and had a graduate student start working on it. Alice went back to working on it a little bit, and I did some work, because at that point, I worked in the lab. We made some really remarkable discoveries about VSV. It was fundamentally different than polio in all of its strategies, and I could describe why, but that's another story.

The last experiment I did with it was to show that it contained a polymerase in the virus particle that copied the genome into complimentary RNA. It was very different than polio, which encoded a polymerase that was made in the infected cells, but didn't carry it in with it. That experiment led me to think, maybe there are a lot of other viruses that have polymerases in their virion, because that would provide a unifying notion of how these viruses work.

We started gathering viruses from various places, and in fact, it turned out to be true that a lot of other viruses had polymerases, and we published a number of papers on it, and worked on it. By that time, we could pretty well characterize most viruses in terms of their overall strategy, except for the RNA tumor viruses which caused cancer as their primary interest. That had a totally obscure molecular biology-- what polymerases they used, how they dealt with information, all of that was totally unclear.

Howard Temin had for 10 years been saying that the RNA viruses-- they pretty well have to copy their RNA into DNA to make any sense out of the way they transform cells, and the way they cause cancer. He couldn't prove that, and really almost nobody believed him until about the last year before I got involved with it where there started to be some glimmers of suggestion that maybe he was right. I did with RNA tumor viruses what I had done with many other viruses, which was to look in the virus particle for polymerase, but in this case, because of the possibility that it might copy into DNA, we looked for an enzyme that would copy RNA into DNA.

Such an enzyme never been seen before, and its existence would have and did violate the central dogma of molecular biology as enunciated by Crick, which was that DNA made RNA made proteins. Here we're going to say that there's information flow back. Now, when you think about it from a chemical point of view, that was not a big deal. If RNA made DNA, and DNA made RNA, it was all the same sort of polymerisation reaction, and the same basic biochemistry, but it had never been seen and nobody knew it could happen.

From a strategic point of view, in terms of the strategies of information flow in the system, it had enormous implications. I looked for it, and it was a two-day experiment. I showed it was there. I stopped work for awhile to protest against the Cambodia Invasion, got back in the lab, finished the experiments, and published them. That was June 1970. Howard Temin, turns out, had a postdoc with Zutani, who at the same time, did similar experiments. We published back to back, and five years later, we received the Nobel Prize for it.

INTERVIEWER: Did the Nobel Prize-- was it a surprise to you?

BALTIMORE: It was a total surprise. I was actually on sabbatical in New York when it was announced. I got on the first shuttle flight that I could find, and flew up to Boston to celebrate. My wife wasn't even around. She had called me. She told me I won the prize, because she was a scientific meeting in Scandinavia, and had heard about it a little before it was formally announced.

I didn't even know really when the prizes were announced. I wasn't aware of them. People had said, you're going to win a Nobel Prize. I figured that someday it'll happen when I'm 60, right?

INTERVIEWER: Did it turn out to be difficult to get it so young?

BALTIMORE: Not difficult-- no. It was perfectly easy. I went to Stockholm, and were anointed.

INTERVIEWER: Did it make a difference in your work?

BALTIMORE: It certainly made a difference in my life in the sense that it gave me a public-- I mean, not real public, but scientific public notoriety that led people to invite me to all sorts of fancy venues to want to hear me pontificate on things I didn't know anything about.

It was a distraction to some extent. It was an opportunity from other points of view, and it was a responsibility because there are very few Nobel laureates. We are seen as a sort of embodiment of the science in which we're involved. We become the spokesman for all those thousands of other people who don't have Nobel Prizes, but who are working toward exactly the same aims that I'm working towards in terms of development and knowledge about biological systems. So that's a responsibility.

INTERVIEWER: I would imagine it makes it easier to get funding for things.

BALTIMORE: Everybody imagines it would make it easier to get funding, and it's not really true. In fact, the panels that review you are as likely to turn down a Nobel laureate as anybody else if they don't think you're making sense. I think there's always a suspicion, in fact, that you're trying to coast on your laurels, rather than getting down there in the trenches with everybody else. No, I don't think it's that.

From other points of view, particularly if you're getting money, not from the Federal government through the peer review system, but through other venues-- charitable organizations, things like that-- then having a Nobel Prize gives you a visibility. They want you in their stable, because then they can say we're funding 20 Nobel laureates or whatever. There is, from those points of view, an opportunity.

I don't know whether I ever would have been introduced to Jack Whitehead-- which is now a story coming up here--

INTERVIEWER: My next one.

BALTIMORE: --if I didn't have a Nobel Prize. I just don't know, actually, literally, how much of a role that played, but it certainly made me a more visible person to him and to other people. Yeah, there are a lot of pluses.

INTERVIEWER: So let's talk about the Whitehead Institute, since you were so involved in its creation. I'm interested in how it came about, and about the objections that existed at the very beginning.

BALTIMORE: In 1980-- so I won the prize in 1975-- I came back to MIT, and actually switched my research focus from viruses to immunology, and then carried actually both interests in parallel for many years thereafter. I had become interested in cellular systems and in the development of the immune system, which is just one of the most wonderful puzzles in biology. It was a problem that I had an interest in from the time I'd been at the Salk Institute-- so for maybe 15 years, over 10 years-- but I hadn't done anything about it, because you couldn't work on it from a molecular point of view. You could work on it from various other points of view.

Susumu Tonegawa then did his great experiment, which opened up the molecular biology of the immune system. I wanted to get into that business, and I had this itch I hadn't scratched for all those years. Between 1975 and 1980, I developed that, and then carried it forward. Many of our discoveries beyond the Nobel Prize were in immunology.

So in 1980, I got a call from Joshua Lederberg. Josh was then president of Rockefeller University, and he was an adviser to Jack Whitehead. Jack had a series of advisors-- wise people. He said, do you know about Mr. Whitehead? I said, well I did know a little bit, because he had once come through Cambridge, and had thought about setting up his institute in Cambridge, but had decided not to originally. That was roughly around 1975.

He developed the Technicon Corporation, which sold all of the clinical analysis equipment in the world at that time. They developed these huge machines that could in power look at a whole range of parameters very quantitatively. He sold that to-- well, at the time when I met him first, that is, when he came through here, he hadn't yet sold it, but it had gone public. It made him a billionaire, when a billionaire was a very unusual thing.

He had made a commitment to spend $135 million to build an institute, which was his form of giving back to the world from which he felt he had derived his fortune. He was doing it in a very tax efficient way that was going to allow him to get all sorts of benefits from it. It was a mixed set of motives that he had. He finally said he was going to do this at Duke University in somewhere around 1975, and he set it up at Duke, but he was meddling too much. Duke had bought a bit of a pig in a poke, because of all this money involved and they finally splintered, came asunder. He sort of bought them off and gave them a fair amount of money, and closed down that chapter.

By this time, he had sold the company and had no longer these complicated tax reasons to do this. The idea had captured him of building an institute, and he'd been convinced by his advisors-- Lederberg and others-- that he should do this in fundamental biology, not in medically oriented biology, which was the original idea, and where he had gone to Duke because of its medical school. He was looking around for somebody to be the director and to build this thing, and to create it.

I got this call from Josh, would you come down and talk to him? I came down-- it was in August of 1980, as I remember-- and he said, if you were going to build an institute like this, or how would you advise me to focus an institute like this? Jack later told me that my answer to that question was the first time that he'd asked that question and gotten an answer which was not about the individual's own research. I didn't say I would build an immunology institute, or virology institute-- I looked at the currents in biology of the time, and said I'd build an institute focused on developmental biology.

Developmental biology was then in a sort of on a take off platform of understanding. There was lots to be done, lots of good people, and lots of good trainees. This was something you could really do, and Jack was very impressed that I didn't ask him for the money so that I could do my own work, that I was really of looking at it from a sort of global perspective. I got a call back from him not long after that interview, and he said would you consider being director?

I spent a little while thinking about that-- talking to the family, and whatever else, and said, I was willing to sign on as an adviser to him. I would then create-- or try to find the conditions to create-- the institute, and if I could create it in a form in which I was excited by it, then I would be director. Behind that-- and Jack recognized that-- was a view that MIT was the place for this. He had not thought of MIT. He was still focused either in the Ivy League or schools that had medical schools: Stanford, although Rockefeller was on his list because Josh was there.

He said, you have to do a serious consideration of the venue for it, and I said I would. I talked to people at Stanford , I talked to people at Harvard, I talked to people at Rockefeller, and elsewhere. It became clear that first of all, I could do it at MIT, but it was going to take a lot of political work, because it was some just totally brand new concept. I'll tell you about the concept in a minute.

Perhaps we could do it at Stanford, because Don Kennedy was then president of Stanford, and saw the attraction, and actually knew Jack. They probably couldn't do it at Rockefeller, because the faculty at Rockefeller would never do anything new. You could do it at Harvard Medical School as easily as anywhere else in the world because Harvard was already an institution, the medical school in particular, built out of various independent elements: Mass General, Brigham and Women's, Dana-Farber, and so another institution-- they were all set up for. MIT was not.

The ground rules of this from Jack's side were this has to be an independent institution, with its own board of directors, and its own finances. What I said was, fine, but it has to be allied with a major center, and Jack already knew that because he knew that $135 million-- $35 million for the building, $100 million endowment-- was simply not enough to build a totally free standing institution. I then went into this negotiation face-- I said to Jack, I think it MIT is the right answer. He said, well, you started off feeling that way, so it's not a surprise you came up with that answer. I admitted that I had a predilection to MIT, because I felt that I knew how MIT could provide quality control for the institution.

Quality control was the most important thing that you would get by alliance with a major institution, and that was easy to see. We'd make all of the faculty members of the biology department, and then they'd have to fit the same criteria as any other member of the biology department. One thing MIT knew how to do very well was to find the very best people in biology and bring them to the institution. That made the Whitehead Institute a sort of arm of the larger biology effort at MIT, and that was great.

We'd done that with the cancer center, but the cancer center was an independent institution. It didn't have its own board or finances. It was a center of MIT. Here we were trying to do something new. That ran into a buzz saw, and it was made worse by the fact that it was played up by *The Boston Globe* as a takeover of MIT biology by this financier Whitehead.

There were also some political currents. There was, in particular, a very strong socialist oriented faculty member in political science-- whose name I now forget-- who said this was a corporate takeover of MIT. It became an embarrassment in newspapers. It became a cause celebre on campus, and we were trying to take it through the Faculty, and get a Faculty vote to agree to this unique situation for MIT.

MIT had never formally affiliated with another organization before. MIT had different schools: School of Science, School of Engineering, whatever, but that's as far as it went. Everything was part of one faculty, and they felt that I was going to get too much power as director of dictating who was getting hired at MIT. Of course, I'd set this up so that, in fact, the director wouldn't have that power because the biology department would be centrally involved in every appointment, but people were worried. New at universities is often very hard to sell.

We had a stormy period of a year of negotiation, and finally a vote in the Faculty. The Faculty finally voted in favor of it.

INTERVIEWER: What do you think convinced them?

BALTIMORE: I think, ultimately, MIT is a risk taking institution. It takes a long time to get to that point, and they had to be convinced that we thought through every question, but there was no question that anybody could ask in the faculty meeting, for instance, that we couldn't answer and that we hadn't thought, or that we didn't have an answer to. It was a very important time, because initially when I was putting this thing together, I couldn't have answered a lot of questions. By the time it came to that vote, every question had come up and been debated in the biology department. I'd had some real skeptics in biology, and had to convince them. Finally, Boris Magasanik was the most important skeptic who turned around and said, I'm convinced that we've put into place all of the things that will make this a success for MIT and for itself.

Howard Johnson played a very important role. Howard was, at that point, chairman of the board. He said-- from the first time I met him and described it-- we have to do this. He helped me politically, and was terrific about it. Paul Gray who was president, was a little uncertain, because he was being torn from various sides by various factions. I can understand more.

INTERVIEWER: So from your years it as director of the Whitehead Institute, what accomplishments are you most proud of?

BALTIMORE: I'm most proud of the Whitehead Institute as an institution. Within a year after we opened its doors, it was known as one of the premier biological research venues in the world, because we'd aggregated this remarkable group of people. Their impact was instantaneous: Rudy Jaenisch, Gerry Fink, Harvey Lodish, Bob Weinberg.

In terms of programs, I'm most proud of the Fellows program. I said very early that one of the things I want to see us do was to find another track for the development of young people. The standard track is you go to graduate school, you do a postdoc, and you get a faculty position. There's no place in there for somebody who has the strength of independent thinking to develop his or her own program at a young age before taking on the full trappings of faculty teaching and committees and all that, and so I set up that track as a fellow of the Institute.

We've had seven slots for that. Some of the very best people in America today-- in the world, and in biological sciences-- came through as fellows. It turned out to be a proving ground for young faculty at Whitehead, too. I didn't want it to be that; we, in fact, said we were not going to hire these people, but then they were so good, and so wonderful, that we couldn't not hire them. We hired David Page, who is now the director of Whitehead, and Peter Kim, who's the research director at Merck Corporation, and lots of others.

INTERVIEWER: No slouches.

BALTIMORE: We had a slouch or two.

INTERVIEWER: Why don't we talk first about the cancer center. Why don't you talk a little bit about your involvement and its importance.

BALTIMORE: This, again, takes us back to Salvador Luria. Luria, in 1970, 1971, realized that there was going to be a National Cancer Act, and that that was going to provide money for building cancer centers. He came to me, and I was the only one working on cancer at MIT at the time-- at least the only one I know about. Actually, there were probably some people in food science, but they weren't part of the effort. He said, are you up for developing a cancer center here, and it was very exciting to me, because it would really increase my capabilities as well as providing a group of interesting colleagues to work with.

He went to Jerry Wiesner, who was president, and said, I think we should develop this center. Jerry was just terrific and very supportive. He understood the trends in biology and he was always near biology. There was Rosenblith, who was provost at the time, and who worked in biology.

We got the blessings of the administration, and went forward. This was the only designated specialized cancer center in the country that wasn't connected to a medical school, but was just free-standing, researching the fundamentals of cancer. Arguably, we have made more contributions to modern cancer understanding than any other institution by keeping our focus on basic issues. Of course, this has ballooned out into the Koch Institute, which brings together cancer research scientists and engineers in the same brand new building that will be occupied a little later this year.

INTERVIEWER: Why don't you rattle off a couple of the contributions?

BALTIMORE: Probably the major contributions were Phil Sharp's discovery of splicing, which without that, we would not understand how mamallian cells process their information. It was as fundamental a discovery as anything made ever in molecular biology. Bob Weinberg's discovery that cancer was a genetic disease, fundamentally. He showed that when you transform a cell with a chemical, that the chemical actually hits an oncogene.

We knew about oncogenes from working on viruses, but we knew nothing about them in cells, a discovery for which I still believe he should have won the Nobel Prize, but he's won everything else but. I think the work that we did on Abelson virus, which set the basis for making Gleevec, taught us a lot about the development of the immune system at the same time was important.

There was David Housman and his work in genetics, Nancy Hopkins, her work in [? resilient ?] viruses, and then finally in zebrafish. We were the fifth floor of the cancer center, the group that I've just told you about: Weinberg, Sharp, me, Nancy, and David Housman. It's not bad. There was cell biology downstairs: Hines and Solomon, and immunology-- Herman Eisen came here from Wash U and set up a fabulous immunology programs. Susumu Tonegawa, who did such important work in immunology, didn't actually do his Nobel winning experiment here, but he came here and developed that, and then moved himself into neurobiology, where he's been so important.

INTERVIEWER: I don't know if it had anything to do with being director of Whitehead Institute, but you developed an interest in administration and wound up going to Rockefeller as president. Can you talk a little about the appeal of administration?

BALTIMORE: I got just a lot of positive personal feedback from developing the Whitehead Institute. It was a lot of work, and it's a lot of detail, but it's mind expanding to begin to understand the full range of an institution and all of the kinds of problems that go into maintaining it-- the requirements for excellence and what that takes, both financially, and in terms of people. I felt I had these capabilities, and that in a sense I owed it to the biological research world to exercise those skills that I had developed.

There is a thrill about shaping an institution and developing its excellence or maintaining its excellence. When I was approached literally by David Rockefeller to head the Rockefeller Institute, now Rockefeller University. I had graduated from there, and so I had a feeling for the institution, and I thought the institution could be doing a lot more than it was doing, and there was an opportunity for growth and reshaping there. I accepted the job.

I love New York. I always did love New York, so going to New York, to me, was a thrill and for my wife. Our kid had just gone off to Andover, and so whether we were in New York or Boston didn't matter a whole lot, and she actually ended up becoming a denizen of New York. She still lives there.

INTERVIEWER: So you didn't mind spending less time in the lab at that point?

BALTIMORE: I didn't. I moved my lab to Rockefeller in stages, so I kept it here for a while, then moved it down. I had some very good people. I got a few more really terrific people at Rockefeller, and I learned how to interact with a lab on an intermittent basis and still continue to be an effective leader, but spend about a lot less time at it.

The funny thing about developing young talent-- which is what a laboratory is about-- is what I call benign neglect is about as good a way of training people as any, because then they have to make their own decisions and live with them. They think them through, and that makes them very powerful people when they leave the lab, because they haven't been coddled along the way. They've had to act as independent scientists through that training period with me, anyway. I think it's one of the reasons why the people who come out of my lab have been so successful.

INTERVIEWER: A lot of managers don't know that lesson, though. I don't want to spend a lot of time on it, but I think we have to talk about the Imanishi-Kari. Summarize it. I'm particularly interested in-- I'm wondering if now that time has passed, whether you have a sense of what it was really all about.

BALTIMORE: No, I had a sense then of what it was about, and I haven't changed my mind. It was about political power. It was about John Dingell, and the way he could exercise unilateral political power in US House of Representatives through his stewardship of the Energy and Commerce Committee.

He was a bulldog, he was a bully, and he was out to aggrandize himself as the center focus of the Congress. He was using this particular controversy to prove that NIH didn't know how to govern itself, and therefore had to answer to him. It was awful. When I talked to members of the MIT Corporation about it, many of them said, we have men in the same situation in relation to John Dingell, but because we were public corporations, we couldn't stand up to him.

I could stand up to him, and did. I paid a heavy price for that, but I'm proud that I did it. I think was the right thing to do, and I actually diminished a little of his power, but more important, I stood up for some principles that were important in that he was not going to be the arbiter of how science was done. That took ten years.

INTERVIEWER: You've written a lot about ethics, and who should be in positions to judge science, subsequently. Do you want to sort of summarize your position on that?

BALTIMORE: Well, I haven't actually written much about that, or even thought about in now 15 years. That was all sort of over in 1995-- I guess it was-- 1996, and I did write a little after that. The last 10 years I haven't thought about it a lot.

There's no question who should judge science. Other scientists are the only ones who can judge science, because science as it is happening is very technical, very complicated, and very tentative. Everybody in the business knows that. If you look at it from the outside, the uncertainty and the tentativeness of it doesn't come through. It can't. People are looking for certainties, and there are in the larger sense no certainties in science. Certainly in the smaller sense, when things are really going on when they're being argued in the journals, there is no certainty.

Coming in from the outside and trying to arbitrate that is just an impossibility. The peer review system, which is the core of American evaluation of science, is the right way to do things, and in a deep sense, the only way to do things.

INTERVIEWER: This is probably a good time to ask about the work you've done on science policy in Washington, and particularly around the search for an AIDS vaccine. What should be the role of government in supporting science?

BALTIMORE: Government's the only place that there is the money to do modern science. The government funds science in an open way, so that the results of it are available to everybody. There are patents, and the patents do restrict the availability of science, particularly for commercial development, but for basic development, everything's on the table. The government is doing funding of science in the public's interest in a real sense, and that should be their role.

There are elements of science where the scientific-- particularly as they relate to medicine-- community isn't necessarily excited by working on the problem, but the public policy implications may be so important, as in the AIDS epidemic, that the government should use whatever power it has to get people to focus on the issue. Just getting people to focus on it isn't enough. You've got to get the very best people to focus on it, because they're the creative ones.

The number of really creative scientists any one time is very small, and you want to get them involved. I think the government has to take an active interest, even to developing institutions itself. Finally, we were able to get the government to develop really the only laboratory of its kind at NIH, a focused laboratory on AIDS vaccine development. It's called the Virus Research Center, and it's largely focus on HIV.

That aggregated some really very good people all together in the same building, with the same focus, and that's the government at its best, doing that.

INTERVIEWER: What was your experience like, or what's been your experience working in Washington? Is it frustrating?

BALTIMORE: Oh, it's frustrating. It's frustrating because you're looking for very intelligent responses, and you're getting responses which are political, and which are often not from the very best people because of this sort of bureaucratic machinery that runs the government. It isn't necessarily the very best people. They're very well meaning people, I must say. I've never found people whose concern is any deeper than the people that work in government agencies on health, but they're not necessarily the smartest people from the point of view of making policy.

I ran the AIDS vaccine research committee, which was a committee sort of advising the government on research focus, and all of us were frustrated on that committee.

INTERVIEWER: Is that sort of--

BALTIMORE: Actually, there is an interesting MIT connection here. I was at a some event-- I don't remember where. Walter Rosenblith, who at that point was-- this is now 1985, Walter is long out of administration at MIT, but was very involved in Washington, particularly in the Academy of Sciences.

He came to me at this cocktail party and said, David, you're going to get a call to ask you to co-chair a committee to consider what we should be doing about the AIDS epidemic for the National Academy of Sciences and the Institute of Medicine. I'm just saying, if you can, I want you to accept, and to do this. I think it's very important. I did get a call, and I did accept, and it was very important.

This was the Reagan administration, and Reagan was running from anything that smelled like homosexuals or AIDS. All those sort of Republicans got embarrassed about these things, and I felt we had a responsibility in the scientific community to insist that we have a serious program for research in this area. I was able to put together some very good people to look at that question, and to come up with that recommendation, and we did, in a report called-- I used to know the name of that report very well-- anyway, the report to the National Academy of Sciences and Institute of Medicine.

We said this should be a billion dollar research program. A billion dollars was a lot of money at that point, and it was a sort of audacious recommendation, but it was accepted. Within about two or three years, we had a billion dollar research program.

INTERVIEWER: Do you think of that as a contribution you've made for public service?

BALTIMORE: Absolutely. Writing that report was-- I did that with Shelly Wolff, who was then at Tufts, and is no longer with us And he was on more on the medical side, and I was more research side-- It was as important any report ever written at the National Academy.

INTERVIEWER: So I want to talk about Caltech and your lab, but let me ask just a couple of questions-- generic questions-- about MIT. Having spent so much time here, what is it you think that makes MIT unique as an institution?

BALTIMORE: I think the good things about MIT-- first of all, it's focused in science and technology, which is so important in the modern world. It gives a particular flavor to any question which is which is raised. It also tracks these just wonderful students who are a pleasure to work with, and who to go out and make a difference in the world.

I think the real contribution of MIT is that it doesn't take itself too seriously. It takes ideas seriously, but as people, the people are relatively informal. They are not self-aggrandizing the way academics can be and often are in the humanities, I might say. The combination of its style, of its sort of rugged nature-- the buildings are-- some of them are impossible. Until the Gehry Building was built, it looked like a corporate factory.

It is a leveling atmosphere, which is really very good. You have to prove yourself, and you have to prove yourself every day, and that's how it should be.

INTERVIEWER: How about the role of collaboration?

BALTIMORE: Collaboration is something that people talk about all the time, and is now almost an issue. When I came up at MIT, we had very little collaboration, even within the department-- never mind the silos of the individual departments.

When I said I want to have a joint laboratory with Harvey Lodish, who was just coming-- I knew him from before-- he was coming two years after I came, maybe a year. I said, well, why don't we just join our laboratories and sort of work-- nobody had ever done that before at MIT, or at least in biology. I don't know about anywhere else.

That notion of sharing equipment, that your laboratory wasn't your fortress-- that was new. When I actually set up the Whitehead Institute, and set it up to be collaborative, and to have central cores of equipment, rather than having his equipment, and her equipment-- that was new. It actually set a tone for the whole country of how you build these kinds of places.

INTERVIEWER: Now it seems like in MIT is sort of on the forefront.

BALTIMORE: Yes. Now MIT has really picked up the mantra of collaboration, and is focusing on it. I worry about it. I worry about it from the following point of view: collaborative science tends to be complicated science that requires many different inputs but isn't necessarily driven by a single imagination. It is the individual imagination of scientists that makes a difference in the world. I'm no fan of collaboration, per se. Certain kinds of projects can only be done collaboratively, should be done collaboratively, and it's a good thing for those, but if we lost the focus on individuals, we would lose the core of innovative science.

INTERVIEWER: That's an important thing to say. Is there anything else you want to say about in MIT that you think is important too?

BALTIMORE: I could talk about MIT forever, but no. I needn't.

INTERVIEWER: Let's look it Caltech. Talk to me a little bit about the appeal of going there, and how you felt about being president, and your accomplishments there.

BALTIMORE: Let me say one other thing before that, because I came back to MIT again. I went to Rockefeller, and then the Imanishi-Kari incident exploded in my face, in a way. I couldn't continue on as president there. I stayed on for awhile as professor, because I had a laboratory with some momentum, and my wife a good job in New York.

I was offered, actually, right when everything--- the day I announced that I was stepping down, I got a call from the president of MIT, saying, you want to come back? Do you want to come back? I said, give me a minute to think. I finally decided that is what I wanted to do, and so I came back here in 1994,1995. I figured that was going to be the rest of my career here, was going to be here. I wasn't going to do anything else. I didn't even think anybody would ever asked me to do anything else, because there had been such a sort of acrimonious situation. I made a couple mistakes as president of Rockefeller, which I won't talk about, but I could sometime.

Here I was-- and had now laboratories in building 68-- and I got a call from Caltech, saying would I talk to the search committee for the presidency. I said, sure, I'd talk, and I figured they wanted my sage advice about institutions. There weren't that many people who had as much experience as I did in developing institutions at the time.

They made it plain that they wanted to consider me for the presidency. I said, you've got to know what you're getting into here, and be absolutely certain that you're comfortable with it. The guy who had written the book about me, called *The Baltimore Case*, was a faculty member at Caltech-- Dan Kevles. He later left and went to Yale. He was the chairman of the Faculty at the time, the man who had appointed the committee that then sought the presidency-- sought the president.

He had certainly vouched for me, and knew as much about the incident as anybody in the world. He could answer almost any question, but the man who ran the search-- Kip Thorne-- was a theoretical physicist. Caltech, as opposed to any other institution, has the Faculty run a search for the new president, although the board ultimately makes the decision. Kit talked-- and I know this-- to every single person who had anything to do with me at any point in my life, if they were still alive. He finally convinced himself that they were safe offering me the presidency. By that time, the Imanishi-Kari incident had resolved completely, and she'd been entirely exonerated. By implication, I had been exonerated for what I had done. They finally said, you are our favorite candidate. They made the recommendation to the Board. The Board came to me and said, you are our chosen candidate-- do you want the job? I had to take the idea seriously, and that was really tough. I had more or less made up my mind that I wasn't going to take on anymore administrative activities. I had a lot of science I wanted to do. I was not unhappy about that.

I got excited by the idea. Really what got me excited was that Caltech is a unique institution. It has 900 undergraduates, 1200 graduate students, and 300 faculty. It hasn't changed those numbers except for a little creep in the graduate student side in 30 years. It is the only steady state university-- probably, major university in America.

Everybody else is always growing. They judge their presidents by how much they've grown-- this new initiative, a new school. Caltech grows a little bit, maybe 1 percent a year, maybe not. It may build some buildings, because it needs more space for the existing programs to expand, or they may need better space in which to prosecute-- prosecute is the wrong word-- to do their science, or their engineering, whatever it is.

I did, in the end, build some buildings. It wasn't to expand the school in the terms of its purview. Caltech, when it starts something new, it pretty well has to give up something old in order to do it. Nobody else lives that way. It gets the best undergraduates, at least from the point of view of the ability to take tests. They're a joy to work with, and a joy to know. It was a unique request, and I couldn't-- I finally convinced myself that I didn't want to turn it down.

My life took a new trajectory and moved me back out to California, where I hadn't been now since I left Salk in 1968. I went there. Alice, my wife, had more or less finished the job that she wanted to do at NYU, where she was the dean. We were actually living a commuting life there.

She was going to take off the next year, anyway, and she didn't know whether she'd go back, or wouldn't go back, but she was excited by the adventure of moving to California.

INTERVIEWER: We have like a minute. Can you very quickly tell me what's going on in the Baltimore lab now, and whatever you'd like to say about your time at Caltech?

BALTIMORE: My time at Caltech has been very wonderful. It's a great school. It really is a very special place. I was able to run a big campaign, and to bring a lot of money, and build some buildings that needed building to maintain the strength of the institution. I got the single largest gift in history to an academic institution-- $600 million from Gordon Moore. That gift-- about half of it went into programs, and half of it went into endowment.

The money that went into programs has allowed Caltech to develop strengths in all of its various elements in ways that would not have been possible otherwise, so I feel like I've made a real contribution across the whole board. I've stayed on there because a professor at Caltech is coddled, and treated very well, as well as you can be. I mean, there are exigencies of the world, particularly the economic situation now that affects Caltech like any other institution.

I enjoy working there. I built back up my laboratory, because during the time I was president, I maintained a laboratory there, but it was a somewhat restricted level. I cut out a lot of things that I had been doing before. I've now developed a laboratory with really two arms. One's a basic research side, following along in the same traditions that I've always worked in-- using viruses as probes working on the immune system, and working on the inflammatory responses. We're working on now this new element-- we're working on micro RNAs, which are something that was only discovered really in 2000.

I've got some very good people. Things are going well. The other side of my laboratory comes out of some experiments that a young woman who was my student-- and is still in the lab as a lab manager and postdoc-- Lili Yang did for her thesis. It was to show how we could change the properties of the immune system by using gene insertion methods.

I won't go into the details of it, but I've made that now into a program that I call engineering immunity, in which we're trying to reengineer how the immune system functions in people as well as in animals. We have programs in cancer and AIDS that are going along. I got the largest grant of my life from The Gates Foundation to try to develop the HIV side of this.

I just got another huge PO1-- one even bigger-- grant from the government for the cancer program. That's very exciting, and I've been able to bring people together from UCLA and Caltech, so that we have a medical outlet for the kinds of things that are going at Caltech. I just initiated this year's-- what I call The Joint Center for Translational Medicine, between Caltech and UCLA.

It's a very special environment. Unfortunately, Caltech and UCLA are a little far away from each other, particularly when the freeways are all clogged up. There are logistic issues. It's not like Harvard and MIT, which are right next to each other.

INTERVIEWER: Thank you so much

BALTIMORE: My pleasure.