Lenfant, Claude 2000 D

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Dr. Claude Lenfant Oral History Transcript

Conducted by: Dr. W. Bruce Fye

March 13, 2000

This is the fourth in a series of oral history interviews with Dr. Claude Lenfant, Director of NHLBI, conducted in Anaheim, California, on 13 March 2000.

FYE: Our discussion today will start in the year 1970. In 1970, you were appointed Director of the Division of Lung Diseases at the National Heart and Lung Institute and you held that position until 1980.

LENFANT: Correct.

FYE: Who appointed you Director of the Division of Lung Diseases?

LENFANT: It was [Theodore] Ted Cooper, who was the Director of the Institute at that time.

FYE: Tell me about the dynamics of how that appointment came to be.

LENFANT: I thought we had spoken about that. When the Heart Institute was renamed the Heart and Lung Institute, Cooper sent a letter to lots of people in the country to ask what should be done, what was needed, and so on. I got one such letter and, as I told you, Hugh Smith and I worked on a reply to that letter. That started the whole thing in the sense that Ted Cooper liked our reply, he invited me to come to NIH, and eventually he asked me to come to get the program going. I came on a leave of absence of two years and I am still there!

FYE: When you came as Director of the Division of Lung Diseases, what were your main responsibilities?

LENFANT: Basically, it was to get something done, to give a focus to the Division's activities. It is interesting. Let me reflect on that after almost 30 years of experience. The community out there wants something. They want an institute for this, an institute for that, and then, what they want is a focus, a place that if you wanted a map of NIH they can put their finger on it and say this is where it is. Basically, that is a little bit what happened. Do not believe that billions came in to start a lung program. I was becoming the focus that people could call or talk to, and little by little we started a few new activities, and this is what happened. My responsibilities were to generate activities, to make sure that the world out there would know that, yes, there was something happening.

FYE: Thinking about the first years at Bethesda, did you develop an intramural research program?

LENFANT: I forget if it came the first or the second year, but, anyway, it came pretty soon. We opened an intramural program. There was a fellow, who, I believe, started in the Allergy Institute, and then talked to me, and we decided to create something, so, eventually a pulmonary branch was started. That fellow was there for maybe two or three years, something like that, and he left. Then, during that time, the decision had been made to send somebody who was a very smart man, I am sure you know his name, it is [Ronald] Ron Crystal. Ron Crystal was a hematologist. His interest was in blood disease, mostly the molecular aspect of blood disease. He had been a postdoctoral fellow under [W.] French Anderson. I do not know if that name rings a bell, but French Anderson is a man who is in the world of gene therapy. He is the one who really developed the concept of gene therapy. He was in our Institute, he was the head of the branch or laboratory of molecular hematology, and Ron Crystal was one of his postdoctoral fellows or maybe a research associate. While we developed the branch with the first man, Ron Crystal was sent to the Cardiovascular Research Institute in San Francisco to learn something about the lung.

FYE: Julius Comroe was there?

LENFANT: Yes, with Julius Comroe. Ron went there and he spent two years, something like that, and when the first fellow left, then Ron Crystal was appointed to be in the branch. Shortly after that I gave up my responsibility there and Ron Crystal took over.

FYE: The lung branch?

LENFANT: Yes. Intramural.

FYE: Intramural research on pulmonary matters.

LENFANT: Yes.

FYE: What was the focus of research?

LENFANT: During the term of the first man, it was not a very good focus. It was very diffused, and certainly not very innovative. When Crystal came, he came with his background of molecular approaches that he had gotten from French Anderson in the blood area, and he immediately started introducing this approach in lung research. His first focus–I am not sure I can remember–but I think it was work in alpha-1-anti trypsin deficiency, which in those days was the big thing.

FYE: For emphysema, but their relationship...

LENFANT: Yes. He also focused on idiopathic pulmonary fibrosis, trying to find the molecular aspect of it, and then he got involved in sarcoidosis. Ron Crystal is a very smart guy. The problem that he had was that he had kind of a hit and run approach to things. He would touch or start something in lots of areas but he would not persevere. He would jump from here to there all the time, and that brought lots of criticisms of him. I personally think that he did not deserve all the criticisms that he received, but all that was somewhat compounded by his personality which did not help to attenuate the intensity of the criticisms.

FYE: How did you make the Institute known? How did you publicize your new institute to the academic community, to the scientific community, and around the country?

LENFANT: The name of the Institute was changed and there were more people. You have all these people who are watchers of NIH, and they know what is going on here at the Institute.

FYE: I guess It is hard to hide it if suddenly the word “lung” winds up in the name of the Institute.

LENFANT: Yes. That was no problem. But, in addition, I was not an unknown quantity in the field. In those days, I was a member of the Young Turks [American Society for Clinical Investigation], I was a member of the Old Turks [Association of American Physicians], and I knew a lot of people. I was pretty active. I had, I think, a good research program. In those days, it was a good research program. Today, it would be a lousy one, but that was what it was in those days.

FYE: I know this occurred just before you got to the Institute, but what were the dynamics of the name change, and the fact that a lung branch was developed. You have mentioned several times that Ted Cooper was the main pusher behind that but who were some of the other players? Where was his base of support?

LENFANT: I think it was mostly the American Lung Association and the American Thoracic Society. Then, these two groups were very friendly. Now they are “divorcing” or splitting, if you want, but at that time they were very friendly, and they were the movers and shakers in the political arena. They wanted to have a lung institute and Cooper took the position, which I support 100 percent, that that was nonsense. There was no reason to have a lung institute when we had a heart institute. The two should be coupled. So it is very interesting. That still happens actually; groups out there want to have a focus at the NIH, but they do not know exactly what they want besides wanting a focus. They want a name, as I said, a name on the map. It does not matter where it is, what it does and so forth, it is the name.

FYE: Was there much resistance from the heart community that the National Heart Institute was suddenly going to have a sibling or another entity as part of it?

LENFANT: Yes, I think there was. In 1973 or 1972 the National Cancer Act was passed, and Cooper, who was a real political activist, actually he was very astute, said, “Well, let’s develop a heart and lung act.” He almost succeeded in having something equivalent to what the Cancer Institute had. Now, you have to think about how it was then. There was the hierarchy in which a distinct entity would sit at a given level in the government. You had Departments, and under the Departments there were agencies, under agencies there were bureaus, under bureaus there were divisions, and then branches. Okay, all the institutes at the NIH, which was a misnomer really, were divisions. So the NIH was the agency and then you had the divisions. There was no one at the status of a bureau. The National Cancer Act elevated the cancer division to the status of bureau.

FYE: Even above the level of the NIH?

LENFANT: No. Again, here is the way it worked. You have the Departments, okay? [draws a diagram] Under the Departments you have agencies, and in our case the NIH is an agency. Under the agencies you have bureaus and under the bureaus you have divisions. At NIH, here were NCI and the National Heart and Lung Institute. There was no one at the level of bureaus. What the Act did was to elevate NCI to the status of a bureau. So Cooper was very anxious to get the same thing for NHLI. What happened was that the Act gave NCI two or, I guess, three things. One was a relative independence, two, a budget bypass authority, and three, the director of the Cancer Institute was a presidential appointment.

FYE: So all of the advantages you have just listed were the advantages for the National Cancer Institute after the National Cancer Act.

LENFANT: Correct. Cooper wanted to have the same thing, but what happened was that [Richard] Nixon, who was the President at that time, had his troubles, and so the elevation of the Heart and Lung Institute to the level of a bureau was only partially accomplished in the sense that it became a bureau, but it did not get the budget bypass and the director was not a presidential appointment.

FYE: It advanced in some respects, but it clearly did not have the autonomy or the ability to control its destiny that the National Cancer Institute had.

LENFANT: Correct. You have it.

FYE: Tell me how would it be an advantage or disadvantage to have the director be a presidential appointee? I sense that was viewed as an advantage.

LENFANT: What it really meant was that the budget of the Institute instead of going up the Department would go directly to the OMB [Office of Management and Budget]. This made it less subject to political manipulations and things of that sort.

FYE: So rather than have to go up the chain–you have just drawn this diagram for the National Heart and Lung Institute–it had to go up through the NIH and through the Department, at that time of Health, Education and Welfare, to get any budget authority from the government.

LENFANT: And then compete with all the other institutes.

FYE: Right, but by going directly to the OMB . . .

LENFANT: In principle, you eliminate or reduce the competition.

FYE: So the Cancer institute had a distinct advantage.

LENFANT: Correct.

FYE: This must have caused a great deal of tension among the institutes, I would think.

LENFANT: Oh, yes. In fact, after the Heart and Lung Institute became a bureau, and it was realized that it had become a bureau but without the privileges of being a bureau, then there was lots of agitation among all the other institutes and their constituent groups. That resulted in all the institutes becoming bureaus.

FYE: I can identify very distinctly with that phenomenon. There is a certain analogy in terms of departments of medicine and divisions of medicine; how some have jockeyed to become departments and the tension that that has caused with others. This happens any time there are people that perceive their peers getting an advantage. So how long was this problem in terms of this sort of political jockeying at the NIH?

LENFANT: It took a couple of years. But to come back to your original question which was– all of a sudden it skipped my mind–you were asking about . . .

FYE: How the heart and lung institute was received . . .

LENFANT: Yes. That is right. You see with the development of the National Heart and Lung Act, the American Heart Association became agitated because the Act stated that the budget of the Institute would be such that at least 15 percent would be for the lung and 15 percent for the blood. This is the time when the blood came into the picture and got its name attached to that Institute.

FYE: Now, let me understand this. Do you mean 15 percent of the total NIH budget or just of the NHLBI budget?

LENFANT: The NHLBI budget.

FYE: So really at a maximum 70 percent of NHLBI's budget at that point was still theoretically targeted for heart and 30 percent was split between lung and blood.

LENFANT: Exactly. You see, the heart community was very concerned and so, in turn, that got the lung and the blood communities very concerned. They fought very hard to assure that it would be at least 30 percent; in other words, it was not a ceiling, it was a floor. So this is what happened, and, of course, over the years, for a variety of reasons, this floor was "raised" and the budget for the lung and the blood increased quite significantly. Today, the split is probably something like 60 and 20 and 20.

FYE: So, gradually, the amount of the budget that went toward lung and blood increased, but never to the level of heart. Heart remains very dominant.

LENFANT: Yes, but one thing that you have to understand is that much of the research which is supported by each of the three entities is germane to all of them. The best example that I can think of, actually, is the one that I use each time that I discuss this with the heart community. Look at thrombosis. You see thrombosis is part of the blood budget, and I tell the heart people, "What are you talking about? Basically you own half of the blood division."

FYE: It is fascinating, too, as I reflect back on your early career with the heart-lung machine and the oxygenators how you obviously were correlating and coordinating and working in all three areas because they were all vitally important to the research that you were working on. There is a certain symmetry of your career and the structure of the institute. Tell me a little more. I am sure there must lots of interesting stories about this jockeying for position among the lung advocates, the heart advocates, and the blood advocates, both within NIH and externally. Could you expand on that?

LENFANT: What you have to understand is that, yes, there was lots of jockeying, but at least there was an apparent protection of the heart community owing to the fact that Cooper was clearly recognized in the heart community. Basically, he was a cardiovascular pharmacologist, he was not really a cardiologist. But then he was followed by [Robert] Bob Levy. And when Bob Levy left, Peter Frommer... Do you know Peter Frommer?

FYE: Yes. I saw him today, in fact.

LENFANT: Peter Frommer was clearly viewed by the heart community as the front runner to replace Levy but, in fact, it did not happen. I became the director. But I can tell you that the American College of Cardiology campaigned very hard for Frommer and, in fact, did whatever they could to keep me from becoming the Director of the Institute.

FYE: Was this in 1972? No, this was 1982. We have moved up to 1982.

LENFANT: Yes, the College of Cardiology was–I suspect that it was in great part because Frommer was campaigning to get the support from the College of Cardiology.

FYE: I do not know whether I should fast forward to this point or try to remember to come back to it, but, I guess, Peter was heavily involved in the CASS study [Coronary Artery Surgery Study] and that would have been started at that time. I am thinking that he would have been known to the cardiology community...

LENFANT: No, the CASS was not so. . .

FYE: I know it started when I was still a fellow at Hopkins, so it started in about 1975, I think.

LENFANT: Yes, the CASS was started, but Peter had been very much involved also with the myocardial infarction research units.

FYE: The MIRUs.

LENFANT: Yes.

FYE: So, he was well known to the heart community?

LENFANT: Yes. He was running this program.

FYE: He was viewed as an advocate for them?

LENFANT: Sure. And there, you see, I was the enemy really!

FYE: How did you win? What were the forces that came into play?

LENFANT: Well, don't ask me. I do not know.

FYE: Just from your perspective. What was it that you think led to your choice?

LENFANT: First of all, at that time, my *curriculum vitae* was somewhat different than that of Peter Frommer. I had been a full professor in a good university, and I had built up an extensive bibliography. I had been at the Young Turks and the Old Turks, and Frommer had none of that. I do not know what he did during the interview process. I was not there. All I can tell you is that I knew [James] Jim Wyngaarden, who was the one who selected me. I knew him because I had been offered a position twice when he was Professor of Medicine at Duke.

FYE: I do not think we ever talked about that.

LENFANT: So, I knew Jim, and, in fact, it was very interesting. After Jim appointed me, the first thing that he said to me was that now that you are appointed, I am going to give you all these letters I get from the College of Cardiology and I want you to get them off my back!

FYE: How long did it take for you to get them off his back? How long did it take for the ACC and other heart oriented groups to accept you into their community.

LENFANT: You know, we drafted letters. Then the ACC president at that time, I know exactly who she was, it was a woman, Suzanne Knoebel, and she was dead against me. She really was!

FYE: Interesting.

LENFANT: Very much so. But I got to know [William] Bill Nelligan, and Bill and I got along very well. Also, I mean, my interpretation is that Bill and some of the people who were there–I cannot remember, there was a fellow, the editor of the ACC journal, Dack...

FYE: Simon Dack.

LENFANT: Simon Dack was there. I met with them and they were all rather on the conservative side of things and I am fairly conservative myself, so we kind of hit it right! Yes, that was quite interesting. Bill and I got along very well.

FYE: He has a reputation, and, of course, I have come to know him in more recent times, as being an amazing facilitator. He has an amazing ability to mend wounds and bring people together. Certainly that is the idea with the American Heart Association and the College and such. So you sense some of that yourself.

LENFANT: Oh, yes, that was quite an interesting time.

FYE: It strikes me that you certainly did have qualifications in the area of cardiology because it just depends on how you look at the research you were doing on the heart and lung in the 1950s. I mean, you could say, well, he is a pulmonary physiologist . . .

LENFANT: But nobody knew that about my heart work.

FYE: I see.

LENFANT: Nobody knew that really.

FYE: Because your identity in the United States had all been focused around pulmonary physiology.

LENFANT: Yes.

FYE: Going back to the earliest years at the lung division, how did you develop the extramural programs? How did that work in terms of getting started?

LENFANT: We developed lots of programs that had never existed before. For example, the specialized centers of research [SCOR] were developed at that time. To use the jargon of NIH, NIH now issues lots of requests for applications [RFAs]; the very first request for applications at NIH was issued by the lung program. Also, we created a number of training programs which were kind of a novelty. We started a program which was called the pulmonary academic award, we started another program which was the training program with... How would you call it? I cannot remember how it was called. We started a number of programs and that is how I got acquainted with people at the Marshfield Clinic, actually. There was that fellow Wenzel,

FYE: Fritz Wenzel.

LENFANT: And I remember a man . . .

FYE: From the Marshfield Clinic? Richard Sautter probably, or Dean Emanuel.

LENFANT: Dean Emanuel, yes. We also started a program that actually was adopted NIH-wide afterwards, it was the young pulmonary investigator award. Then it became the young investigator award of the NIH, and eventually it became the new investigator research award. So, we started a number of new programs.

FYE: Of course, the attraction for the academics or the researchers was that these programs carried money with them . . .

LENFANT: Yes, sure.

FYE: They were grants, and so it enabled them to pursue their research interests.

LENFANT: Yes. That is probably what got the Heart Association and, I suspect, the College of Cardiology to be a little on edge wondering what was going on there. All that money going into the pulmonary profession. It was not all that much money; actually, it was a very modest amount of money. But you know the perception of people is often what counts.

FYE: When you wanted to start new programs in the early years, the 1970s–some of the programs you just mentioned, the innovations–how much bureaucracy did you have to go through? Did they have to be approved by the NIH director?

LENFANT: Very little bureaucracy, and, yes, the Institute Director had to approve them.

FYE: Did you have a budget and you just . . .

LENFANT: No, Cooper had to approve that. He was very supportive. I mean, he really had no choice, he was the one who started the whole thing.

FYE: But, for each of the programs you mentioned, would you have to develop a budget for it?

LENFANT: We developed the idea, the concepts, and then, I would go to Cooper and say, “This is what we want to do and can we get the money?” There was a little bit of back and forth on how much money. But Cooper was very supportive.

FYE: Individuals like that make a huge difference, don’t they?

LENFANT: Yes, this is what it takes. Somebody who wants to do it.

FYE: Let me try to stay on track here in my questions. One of the things I wanted to ask you about was what clinical facilities were available at NIH for lung diseases? What did you develop in the area of the Clinical Center for a clinical program in lung diseases?

LENFANT: Crystal is the one who started the clinical research components–I forget if it was about alpha-1 anti-trypsin deficiency or idiopathic pulmonary fibrosis or whatever. Then he got into cystic fibrosis, too; he did the first cases of gene therapy in cystic fibrosis at NIH. We had a ward for the Institute and, basically, if you had a protocol that was approved then you had access to the facilities.

FYE: What would be the mechanics of getting patients to the institute on that ward?

LENFANT: Contacts.

FYE: So you would know clinicians or clinical investigators around the country, and they would contact your office or Dr. Crystal’s office.

LENFANT: Yes. Dr Crystal is talking at this meeting today.

FYE: What a small world.

LENFANT: Now, he does some gene therapy for angiogenesis.

FYE: Angiogenesis.

LENFANT: Yes. I saw his name, and I do not know if he is the one who is going to give the paper. I suspect it will be him but, anyway, he is talking. He is a big competitor of Eisner. Do you know him?

FYE: Yes, I do.

FYE: Tell me a little about the clinical facilities and the Clinical Center. I know that was not your focus, but what was it like? About how many beds and who staffed the ward.

LENFANT: I think the Institute had maybe 40 to 50 beds, maybe 60 beds, something like this.

FYE: For heart, lung, and then eventually blood diseases.

LENFANT: Yes, and metabolic diseases too. We had beds on two floors. I must say I do not know how many beds we really had, but patients were admitted on a protocol. I mean, we needed to have a protocol to bring patients. We cannot take somebody else's patients.

FYE: They had to fit into the clinical research that the Institute would support.

LENFANT: Yes.

FYE: Now I am going to move ahead in time and turn to 1982. In that year, in July, you were appointed director of the National Heart and Lung Institute. It was the National Heart and Lung Institute at that point, and not Blood yet?

LENFANT: It was all three. Blood was added in 1974.

FYE: Okay. I have that elsewhere!

LENFANT: I think it is 1974.

FYE: Okay, by 1976, certainly.

LENFANT: It may be 1976. If you want the absolute date, we can find it somewhere.

FYE: It was 1976. I am sorry for this confusion.

LENFANT: That is right. They [Blood] came at the time of the re-authorization of the Act.

FYE: 1972 was the act.

LENFANT: I think, for the heart institute, it was in 1973. Cancer was 1972, and we were 1973. The authorization is always for three years, so it has to be. . . I mean, we could find it . . .

FYE: I know the Act was passed in 1972,

LENFANT: Okay. So, it may be that the re-authorization lapsed for one year, this happens all the time, but the blood constituency managed to get their name in the Institute at the time of re-authorization.

FYE: Maybe we should talk a little about that. We talked about the dynamics of how lung became a sibling, and now there is another little sibling, blood, coming along.

LENFANT: Yes.

FYE: Who pushed that, and how did that happen?

LENFANT: The blood community had a lobbyist in Washington who was very effective. His name was John Grupenhof. He is still around except that he no longer works with the blood. He was a former staffer of some member of Congress. He was a very effective man. So, anyway, the blood name got in.

FYE: And all of this, of course, is a political process.

LENFANT: Oh, sure.

FYE: That is how these names of institutes are changed.

LENFANT: Sure.

FYE: But was there much stimulus from within the NIH to do this or was it mainly from the blood community and this lobbyist?

LENFANT: It came from the fact that, even when the institute was only the Heart Institute, we were supporting lots of research on thrombosis. For example, the urokinase and streptokinase trials of the late 1960s were done in the Institute. In fact, we had a branch which was called the thrombosis branch in those days. When I came to the Institute, there was an established thrombosis program. So, by then, there was quite a bit of work that that had generated. There was also a blood intramural program. In fact, when the Institute's Act passed in 1972, I believe that the blood division was created at that time. And because there was a blood division, the blood community demanded to have its name put in the name of the institute. I think that is how it worked.

FYE: Did that trigger the same response from the heart community? Then I assume the lung community had a response, although they had only recently joined the club.

LENFANT: I think there was always a bit of anxiety. Of course, now today, 20 years later, it is completely irrelevant because every month somebody wants to have a new institute, but in those days it was much more controversial. In fact, I think that what happened to the heart institute was precedent setting because after all that there were more and more splits, and new Institutes appeared.

FYE: So 1982 was a pretty dynamic year in Washington in terms of health and research. You mentioned that was the year you were appointed Director of the National Heart, Lung, and Blood Institute. C. Everett Koop was sworn in as Surgeon General that year and James Wyngaarden was sworn in as NIH Director.

LENFANT: Yes, it was. It was early in January.

FYE: Just a few months before you, as you were appointed in July.

LENFANT: Yes.

FYE: I guess from what you said earlier in terms of James Wyngaarden's role in your appointment if he had not been the Director, it might have been different.

LENFANT: I certainly do not want to imply that Jim Wyngaarden appointed me just because he knew me. I mean, I do not want to be boastful in saying this, but I really think that my qualifications or experience far exceeded that of Peter Frommer.

FYE: Were there other candidates as well that you recall or were you two more or less the short list at the end of casual inquiries?

LENFANT: There was another internal candidate who, at that time, was the Director of the Division of Heart and Vascular Disease. It was a woman by the name of Barbara Packard.

FYE: Oh, yes, Barbara Packard.

LENFANT: But she too had had no university experience. I do not want to have to say this, but such experience is what I think was unique about me, because, at that time, you could count on your fingers to identify the people at NIH who had had a university experience. They were very few at NIH. I was one of the rare specimens who had spent 10 or 11 years in academia and advanced through the ranks from instructor to full professor of medicine, biophysics and physiology, in a good university. In fact, Cooper had been in a university, he was professor of pharmacology and chair of pharmacology, at the University of New Mexico in Albuquerque, but I think that he and I were probably two exceptions, certainly within NHLBI.

FYE: This raises an interesting question in terms of why it should have been that there were so few people from outside at NIH. Is it that there were barriers to getting into NIH or is it that there was not as much attraction for outside physicians to go to NIH?

LENFANT: I think it is the latter. Remember that the end of the 1960s and 1970s were during the time when there was lots of money going to NIH. Do you know that priority scores between 300 or 400 were being paid.

FYE: I am sorry, what did?

LENFANT: The priority scores for the grants. The grants were paid on those scores. Today you would not even think of paying grants with such a priority score.

FYE: I see.

LENFANT: So I believe that there was lots of money, it was the time when many people who had been at NIH before had gone to the universities, had become the competitor of NIH and then . . .

FYE: Part of it, I guess, is that NIH is a federal program, and it is bound basically by federal salaries in some fashion.

LENFANT: Yes. There was that, too.

FYE: I mean that you are limited in terms of the government assignment of different government ranks or . . .

LENFANT: Although, at that time, the differences were not as great as they are now or what they were a few years ago. I think the great differential in salaries came during the late 1970s and the 1980s.

FYE: That is when the difference arose?

LENFANT: When I went to NIH, I took a small pay cut, but it was not a very significant one.

FYE: I am thinking again of when you were chosen to be Director in 1982. I imagine that you were asked to describe your vision of NHLBI. Could you recall what your vision was?

LENFANT: Oh, yes. That was really the time of the emergence of molecular medicine, and I was appalled to see . . . you see my predecessor was Bob Levy, and Bob Levy, who was, in fact, a very good person, had a very narrow focus of medicine which was lipids. If it was not lipids, it was nothing.

FYE: He saw the world through a lipid!

LENFANT: Sure. And Frommer's vision was really hemodynamics and ejection fractions and things of that sort. I thought that was a very narrow view of the field. I thought that it was time to bring molecular medicine into the Institute, and if you look at the history of the programs, I think that you will see that the emergence of molecular medicine in the institute began with me.

FYE: So, you were explaining that your vision was based on and framed around molecular biology.

LENFANT: Yes. To bring molecular approaches, new approaches, to move the field to the cellular level other than just the physiology. I think that is a fair assessment, and very quickly we put into place a number of programs to implement that.

FYE: Now my perception is that cardiology lagged in terms of moving into the molecular biology era.

LENFANT: Certainly, behind the blood. I mean, the blood is really the precursor of molecular medicine.

FYE: I am thinking of sickle cell anemia with Linus Pauling.

LENFANT: Yes, sure.

FYE: I think that was in 1948.

LENFANT: That is right. Then the lung also moved into that. But not the heart. If you look at the heart at the beginning of 1980, it was all hemodynamics and basically applying physiological techniques.

FYE: This is easily documented, what you are describing now. Why do you think that is? Why is it that blood and lung were so far ahead?

LENFANT: I do not know. Maybe because cardiologists were not trained into that.

FYE: I wonder if part of it is because there was so much emphasis in cardiology at that time on what might be called macro therapies, in other words, ischemic heart disease . . .

LENFANT: Sure.

FYE: . . . although thrombolytics were a big thing and that is not macro. That is molecular biology. But I am thinking of angioplasty and different things . . .

LENFANT: Well, angioplasty started just at the end of the 1970s, 1978 or 1979, but that was still hemodynamics.

FYE: Exactly.

LENFANT: The idea at that time was to reopen the vessel to let the blood flow go through. Thrombosis was not yet brought into the coronary artery disease process. People were beginning to talk about stunning and things of that sort, reperfusion, and the first thing was the concept of the antioxidant. So it was very slow. I would say it was a very slow process to bring in a new concept and new ideas.

FYE: Did you find it harder to engage the cardiology community in molecular biology? In other words, if you had a vision of molecular biology and theoretically that vision would go in all three directions: heart, lung, and blood. Did you find the communities responded differently?

LENFANT: No, the community was very . . . it worked. If you asked these people, I do not know, but I have the feeling that the community accepted all that. I think they saw that I was very supportive of all that was happening. One thing, too, that I think made me very different from some of the other people is that I was very open to talking to anyone, anywhere, anytime. Which I think is very important to do.

FYE: Communication.

LENFANT: Yes. But many of my colleagues at NIH are very aloof, they are in charge! I do not believe in that.

FYE: Who were some of the people that you depended on most in the 1980s after you became Director.

LENFANT: Julius Comroe, Eugene Braunwald. I brought Braunwald back to the Institute as a Council member. There was alsoElliott Rappaport.

FYE: Elliott Rappaport, San Francisco.

LENFANT: Yes. There was, what is the fellow from Emory? I forgot his name.

FYE: [J.] Willis Hurst.

LENFANT: Willis Hurst. You see I had known them on the advisory council. And, of course, John Kirklin, whom I knew for many years.

FYE: Because of your heart-lung research.

LENFANT: Yes. So, I would talk to these people all the time.

FYE: You had a network of people around the country in prominent academic positions and that helped you in your role in Bethesda.

LENFANT: Yes. I may not have been accepted, you see, although I think I was accepted relatively quickly, but certainly within the Institute I was not accepted immediately. But the fact is that I had more assets than other people and people in the institute realized that people knew me out there. It is very interesting to see the dynamics.

FYE: Fascinating. When you say that you were not immediately accepted within the Institute, you mean when you became Director in 1982.

LENFANT: Oh, yes.

FYE: Because you had been there for ten years, I would assume that you would become accepted in that role.

LENFANT: But I had left the Institute for 18 months.

FYE: Tell me about that.

LENFANT: Well, when I went to the Fogarty International Center.

FYE: But did you expect to go back? Was that a leave of absence or what?

LENFANT: No. I viewed that as my basically planning to leave the Institute, the NIH.

FYE: What did you do at the Fogarty Center?

LENFANT: I was the Director of it.

FYE: What did it do?

LENFANT: Only international research activities. In fact, I was the first NIH associate director for international affairs and I was the contact to the rest of the world really. Then, I was part of the U.S. Delegation to the World Health Assemblies, to the World Health Organization, and things of that sort.

FYE: What was the scope of activity of the Fogarty Center at that time?

LENFANT: It did not have too much, it was very limited. It had fellowships and also brought some scholars to the campus. It was actually pretty lousy. It was supporting some programs that it should never have supported. For example, it was supporting sometime somewhere in Panama, I think it was in Panama, it was called the Gorgas Institute for Infectious Disease or Malaria. I forget which it was.

FYE: Gorgas?

LENFANT: Yes, I closed it down. So that did not go down very well! And I had the vision of really making the Fogarty Center a bridge between the NIH and the rest of the world.

FYE: That is what the Fogarty Center did, from your perspective.

LENFANT: It should have done it, but it was not doing it very well. When Bob Levy left, I have forgotten who encouraged me to apply for the directorship of NHLBI. I think Cooper encouraged me to apply. Cooper at that time was at Upjohn. He was the CEO of Upjohn, and he and I had kept a very nice relationship. We became very friendly over the years, and he encouraged me to apply.

FYE: How was the Fogarty Center funded? What was the structure?

LENFANT: Just like NHLBI.

FYE: So it was a part of the NIH.

LENFANT: Yes.

FYE: But you said you joined it as you were contemplating leaving the NIH. Was it that you saw that as a way to meet other people?

LENFANT: Well, I had had enough of NHLBI. Bob Levy and I did not get along very well, and it was really getting on my nerves. In fact, I had left. First of all, I took a sabbatical and spent almost five or six months in Taiwan. When I came back, I applied for the Fogarty position, and I said, "It is time that I decide what I am going to do, I am going to go there." And I got the job. Fredrickson appointed me. Fredrickson was the Director of NIH.

FYE: Donald Fredrickson.

LENFANT: Yes. Then Levy left NHLBI, and Cooper is the one who urged me to apply for the NHLBI job, so I did it and that is what happened. The chair of the search committee was Ruth Kirschstein.

FYE: Now are we back to the NHLBI Director search committee?

LENFANT: Yes. The chair was Ruth Kirschstein. Right now she is the acting director of NIH since [Harold] Varmus left. She was the Director of the National Institute for General Medical Sciences, and she was the first woman director of an institute. She is very, very good, but, anyway, she was chair. [Steven] Steve Epstein was on the search committee. And I am sure that Steve Epstein probably was looking at the candidacy of Frommer with a good deal of interest because they are very good friends.

FYE: Interesting dynamics. So then you came back as NHLBI Director.

LENFANT: Correct, yes.

FYE: And there was a period obviously as you say of some rapprochement when you have to get back into that environment!

LENFANT: Yes.

FYE: How was that?

LENFANT: My position was, "Like it or not, I am here!"

FYE: Okay, you were the Director.

LENFANT: That is right. And I became very assertive–I was probably too assertive, but anyway. . . .

FYE: Was there much turnover? Did many people leave in that context?

LENFANT: No.

FYE: Not really? So they adjusted.

LENFANT: Yes. I made some changes very quickly. I created a new division. I created the Division of Epidemiology and Clinical Applications. Barbara Packard who was a superb lady, really very nice, was not tough enough in my view to make changes, so I took her out of the Division and brought her to my office. And [Eugene] Gene Passamani, do you know him?

FYE: Yes, Gene Passamani

LENFANT: . . . became the Director of it. Gene did a good job there. We had differences of opinion, but Gene is very smart, he is a bona fide cardiologist, and he did well for the Institute.

FYE: What were some of the main challenges that you faced when you became NHLBI Director?

LENFANT: In the first place that was the time when budgets began to have their problems, and then all kinds of new issues came up. That was the beginning of the thrombolytic approach, for example, and the initiation of new clinical trials. There were lots of new things, and then, the lung and blood were growing and pushing things. These were very exciting and interesting years.

FYE: Even despite the budget crunch which was really a problem because of all the excitement, it was still . . .

LENFANT: But my position was–you are going to get a good laugh about this–that people were competing on the budget which was say $700 million and they would say, "Ah, only $700 million," and each time I would go someplace I would say, "Okay would $750 be enough?" Never could anyone say what the limit would be. So I would always close the discussion and say, "Well, are you telling me that you could do lots of things with $50 million more, but you cannot do anything with $700 million?" And I mean that!

FYE: Hard to respond to that.

LENFANT: That is right! I still think that way, actually.

FYE: It is interesting. Biomedical science in this country since the Second World War has been an enormous growth industry and a very hungry child, you might say . . .

LENFANT: Sure.

FYE: I mean, very ambitious...

LENFANT: Sure. And it is fun. I have no problem with that, but at the same time I take the position that once it is done, you have to move on and stop complaining all the time.

FYE: Thinking of the first decade that you were Director of the Institute, what are you most proud of? What are some of the accomplishments that you are most proud of in that 1980s decade?

LENFANT: First of all, the establishment of DECA, the Division of Epidemiology and Clinical Applications. The community would not agree with that, but I think it gave a new identity to epidemiology research, to population-based research. I think that there is no question about that. I also think that I really established the prevention programs in the Institute. They did not exist before. There was the National High Blood Pressure Education program, but that was the only one. During that period of time, I created the National Cholesterol Education programs, the National Heart Attack Alert programs, and we created a program in blood resources and safety. The asthma education program started later. But, anyway, our Office of Prevention, Education and Control was a unique organization, and now it is an organization which is on the national, if not the international, scene. We established lots of international liaisons, and strengthened others. We certainly brought molecular medicine into the program. We created programs like the cardiology prevention academic award. Lots of things happened during this period of time. We multiplied the number of specialized centers of research. When I became the Director, there was only one in coronary heart disease–it was focusing on myocardial infarction–but now we have pediatric cardiology, we have hypertension, we have heart failure–what else do we have–we have heart disease in blacks. There are all kinds of programs. We also started lots of studies to look at the health disparities.

FYE: In different populations.

LENFANT: Yes.

FYE: It is a rich record of achievement.

LENFANT: I think the Institute has grown up during this period of time.

FYE: We are going to come back to a number of those programs because I do want to ask you to expound upon each of the ones you mentioned. Thinking again of the 1980s, you just mentioned some of the things that you are proud of in terms of accomplishments. During your first decade of Director, what were some of your greatest frustrations or disappointments?

LENFANT: I have always been frustrated and still am, actually, by the lack of real interest of the community we serve. They are really like a bunch of little kids to whom you do not give a second serving or more of the dessert. It is that kind of attitude. It is impossible to please these guys.

FYE: By those guys you mean the biomedical research community, the people that you say you serve, the people that apply to the NHLBI for grants, and the like.

LENFANT: Yes. One thing, too, that I found very difficult, but that is America, we just cannot establish priorities and say, "Okay, that is what we need to do and we are going to do it, even if it takes not doing something else." Clinton, that is what he does, and look at all the flack that he gets from everybody. But for this nation it is very difficult to do that. This is something that NIH seems not able to do, that is, to end something to do something else.

FYE: Obviously, you are contrasting that with your experience in France, I assume. But how is it different, what do you think it is that makes this country different, is it the democracy versus . . .

LENFANT: Yes. One thing that I did that was a very interesting and challenging activity was to close the cardiac surgery program in the Institute.

FYE: I have that on my list, but let us talk about that.

LENFANT: That was after Glen Morrow died. We were left with [Charles] Chuck McIntosh who was a superb surgeon. His techniques were just perfect, but it was very difficult because he was not certified. I do not think he was Board Surgery certified, he just became a surgeon. I do not know how this happened, but there he was, and for this reason, we could not make him the chief of surgery. So we looked around, and in those days salaries were bad, but we found a man who had personal wealth and had been a reputable surgeon. His name was Richard Clark. He was from somewhere in St. Louis, I think. Anyway, we brought him in and pretty soon it became clear that he was not what I had expected. I understand, although I never witnessed it myself, that he was no longer a very surgeon. So what came from all that is that the program began to flounder. Then we recruited another surgeon who was Julie Swain, the sister of Judy Swain.

FYE: Julie is the surgeon, though.

LENFANT: That is right.

FYE: Is that what you said, Julie Swain?

LENFANT: Yes. She is the identical twin of Judy.

FYE: Identical twin?

LENFANT: Oh, yes. Except for one thing that Judy is a very sociable person.

FYE: Judy is social?

LENFANT: Yes! In contrast Julie is a ball of fire. She cannot talk, she can only yell. And she is a surgeon. I mean, it is very interesting. Somehow there must be a little gene someplace that did not work. Judy is married. Julie is not married and is a very temperamental lady. So Julie got in there and she was throwing instruments across the surgery room! Everybody was complaining and then there were a few situations that were a little bit fishy. There was no research protocol. It was all service, and most of the surgery was by admitting patients from Pakistan, India, and all that at great cost for the Institute. Finally, we decided enough was enough. We would close cardiac surgery and Edward Korn, who was the Director of the Intramural Division, and I confronted this option; one day we called the troops and told them. That was quite an experience, I must say. Julie left shortly after and Clark left, and that was it. But now we are thinking of reopening the cardiac surgery program. You know that [Dr. Elizabeth] Betsy Nable is with us. Do you know her?

FYE: I do. It is funny I do know her. She has come back. She is now at the Institute?

LENFANT: Oh, yes. She was never at the Institute before.

FYE: She was in Michigan?

LENFANT: Yes. She was in Michigan.

FYE: I think she was at Johns Hopkins at one point.

LENFANT: Yes. I think she was at Johns Hopkins at one time, but I am not sure. But she is a product of Boston, of [Eugene] Gene Braunwald.

FYE: I knew her on the Publications Committee of the American Heart Association. We were on that committee together, but I did not realize she was at Johns Hopkins and I did not know that she had gone to the Institute.

LENFANT: Yes. Her husband was offered a job at the NIH as Director of the Vaccine Center, and he wanted it very much. At that time Steve Epstein had left and clearly our clinical services were kind of floundering It was not going well at all. We thought of bringing her in as the chief of the clinical services working under Ed Korn, but she did not want any of that so what I did was to split the division. It is one division, but with two heads. The dynamics of all that is quite interesting, but she is superb, she is a good person. I do what I can to support her. I hope we can keep her happy.

FYE: Thinking back about the heart surgery program . I understand that the focus of it was on hypertrophic cardiomyopathy and the myectomy and also on valve replacement before you were there but when Glen Morrow got started. Of course, that was the era of prosthetic valves. I read something that you wrote in 1990 explaining the decision to close the heart surgery program in 1990. You emphasized that valve surgery had become a standard procedure and that cardiac surgery in general was a mature discipline and high quality cardiac surgery was widely available throughout the United States.

LENFANT: You did read my pieces! That is fantastic.

FYE: Yes. You made it clear, however, that the Institute would continue to fund extramural cardiac surgery research. How was that decision received? I can imagine that the people in the Institute who were in the heart surgery program were devastated or upset, but how did the larger community of cardiac surgery respond?

LENFANT: Very badly. The thoracic surgery community objected very much to that, and we made an agreement–I cannot remember the terms exactly–but I offered to the American Society of Thoracic Surgery–yes, that is how it is called–that we would support a fellowship program for a cardiac thoracic surgeon who would want to come to NIH to learn research principles and basic research. We made the arrangement that they could be there for one year, or two years, or two months here and two months somewhere else to keep their surgical skills. Nobody every applied for that. That was, in my view, the decline of thoracic surgery research as we knew it in the 1950s and in the 1960s. In fact, we now fund very few research grants in departments of surgery. I hope that if we can resurrect cardiac surgery at NIH, especially with Betsy Nabel, then that would re-energize cardiac surgery, that is, experimental surgery research in the country. But now we fund very little surgery research. I would say if we give ten grants out of 3500 to surgeons this is about it.

FYE: Do you know the term that has been used for the surgeons who pioneered open heart surgery? They were "physiological surgeons" because they were dealing with hemodynamics, they were trying to fix broken hemodynamics, and they had to think physiologically. Do you think that part of the reason that research in cardiac surgery has declined is that there really are limits to how many things you can fix and how many ways you can fix them?

LENFANT: Yes.

FYE: What things do you envision going forward . . .

LENFANT: Well, there are things which have occurred over the years, the Batista procedure, for example, and then another one is called the Dor procedure. It is a kind of a variation of the Batista procedure. The transmyocardial laser may be something, and, of course, there is the whole area of robotic surgery, then micro incisions and things of that sort. Also, in coronary bypass it went from using the saphenous vein to the mammary artery . . .

FYE: Internal mammary or internal thoracic artery.

LENFANT: Yes. But now they are using radial arteries, and that may be quite interesting. Actually, it may lead to new interesting research about which artery to choose. Of course, another issue where I think there is tremendous potential is in the treatment of aneurysms; I believe this will be a joint venture between cardiologists, molecular medicine, and surgery.

FYE: When you say aneurysms, do you mean of the aorta?

LENFANT: Yes.

FYE: Not of the left ventricle...

LENFANT: No.

FYE: But vascular aneurysms. . .

LENFANT: Yes, vascular aneurysms.

FYE: Right. I think it is interesting that you obviously do have a vision of cardiac surgery as a potential growth area in terms of research rather than the very narrow vision I put out there as a bunch of–I am reluctant to say this–plumbers that come to fix . . .

LENFANT: Yes. There are opportunities, but we have to develop a focus and then, of course, God knows what is going to happen with gene therapy. That is an open question right now. Let me put it this way. If thoracic surgeons and especially cardiovascular surgeons remain physiological surgeons then they will not go anywhere, but if they become molecular surgeons or that kind of surgeon . . .

FYE: That is the word I was thinking of when you started. I think you have maybe coined a term. That is interesting.

LENFANT: I think that they will find that there is a new domain for them.

FYE: Have you written about this?

LENFANT: No.

FYE: I would urge you to write about this. I am quite serious. In fact, I would urge you to put "molecular surgery" in the title, or "molecular surgeon," because I do not know if anybody has ever used that phrase but you really built up to that. It makes a great deal of sense the way you have articulated that, and it is also very symmetrical with what you explained earlier that you did almost twenty years ago at NHLBI to begin with, in terms of shifting the focus to that. I do not want to jump around too much here in chronology, but it is fascinating to go down these paths. Now let me see. I am still with your role as director, and there are going to be a variety of programs I will ask you about. Moving ahead into the 1990s, I will ask more or less the same sorts of questions but I will frame it a little differently. During this most recent decade, what aspect of your job did you enjoy the most?

LENFANT: I think the thing that has been the most important to me during the last, I would say, ten years has been to keep the institute a single entity. Today, there is a great deal of divisions and individual interests which, in philosophical terms you could say, is weighing the individual interests versus the societal interests. In an organization like ours which has so many interests, it is very difficult to keep it as a single organization. Yet I think it is absolutely critical for a very simple reason, it is that many research areas and some new disciplines and approaches permeate the entire institute. I think that for the economy of things we just cannot multiply each thing we do, so we have to focus on developing threads which are common to all the programs in the institute. This is very difficult to achieve.

Furthermore, the community has become more demanding in many ways. In the last four to five years a conflict with the epidemiology community, the population-based community, has become quite visible. But you can understand why, it is because the research has moved from the person to the organ to the cell and genes and molecules; yet if you are one of these investigators who want to look at people or tens of thousands of people you feel left out. So there are some kind of conflicts that are developing. We have seen many changes in the institute too, many personnel changes, and that has taken a good deal of my time. I personally think that if the institute can stay together, meaning that the heart group works with the lung group and the lung group works with the blood group, and that we all work together, the institute has the capacity of being a true leader at NIH, because this problem of division, of competition within, that I am talking about prevails everywhere at NIH.

In this regard I think that Varmus deserves lots of credit because he had a unifying vision which I personally certainly endorse. He probably wanted to do it too fast and too soon, but the vision is correct, I think. Then, when you look at the programs, we have seen the emergence of disciplines and approaches that you would never have been thinking about just before. Take pharmacogenetics for example, molecular epidemiology, trying to predict the evolution of disease on the basis of the patient's genetic profiles; these are very novel and unique pursuits. Actually I am thinking a lot about all those things because I have been asked to give a presentation on the future of cardiology in this century and that is the kind of thing I am thinking about.

FYE: I wish you well with that! I think it is challenging. I will just digress for a moment. I was asked by Sylvan Weinberg after my book on American cardiology was published to do an ACCEL interview on the future of cardiology, and on the telephone I told him, "I don't do the future, I do the past." But he is a very persuasive person, and kept assuring me he was sure it would be interesting and we could do it. We sat down then many months later in the ACCEL studio to frame the discussion and, finally, he concluded like me that I could not do the future! But from things you have been saying, obviously you are a visionary person and you can. Do you think part of that is from your philosophy background and your interest in that as a young person?

LENFANT: Yes, probably. Philosophically, I am really not interested in today. I am much more interested in where we are going. That is an area where the staff of the institute and I pass each other, we just do not communicate. They do not understand. When an application comes in or a big program is being discussed, they all jump up and down and say it is terrific, and I say, "Okay, where's that going to be in five years?" There is silence. Nobody wants to hear about it, and yet I am very interested in the impact on the budget and how it would relate to the other things which may emerge in the future. Maybe I am too philosophical for what I do!

FYE: Thinking again about the decade of the 1990s, maybe I am asking the same question with different words, but what are you most proud of in terms of your accomplishments at the Institute, and throughout the whole scientific community for that matter?

LENFANT: I think that what has been done, or what has happened during the last ten years, the 1990s as you put it, is that we have established changes in the approach to the Institute's research. Clearly, now the dominant force in the Institute, and I would almost say it is perhaps too much–we may have overshot the goal here–is the development of the molecular approaches; I believe they are very important, but in my mind they must remain just approaches, not the end. I think there may be a tendency in the Institute, certainly among my colleagues, and also outside the Institute, to view these new approaches almost as an end. You must be aware of the dispute between the Genome Institute and the private sector with regard to the mapping of the human genome. Do you know what I am talking about?

FYE: I know about it superficially. I certainly know what the project is.

LENFANT: Basically, the issue is that the Human Genome Institute is mapping the genome, and every week they put on the web the result of what has happened during the week. Now, there is at least one private company which does exactly the same thing and seems to be doing a little better than the Genome Institute. I must admit I am not competent to evaluate this but, from what I am hearing and seeing, I think that the private company has a better approach, a better technique. But they do not put anything on the web because they want to patent their work. It is not just patenting a gene, but they want to patent the utility, the function if you want of the genes they discover. That is quite interesting to me as this dispute is about the value of today versus the value of tomorrow. I think these are very interesting issues.

FYE: You have just described some of the things in the 1990s of which you are most proud and that you are pleased about in terms of your role at NIH or at NHLBI. Are there other things that you are especially proud of in terms of the 1990s?

LENFANT: I think it is very difficult to single out just one thing. The Institute is a very complex organization. I hope, and history will judge, that it is a successful organization. But I am not sure that I can single out just one thing saying that is driving the whole thing. It is the constellation of all these things that makes the organization work. So, in this regard, it is very difficult to answer your question. You have people who think that the only research that is good is breakthrough research. I do not. I think what is more important is sequential research, one little step at a time. Of course, everybody wants to have a breakthrough, but it just does not happen, and most of the time the big things which are happening, with the big splash, have only a very relative value.

FYE: You know that Julius Comroe wrote a two-volume book about the top ten advances in cardiovascular and pulmonary medicine and physiology, and part of his premise, if not his whole premise, was that so much of medicine and science, medicine in particular, develops in steps. It is stepwise development.

LENFANT: Yes. He wrote a book about it. I forget the title, but, anyway, the theme is that medicine is like climbing a mountain. I do not know if you know the book.

FYE: Yes, I do know the book. I think it has a white and blue dust wrapper, *Exploring the Heart*, or something like that.

LENFANT: *Exploring the Heart*, that is it. And that is the way I feel. It takes everybody tied together. So, in this regard, that makes it very difficult to say, "Which is the person that has been the most important or what is the thing that has been the most important?" What I view as the most important is the whole enterprise.

FYE: Now thinking back about the 1990s . . .

LENFANT: I think the best testimony–I do not think that the Institute can take credit for what you see here at the ACC meeting–but we have been a very strong determinant of the research which is reported here. I would be hard put to single out any specific thing that is responsible for the advances, but there are lots of little things which have contributed to this march toward progress.

FYE: Just to clarify when you say what you see here, we are sitting in a hotel in the context of the American College of Cardiology's 49th Annual Scientific Sessions with thousands of presentations of original research. I am sure a very significant percentage of those reports were supported at least in part by NHLBI grants.

LENFANT: Oh, yes. In many of the new medications and things that we hear about all the time, you could see our fingerprint someplace.

FYE: I think something that it is very difficult to overemphasize is the role of the NIH and NHLBI, in particular, in terms of cardiovascular research and pulmonary research and blood research. But the thing that I came to appreciate so much when I did the research for the book I wrote on American cardiology was that the creation of academic medicine and then its tremendous growth was all fueled by federal money, most of which came through the NIH, and, obviously, in terms of the specialties that come to this meeting, came through your institute. I should not be editorializing on this, but I feel so strongly that people do not understand or have no comprehension of how we got to where we are in this country with such an enormous biomedical research enterprise.

LENFANT: That is correct. Yes.

FYE: Do you want to expand on that from your perspective because you came to this country when it had a pretty small biomedical research community and now it has exploded. It must be at least an order of magnitude larger now.

LENFANT: I think the tremendous asset of this country is basically to have granted freedom of thinking and imagination and of acting to the academic community. I do not think looking at the population as a whole that Americans are smarter than the Russians or French or British or whatever. It is the system. It is just like bacteriology. You need to have a medium for the bacteria to grow! It is the same thing here. I think the environment allows that to happen, and it does work. And I think our role at the NIH, which is the object of considerable debate with some of my colleagues I have to say, is how do we balance our role versus just being the people who help others to express themselves.

FYE: Now, when you say your role, do you mean the intramural research versus the support . . .

LENFANT: No, I mean the extramural research. Many people in government have the feeling that they are in charge and that they should decide on this program or that program, and I do not totally believe in that. I think that we have to be very careful to let the people who are actually doing the work express their own ideas. That is the basis of NIH. I mean, the motto of NIH is to support investigator initiated research. And I think that is a very critical approach. But I would take it to an extreme; investigator initiated research may be impeded by a government agency or government employees, but it can also be impeded by an investigator's own colleagues as well. So many times I have seen investigators put down proposed research by others for what was obviously the wrong reason, and I think that is an evolution of our society.

FYE: It certainly does seem to be. Obvious is too strong a word because it reflects a bias I have, but it seems logical that an investigator or anyone for that matter will be more motivated to do something if it is their own interest, it is their idea, they have passion for it and they are interested in it rather than if they are given an assignment.

LENFANT: Sure.

FYE: That is another way of saying what you have said, and the role that you have articulated–that NIH has played–is allowing people largely to pursue their own interests, their own agendas. They have to fit within certain parameters in terms of the scope of research that is under your purview or of interest in the largest sense to the institute. But is that pretty much it?

LENFANT: Yes.

FYE: Another thing I wanted to ask you–it has nothing to do with the comment you made to me a few minutes ago about the future–but I do have a general question here. I just said, "Your background in clinical research included extensive involvement in cardiac research as well as pulmonary research. It seems unlikely that future directors will have that broad experience. What advice would you give to those persons who are charged with choosing your successor?" What should they look for?

LENFANT: I do not know that I can touch that one!

FYE: Well, they will not ever see or read this until a long time after they have made the choice probably . . .

LENFANT: It is an important question; a very, very important question. I feel very strongly that somebody who comes as an institute director to NIH, especially an institute like ours which has such a wide range of interests, must be very open-minded. I see that in other institutes when new directors are coming, they have definite ideas about what is good and what is bad, and I suppose that is why they are picked. I suppose you could ask, "Why did they pick you then?" But I see a real risk in this. The best demonstration of this is the time it took [Harold] Varmus to adapt to his job. I mean, when he came if you were not an expert in molecular genetics, you were nothing. I see this as being a very great risk for the NIH. So the first thing that I would say is that it is critical to take somebody who is open-minded and has a true realization of his or her responsibility. You just cannot say, "This is what I do, this is what I believe, and this is what we are going to do." I do not believe in that. And I think that whoever oversees that process should be very sensitive to that.

FYE: Now, I would ask you to contrast what you have just said about looking forward with what you told me earlier. I think there is a difference, but I would like you to see if you could tell me what the difference is. You said that when you became NHLBI Director in 1982 you had to go in and make some tough decisions and that you had some strong ideas about things. So I think, on the one hand, you are talking about an idealistic view of what would be optimal but then, on the other hand, you experience something that you have had to be more pragmatic about.

LENFANT: I think the answer to that is simple and again, others should attest to that or challenge me, but I wanted to be additive, not instead of.

FYE: Additive, rather than instead of.

LENFANT: Yes. I think that, admittedly with time, things must be replaced by others. That is the way it goes, but it should be a very progressive process and not like a sudden turn around. So what I am trying to say in answer to your question is that I think it is very important that people who occupy this position be very open-minded about what is going on. I would say to you what a purist would say, "This person really has no expertise in anything," which is what some people would say about me, but if you can compensate for the lack of expertise by being receptive to the ideas of others then I think that is okay.

FYE: I understand that.

LENFANT: I do not want my ideas to compete with the ideas of others, at least in this area of the management of the institute.

FYE: Talking about the management of the institute, you have served under a number of NIH directors.

LENFANT: Yes.

FYE: I wonder if you could say something about your relationship with the Office of the Director, in other words with the position. Just what is the relationship and how do you interact with that highest level of authority at the NIH. Then, maybe, you could digress and describe a little from your perspective the different directors under whom you have served.

LENFANT: I have some pretty definite ideas on that! My idea is that it is important for institute directors to have a close relationship with the Office of the NIH Director. I think that is very important. After all, my position is that I am not independent, I work at NIH. I am a component of NIH and therefore, I have to, and basically want to, be a good soldier. If I do not agree, I want to be able to say that I do not agree and why, but I recognize that somebody has to make a decision. That is all well and good, but it works only to the extent that the NIH Director is interested in that. It has to be a two-way street. Over the years what I have found is that not all NIH Directors are open to that kind of approach. There are NIH Directors whom I would see all the time and there are others whom I saw maybe once or twice a year. So, there are very different personalities. I think the latter level of interaction is bad, but if that is the way it is, that is the way it is. You have to accept it.

FYE: Could you go through each NIH Director, maybe sequentially, and give me just a thumbnail sketch of them from your perspective.

LENFANT: The first one was Marston

FYE: Robert Marston.

LENFANT: Yes. Who died this year, or sometime recently.

FYE: Very recently.

LENFANT: He was the director of the NIH when Cooper brought me here, and he was followed by...

FYE: Before we go on, maybe could you describe him a little for me, from your perspective.

LENFANT: He was pretty good, actually. He was certainly very supportive of the Institute. Now, admittedly, that was when I first came and I had relatively little contact with him. I did have more contact with the one who followed which was Bob Stone. Robert Stone.

FYE: Robert Stone

LENFANT: I forget what he came from, but I had more contact with him because he was very friendly with Ted Cooper. Again, I had a good social relationship with Ted Cooper and often after work–Cooper was living on the campus–I would go and have a drink in his place–I also knew his wife and his kids–and Bob Stone would come too. Bob Stone was an interesting fellow. He was not at NIH for a very long time but he was an interesting fellow. Then he was followed by Don Fredrickson, and Fredrickson and I had a very nice rapport. I knew him very well. Of course, Fredrickson was followed by Wyngaarden. In between, of course, there was an acting director for a short period of time. In this case it was [Thomas] Tom Malone, who was an African-American who became NIH Deputy Director under Fredrickson.

FYE: Is that Malone?

LENFANT: Yes. After that there was Jim Wyngarden, with whom I really had a very nice rapport. Even in the last two or three years, I still see him from time to time, but less since he went back to Durham. But, after he left the NIH, he lived in Washington, and we would see each other from time to time.

FYE: Let us stop with him for a moment. Could I ask you to describe–I know you mentioned that socially you were friends, too–your professional relationship with him. How would you get to meet with him? Would he set up meetings, would you ask to see him, or how would that work?

LENFANT: I would ask to see him, and he was very receptive. Then we traveled together. He is the only NIH Director with whom I traveled, actually. We went to South America together, and to Japan; we also went somewhere else in Europe. Yes, that was the time of the one hundredth anniversary of NIH. We went to Great Britain together. I had lots of contacts there, and we stayed together at the Royal Society of Medicine. And Jim, I was going to say was, but I am sure still is, an art collector and I do collect some art too. So we would talk about this carving or that carving or this thing or that painting or whatever. Sometimes we were discussing NIH–I do not know if you have heard about the program which is called the Merit Award?

FYE: I have heard the term.

LENFANT: Basically the Merit Award is that if you are distinguished and receive a grant with an excellent score, you may get the next five years of funding without competition. So, it is a ten-year award. Well, Jim and I designed that program. I wanted to do it, so I went to see him, and we went back and forth. Of course, he had to talk to all the other institute directors, but eventually it was done. So we were close. I mean, Jim was a terrific fellow. We traveled to Egypt together, too, and we went down the Nile together. In fact, he took care of me. I will always remember that. I was coming back from, I forget if it was Nairobi or Zimbabwe or whatever, and, believe it or not, it was very difficult to go from one African country to another African country in those days. So I flew from Zimbabwe or Nairobi, wherever I was, to Frankfurt, changed planes, and went back to Cairo. All that without a stop, but I probably ate something bad, and I had lost lots of water. I remember that, finally, when I left the plane in Cairo, there were hundreds of people going through security, and in this part of the world, people are not terribly orderly. In this chaos, I collapsed before going through security. I was taken to some emergency room and I remember so well that I was holding on to my bag because I did not want my bag to disappear! Eventually, I could talk to the physician there, and I told him I was a physician myself and I was going to a hotel. So they put me in a limousine, a fancy taxi, that took me to my hotel. When I was checking in, there was a wedding in that hotel–it was a very nice hotel in Cairo–and weddings in Cairo are no small affair. As I was checking in, I felt myself collapsing again, and I fell under the counter. Jim Wyngaarden, with his friend, was coming to see me, they knew I was arriving, and literally picked me up off the floor and took me to my room and put me to bed.!

FYE: Amazing.

LENFANT: Anyway, I did come around, and then we went down the Nile together.

FYE: It is not fun to get sick in another country.

LENFANT: Oh, no. That was the second time in my life that it happened to me. Another time was in Hong Kong when I was coming there from Thailand. I know I had eaten something bad in Thailand, and I was so sick. That was before I was at the NIH, actually. After Jim Wyngaarden came our friend Bernadine Healy and, in between, there was a very nice fellow actually as an acting director. But in came Bernadine Healy. She was a bit difficult. And then Varmus. So, I have known six NIH directors.

FYE: In terms of Bernadine Healy, with whom I trained and whom I do know, expand upon your comments.

LENFANT: You know she is now at the Red Cross, and she is turning the place upside down. The blood is leaking!

FYE: Great metaphor. How did you find it difficult? What were the dynamics that led to the frustration?

LENFANT: First of all, she was at NIH for what, about three years, something like that?

FYE: I think that is about right.

LENFANT: I think I saw her three times, that is all. It was impossible to see her.

FYE: Interesting, too, because, of course, her background is in cardiology so you would have thought that she would have had a special interest in NHLBI.

LENFANT: She was so purposely–although I did not know the reason–unpleasant with me. It is so interesting because when the negotiations were taking place for her to get the job, I was having my back problems. In three months I had two disks removed, one was an emergency one, which was quite an experience, I must say. The other one was planned, and I had it done at NIH in one of their protocols there. But, after that, I went back home and I was flat on my back. One day she called me–I knew her very well. I mean, she was in many of our programs–and she said, “Oh, I cannot wait. I hope I am going to get that job. I cannot wait to have it, and you and I are going to work together. We are going to do great things, and I hope you are . . .” Then, maybe one week later, I went back to work and she already had the job. From then on, it was as though I had spoken to a different person. Amazing behavior.

FYE: How was she received by the rest of NIH institute directors?

LENFANT: Very badly. I do not think there was anyone who really supported her very well. But I will tell you, Bruce, it was very difficult. I have been at NIH for a long time. I know many people there whom I have seen come and go, and come again and so forth. They all tell you things and it is very difficult to know if what they say is true. I can tell you lots of people seemed to be on good terms with her and, later, I heard that it was not true, it was terrible, and the converse was true. So I do not really know. My perception, again, it is a personal opinion, is that she was not well received at NIH. She certainly left a lot of anger in people, who were not happy with what she had done, mostly because they did not know why she was doing it.

FYE: So it was a communication question in part.

LENFANT: No, it was really her personality. She certainly wanted to establish herself as a great leader, and she would do it no matter what. But then, in contrast, [Richard] Dick Lewis, the former ACC president, who was in the same institution after she left NIH, though that she was walking on water. But, not at NIH. Then she was followed by [Harold] Varmus, who was another interesting person.

FYE: Yes.

LENFANT: I have known six NIH directors and probably four or five acting directors.

FYE: Now, the directors are political appointments.

LENFANT: Oh, yes.

FYE: So, how big a deal is it for directors of institutes such as yourself that, for example, after the election coming up you could have a new administration, and they conceivably could appoint a new director. In fact, I guess, you might expect that. In other words, how often has it been that when the party changed, for example . . .

LENFANT: Each time.

FYE: Each time it is a new appointment.

LENFANT: Yes.

FYE: I guess, it would not have to be. I mean, theoretically . . .

LENFANT: No, it does not have to be. Right now, after Varmus left, we hear that there are two or three candidates and that one of them may have been chosen. His name is going through the FBI security clearance and all that stuff. If he clears, which is certainly going to happen, then his name would be sent to the Senate for confirmation because any presidential appointment needs to be confirmed by the Senate. The issue is, is that going to happen?

FYE: Right. In the time frame because there are only ten months left in the present administration.

LENFANT: Sure. So we will see what happens there. My guess is that it is not going to happen.

FYE: So there will be an Acting Director until the next Administration.

LENFANT: Ruth Kirschstein is the Acting Director.

FYE: You are right, you mentioned that..

LENFANT: Yes.

FYE: Obviously you cannot change the process of political appointments, but, from your perspective, would it be better if the directorship were not a political appointment? In other words, for continuity. I am just speculating. I am not saying that this should be your answer, but have you sensed or do you sense among your colleagues as directors of other institutes a wish that the NIH was not vulnerable perhaps every four years to some new director who could conceivably change directions, and who certainly would be a different personality?

LENFANT: Yes, I think some of my colleagues would say it would be better. Personally, it does not bother me.

FYE: It is the way our government works, after all. We do that with the president every four years theoretically. As a government agency, I guess, you could imagine why people are comfortable with that model.

LENFANT: It does not bother me at all.

FYE: How much autonomy do you feel you have as director of NHLBI?

LENFANT: Lots.

FYE: Has it grown or shrunk during your 20-year tenure, do you think?

LENFANT: Probably shrunk a little bit, expressed in a different way. But I think the institute directors have lots of autonomy.

FYE: What does that mean, for example, in terms of the budget. It is an enormous budget. How much is your budget for NHLBI this year?

LENFANT: $2.1 billion.

FYE: An enormous amount of money!

LENFANT: Yes, that is a nice piece of change.

FYE: More money than probably the gross national product of half the countries on the earth.

LENFANT: Probably!

FYE: That is an enormous budget. Obviously, there is a huge staff inside and outside the institute that works on that budget, but how much input do you have? I do not imagine you micromanage $2.1 billion, but in terms of creating new programs now or shifting moneys toward a program or away . . .

LENFANT: Lots.

FYE: . . . from a program. You have a lot of that.

LENFANT: Yes. I do not mean to sound pompous, but I really control the budget. The institute directors control their budgets, actually they are responsible for them.

FYE: Who do you look to get help from in deciding what to do with that budget?

LENFANT: The division directors. But there are checks and balances. Everybody sees what I do, and then we have a National Advisory Council. They know what I do and they can see the expenditure of money, where it goes and so forth. But I can influence things. An institute director, at least in our institute, can influence the distribution of the budget very significantly, not to say completely. I think the trick–again, when I am gone, I am sure there will be lots of discussions, comparisons, and things like that about it–but the trick is to do it in an even-handed manner. I hope I have done that. Whether it is perceived that way as well, I do not know. That is the issue, but it is true that I really feel that my biggest responsibility is this money. My staff would tell you that I am very stingy and conservative, and I argue about spending five dollars! They do not understand that, but I do. I do argue about dollars, not just about millions.

FYE: Let me frame my question differently. Obviously, you do control or influence an enormous amount of money that influences an enormous number of lives, professional careers, and research projects. How much time do you have to spend responding to people that lobby you in one way or another to think about their project? I am not talking about asking for real favors but just saying in the way that everybody who goes to Congress to lobby them says, "Pay attention to my project." How much of that comes to you?

LENFANT: Lots. When you saw me this morning, I had been trying to go to tell the people that I may not be able to be at the convocation tomorrow since the exhibit had opened! That is why I do not show my badge. I hide it . . .

FYE: So they catch you . . .

LENFANT: But lots of people know me, and it took me at least an hour to get out of that place!

FYE: Let me ask you this. When you come to one of these major national meetings, how much of your waking time is spent responding to people who are trying to influence you?

LENFANT: Nearly all of my time.

FYE: Really. That is very interesting.

LENFANT: But they do not try to influence me. Well, I guess, it is for them and for their program, but I listen to them, and I try to explain what the issue is. Let me see, in February, I was in Abu Dhabi, in the Middle East. It is on the eastern side of Saudi Arabia. It is part of the United Arab Emirates. I was there–it was a meeting on high blood pressure–and there was a man from Los Angeles over there, who really cornered me, and I said, "Maybe you should think about a clinical trial." Then, I said, "Why don't you ask for these people to get over there and we'll try to meet." Well, he cornered me again this morning to explain to me that he could not get all the people together here [at the ACC meeting] but that he had set up a meeting with my secretary and that they would all come to see me in April [in Bethesda].

FYE: Well, if you ever would like to use it as an excuse that you have to spend time with Bruce Fye being interviewed! Obviously, you have to spend a good deal of time being familiar with the programs that you sponsor and that you support. There must be literally thousands of them.

LENFANT: I think I know them very well.

FYE: Or at least hundreds.

LENFANT: Yes. We have lots. Just for the research grants that we have, all together we serve approximately 10,000 investigators.

FYE: Investigators?

LENFANT: Yes.

FYE: Would those be just principal investigators?

LENFANT: No.

FYE: Each would have a grant or they would be part of grants.

LENFANT: Yes. We have approximately 3200, 3300 research grants, but we have a large number of centers, we have clinical trials, we have training grants, and so there are lots of people to know. Sometimes when a name pops out, the name does not make it click, but if I am given more information, I eventually can locate pretty much everybody. Well, it is the institutional memory if you want.

FYE: How true. And how important as well. Let me see. I have some changes in my questions here. I think we have talked about the name changes of the institute already. This is just a series of questions that relate to the mission of the NHLBI.

LENFANT: I probably talk too much?

FYE: No, you do not talk too much at all. We have to figure out how to make time tomorrow somewhere so you can talk more! But this question is about the NHLBI research mission, and I am sure I got this from one of the institute's publications or perhaps one of your publications. But it indicates that the research mission can be divided into three components: one of knowledge acquisition, two of knowledge validation, and three of knowledge transfer.

LENFANT: Correct.

FYE: I wonder if you could comment on those three components, broadly from your perspective.

LENFANT: I think that all three are very important. Unfortunately, for most of the people only the first two steps are what they are thinking about. I think that the translation or the transfer of knowledge is a very important component of our activities. But it is very difficult. It is very real money, and that may be the reason why nobody paid attention to it, but it is a very important thing. For the rest of the day I could give you examples of things that you know because you are an expert. Good research outcomes that should be used but are not. That is, basically, I think, maybe because of a lack of awareness or a lack of interest. I do not know.

Yesterday, when I was waiting to get into the reception, I was talking to someone about this very issue, because I was invited just before Christmas to go to a meeting where I had been asked to talk about NHLBI's mission and goals. I speak a lot about that because something that interests me is the widening gap between what we know and what we actually apply. I think it is a tragedy to see that. But that was my story during the talk just before Christmas. Yesterday, I met the fellow who had invited me and he said to me, "We are going to invite you again this year because we thought that your presentation was quite interesting and you gave a fine perspective and that is what we are used to hearing." I said, "But the problem is real and something has got to be done." We talked about it and, finally, this fellow admitted that it cannot be solved unless all the people who are here [at the ACC meeting] pay attention to the problem, and in order to do that look a little farther than just their experiment or their institution.

So the translational aspect of what we do is, I think, very important but so neglected. The two other components, they come very easily. There is a bit of discussion between the truly basic research or applied research, validation and things of that sort. People argue about that, but eventually it gets done and it gets done well. The issue is to go from the validation to generalization and application.

FYE: In terms of knowledge transfer, as you mentioned, there is a gap between discovery and recognition of approaches or treatments and their application in practice. What do you see as some ways to improve that?

LENFANT: The will to do it. I feel very strongly about that. It is not deliberate neglect, but today is a world of plenty. I mean, it is like going to the candy store, there are so many things that you can do which are exciting. I admit that the translational aspect of all that is not the most glamorous thing to do. You can work in the laboratory, and if you are lucky you can make a significant discovery during the week. Today, that is the way it is, and that is what people want to do, but it takes, I believe, the experts we have to get into the translational aspect and make it happen.

FYE: What do you think about guidelines as a mechanism of encouraging people?

LENFANT: I think that they are very powerful. The problem with guidelines is that often people are mixed up about the difference between guidelines and prescriptions. Guidelines are not a prescription. Guidelines are what they are. A guideline is to provide what you could call a smorgasboard to choose from, or you could say it is the landscape in an area but in no way does it eliminate the physician-to-patient relationship that leads to the actual prescription. The fact is that in many aspects of medicine there are an infinite number of variations depending on the patient himself. And guidelines do not do that you see. I am a strong supporter of guidelines, but I recognize their deficiencies. The problem with guidelines is that many people think that it is like the Bible, it is like the law, and then, of course, they want to follow them. But that is because often we do not present the guidelines very well.

FYE: It is a pretty new phenomenon.

LENFANT: Yes.

FYE: Guidelines, right.

LENFANT: Yes. But I think it is a useful one. It has tremendous value and potential provided that it is utilized appropriately.

FYE: What other ways do you think the process of application could be enhanced? In other words, so that you could narrow the gap between knowledge that exists and knowledge that is applied?

LENFANT: I think in the absence of almost a societal turn-around or a different payment system or whatever, it is going to be very difficult to do it. Very, very difficult. I have a fairly pessimistic view of that, but, nonetheless, I think we have to try it.

FYE: Are you thinking of things such as beta blocker usage after myocardial infarction, that sort of thing?

LENFANT: Sure. And that is the worst example that I can give you. We could also talk about the cholesterol lowering or blood pressure control. So it may very well be that one of the barriers is the cost. I do not know what the extent or the magnitude of the problem to pay for that is. There are a lot of people who can pay and are prepared to pay, but it is not done, the subjects are not advised appropriately, and they are not advised because it takes time to explain to somebody the need to do these things. So there are lots of competing demands.

FYE: I think we should probably let you go and give you a chance to get to your next meeting. Thank you so much for this continued discussion.

FYE: As I mentioned, we are going to start [our discussion] around 1970. In 1970, you were appointed Director of the Division of Lung Diseases at the National Heart and Lung Institute and you held that position until 1980.

LENFANT: Correct.

FYE: Who appointed you Director of the Division of Lung Diseases?

LENFANT: It was [Theodore] Ted Cooper, who was the Director of the Institute at that time.

FYE: Tell me about the dynamics of how that appointment came to be.

LENFANT: I thought we had spoken about that. When the Heart Institute was renamed the Heart and Lung Institute, Cooper sent a letter to lots of people in the country to ask what should be done, what was needed, and so on. I got one such letter and I thought I told you that Hugh Smith and I worked on [a reply to] that letter. That started the whole thing in the sense that Ted Cooper liked our reply, he invited me to come to NIH, and eventually he asked me to come to get the thing going. I came on a leave of absence of two years and I am still there!

FYE: When you came as Director of the Division of Lung Diseases, what were your main responsibilities?

LENFANT: Basically, it was to get something done, to give a focus. It is interesting. Let me reflect on that from now almost 30 years of experience. The community out there, they want something. They want an institute for this, an institute for that, and then, what they want is a focus, a place that if you wanted a map of NIH they can put their finger on it and say that is where it is. Basically, that is a little bit what happened. Do not believe that billions came in to start a lung program. I was becoming the focus that people could call or talk to, and little by little we started a few new activities, and that is what happened. My responsibilities were to generate activities, to make sure that the world out there would know that, yes, there was something [happening].

FYE: Thinking about the first years at Bethesda, did you develop an intramural research program?

LENFANT: I forgot if it came the first or the second year, but, anyway, it came pretty soon. We opened an intramural program. There was a fellow, who, I believe, started in the Allergy Institute, and then he talked to me, and we decided to create something so eventually a pulmonary branch was started. That fellow was there for maybe two or three years, something like that, and he left. Then, during that time, the decision had been made to send somebody who was a very smart guy, I am sure you know his name, his name is [Ronald] Ron Crystal. Ron Crystal was a hematologist. His interest was in blood disease, mostly the molecular aspect of blood disease. He had been a postdoc under [W.] French Anderson. I do not know if that name rings a bell, but French Anderson is a man who is in the world of expression gene therapy. He is the one who really developed the concept of gene therapy, and he was in our Institute. He was the head of the branch or laboratory or whatever of molecular hematology. And Ron Crystal was one of his postdoctoral or whatever research associates. While we developed the branch with the first man, Ron Crystal was sent to the Cardiovascular Research Institute in San Francisco to learn something about the lung which he did not know.

FYE: Julius Comroe was there?

LENFANT: Yes, with Julius Comroe. Ron went there and he spent two years, something like that, and when the first fellow left, then Ron Crystal was appointed to be in the branch. Shortly after that I gave up my responsibility there and Ron Crystal took over.

FYE: The lung branch?

LENFANT: Yes. Intramural.

FYE: Intramural research on pulmonary matters.

LENFANT: Yes.

FYE: What was the original focus of research, what was his focus?.

LENFANT: During the [term of the] first man, it was not a very good focus. It was very diffused, and certainly not very innovative. When Crystal came, he came with his background of molecular approaches that he had gotten from French Anderson and in the blood area, and he immediately started implementing that. His first focus--I am not sure I can remember all that--but I think it was probably in alpha fetal trypsin deficiency, which in those days was the big thing.

FYE: For emphysema, but their relationship...

LENFANT: Yes. He also focused on idiopathic pulmonary fibrosis, trying to find the molecular aspect of that, and then he got involved in sarcoidosis. Ron Crystal is a very smart guy. The problem that he had was that he had kind of a hit and run approach to things. He would touch or start something in lots of areas but he would not persevere. He would jump from here to there all the time, and that brought lots of criticisms of him. I personally think that he did not deserve all the criticisms that he received, but all that was a little bit compounded by his personality which did not help to attenuate the intensity of the criticisms.

FYE: How did you make the Institute known? How did you publicize your new institute to the academic community, to the scientific community, and around the country?

LENFANT: The name of the Institute was changed and there were more people. You have all these people who are always watchers of NIH, and they know what is going on there.

FYE: It is hard to hide it, I guess, if suddenly the name lung winds up in the name of the Institute.

LENFANT: Yes, sure. That was no problem. But, in addition, I was not an unknown quantity in that field. In those days, I was already [known]. . . I was a member of the Young Turks [American Society for Clinical Investigation], I was a member of the Old Turks [Association of American Physicians], and I knew a lot of people. I was pretty active. I had, I think, a good research program. In those days, it was a good research program. Today, it would be a lousy one, but that was what it was in those days.

FYE: I know this occurred just before you got to the Institute, but what were the dynamics of the name change, and the fact that a lung branch was developed. You have mentioned several times that Ted Cooper was the main pusher behind that but who were some of the other players? Where was his base of support?

LENFANT: I think it was the [American] Lung Association and the American Thoracic Society. In those days, these two groups were very friendly. Now they are divorced, but in those days they were very friendly, and they were the movers and shakers in the political arena. They wanted to have a lung institute and Cooper took the position, which I can only support 100 percent, that that was nonsense. There was no reason to have a lung institute when we had a heart institute. The two should be coupled. So it is very interesting. That still happens actually. Groups out there want to have a focus at the NIH, but they do not know exactly what they want besides that. They want a name, and, as I said, a name on the map. It does not matter where it is, what it does and so forth, it is the name.

FYE: Was there much resistance from the heart community that the National Heart Institute was going suddenly to have a sibling or another entity as part of it?

LENFANT: Yes, I think there was. In 1973 or 1972 the National Cancer Act was passed, and Cooper, who was a real political activist, actually he was very astute, said, "Well, let's develop a heart and lung act." He almost succeeded in having something equivalent to what the cancer institute had. Now, you have to think about how it was in those days. There was the hierarchy in which a distinct entity would sit in that level of government. You had the departments, and under the departments there were agencies, under agencies there were bureaus, under bureaus there were divisions, and then branches. Okay, all the institutes at the NIH, which was a misnomer really, were divisions. So the NIH was the agency and then you had the divisions. There was no one at the status of a bureau. The National Cancer Act elevated the cancer division to the status of bureau.

FYE: Even above the level of the NIH?

LENFANT: No. Again, the way it worked. You have the departments, okay? [draws a diagram] Under the departments you have agencies, and in our case the NIH is an agency. Under the agencies you have bureaus and under the bureaus you have divisions. At NIH, here was NCI and the National Heart and Lung Institute. There was no one at the level of bureaus. What the Act did was to elevate NCI to the status of a bureau. So Cooper was very anxious to do the same thing for NHI. What happened was that it gave [NCI] two or, I guess, three things. One was a relative independence, two, a budget bypass authority, and three, the director of the Cancer Institute was a presidential appointment.

FYE: So all of the advantages you have just listed were the advantages for the National Cancer Institute after the National Cancer Act.

LENFANT: Correct. Cooper wanted to have the same thing, but what happened was that [Richard] Nixon, who was the President at that time, had his trouble, and so the elevation of the Heart and Lung Institute to the level of a bureau was only partially accomplished in the sense that it became a bureau, but it did not get the budget bypass and the director was not a presidential appointment.

FYE: It advanced in some respects but it clearly did not have the autonomy or the ability to control its destiny that the National Cancer Institute had.

LENFANT: Correct. You have it.

FYE: Tell me how would it be an advantage or disadvantage to have the director be a presidential appointee? That was viewed as an advantage, I sense.

LENFANT: What it really meant was that the budget of the Institute instead of going up this way would go directly to the OMB [Office of Management and Budget] and make it less likely to political manipulations and things of that sort.

FYE: So rather than have to go up the chain--you have just drawn this diagram for the National Heart and Lung Institute--it had to go up through the NIH and through the Department, at that time of Health, Education and Welfare, to get any budget authority from the government.

LENFANT: And then compete with all the other institutes.

FYE: Right, but by going directly to the OMB . . .

LENFANT: You eliminate the competition.

FYE: Right, so Cancer had a real distinct advantage.

LENFANT: Correct.

FYE: This must have caused a great deal of tension among the institutes, I would think.

LENFANT: Oh, yes. In fact, after the Heart and Lung Institute became a bureau and it was realized that it became a bureau but without the privileges of being a bureau, then there was lots of agitation among all the other institutes and their constituent groups and that resulted in all the institutes becoming bureaus.

FYE: I can identify very distinctly with that phenomenon. There is a certain analogy in terms of departments of medicine and divisions of medicine; how some have jockeyed to become departments and the tension that that has caused with others. This happens anytime there are people that perceive their peers getting an advantage. So how long was this a problem in terms of this sort of political jockeying at the NIH?

LENFANT: It took a couple of years. But to come back to your original question which was-- all of a sudden it skipped my mind there--you were asking about . . .

FYE: How the the heart and lung institute was received . . .

LENFANT: Yes. That is right. With their competition. You see as part of the development of the National Heart and Lung Act, the [American] Heart Association became agitated about all that. So the bottom line was that the Act stated that the budget of the institute would be such that at least 15 percent would be for the lung and 15 percent for the blood. During all the agitation about the lung, the blood came into the picture and got its name attached to that [institute].

FYE: Now, let me understand this. Do you mean !5 percent of the total NIH budget or just of the NHLBI budget?

LENFANT: NHLBI, that is right.

FYE: So really 70 percent [of NHLBI's budget] at that point was still theoretically targeted for heart and 30 percent was split between lung and blood.

LENFANT: Exactly. You see, the heart was very concerned and so that got the lung and the blood very concerned. They fought very hard to assure that it would be at least 30 percent; in other words, it was not a ceiling, it was a floor. So that is what happened, and, of course, over the years, for a variety of reasons, this floor was kind of forgotten and the budget for the lung and the blood increased quite significantly. Now, today, the split is probably something like 60 and 20 and 20, something like that.

FYE: So, gradually, the amount of budgeting that went toward lung and blood increased but never to the level of heart. Heart remains very dominant.

LENFANT: Yes, but one thing that you have to understand is that so much of the research which is supported in all three entities is germane to all of them. The best example that I can think of, actually, is the one that I use each time that I have an argument with the heart community. Look at thrombosis. You see thrombosis is part of the blood budget, and I tell the heart people, "What are you talking about? Basically you own half of the blood division."

FYE: It is fascinating, too, as I reflect back on your early career with the heart-lung machine and the oxygenators how you obviously were correlating . . .

LENFANT: Yes, sure.

FYE: . . . and coordinating and working in all three areas because they were all vitally important to the research that you were working on. There is a certain symmetry of your career and the structure of the institute. Tell me a little more. I am sure there must lots of interesting stories about this jockeying for position among the lung advocates, the heart advocates, and the blood advocates, both within NIH and externally. Could you expand on that?

LENFANT: What you have to understand is that, yes, there was lots of jockeying but at least there was an apparent protection of the heart community owing to the fact that Cooper was clearly recognized in the heart community. Basically, he was a pharmacologist, he was not really a cardiologist. But then he was followed by [Robert] Bob Levy. And when Bob Levy left, Peter Frommer... Do you know Peter Frommer?

FYE: Yes. I saw him today, in fact.

LENFANT: Peter Frommer was clearly viewed by the heart community as the front runner but, in fact, he did not become the director. I became the director. But I can tell you that this organization, the [American] College of Cardiology, campaigned very hard for Frommer and, in fact, did whatever they could to keep me from becoming the director of the institute.

FYE: Was this in 1972? Oh, no, this was 1980. We have moved up to 1980.

LENFANT: Yes, the College of Cardiology was--I suspect that it was in great part because Frommer was campaigning here to get support from the College of Cardiology.

FYE: I do not know whether I should fast forward to this point or try to remember to come back to it, but, I guess, Peter was heavily involved in the CASS study [Coronary Artery Surgery Study] and that would have been started at that time. I am thinking that he would have been known to the cardiology community...

LENFANT: No, the CASS was not so [early]...

FYE: I know it started when I was still a fellow at Hopkins, so it started in about 1975, I think.

LENFANT: Yes, the CASS was started, but Peter had been very much involved also with the myocardial infarction research units . . .

FYE: The MIRUs.

LENFANT: Yes.

FYE: So, he was well known to the heart community?

LENFANT: Oh, yes. He was running that.

FYE: So he was viewed as an advocate for them?

LENFANT: Sure. And there, you see, I was the enemy really!

FYE: How did you win? What were the forces that came into play?

LENFANT: Well, don't ask me. I do not know.

FYE: Just from your perspective. What was it that you think led to your choice?

LENFANT: First of all, at that time, my curriculum vitae was somewhat different than that of Peter Frommer. I had been a full professor in a good university, and I had built up an extensive bibliography. I had been at the Young Turks and the Old Turks, and Frommer had none of that. I do not know what he did during the interview process. I was not there. All I can tell you is that I knew [James] Jim Wyngaarden, who was the one who selected me, well because he had offered me a position twice when he was Professor of Medicine at Duke.

FYE: I do not think we ever talked about that.

LENFANT: So, I knew Jim, and, in fact, it was very interesting. After Jim appointed me, the first thing that he said to me was that now that you are appointed, I am going to give you all these letters I got from the College of Cardiology and I want you to get them off my back!

FYE: How long did it take for you to get them off his back? How long did it take for the ACC and other heart oriented groups and people to accept you into their community..

LENFANT: You know, we drafted letters. Then the ACC president at that time, I know exactly who she was, it was a woman, Suzanne Knoebel, and she was dead against me. She really was!

FYE: Interesting.

LENFANT: Very much so. But I got to know [William] Bill Nelligan, and Bill and I got along very well. Also, I mean, my interpretation is that Bill and some of the people who were there--I cannot remember, there was a fellow, the editor of your journal, Dack...

FYE: Simon Dack.

LENFANT: Simon Dack was [there]. . . I met with them and they were all rather on the conservative side of things and I am fairly conservative myself, so we kind of hit it right! Yes, that was quite interesting. No, Bill and I got along very well.

FYE: He has a reputation, and, of course, I have come to know him in more recent times, as being an amazing facilitator. He has an amazing ability to mend wounds and bring people together. Certainly that is the idea with the American Heart Association and the College and such. So you sense some of that yourself.

LENFANT: Oh yes. No, that was quite an interesting time.

FYE: So, basically, you were not able, . . . I should not answer the question before I ask it. It strikes me that you certainly did have qualifications in the area of cardiology because it just depends on how you look at the research you were doing on the heart and lung in the 1950s. I mean, you could say well he is a pulmonary physiologist . . .

LENFANT: But nobody knew that [about my heart work?], you see.

FYE: I see.

LENFANT: Nobody knew that really.

FYE: Because your identity in the United States had all been focused around pulmonary physiology. Okay.

LENFANT: Yes.

FYE: All right. Going back to the earliest years at the lung division, how did you develop the extramural programs? How did that work in terms of getting [started]?

LENFANT: We developed lots of programs that had never existed before. For example, the specialized centers of research [SCOR] were developed at that time. To use the jargon of NIH, NIH now issues lots of requests for applications [RFAs]. The very first request for applications at NIH was issued by me. Also, we created a number of training programs which were kind of a novelty. We started a program which was called the pulmonary academic award, we started a program which was the training program with... How would you call it? I cannot remember how it was called. We started a number of programs and that is how I got acquainted with people in your place [the Marshfield Clinic], actually. There was that fellow Wenzel,

FYE: Fritz Wenzel.

LENFANT: And I remember a man . . .

FYE: from the Marshfield Clinic? Richard Sautter probably or Dean Emanuel.

LENFANT: Dean Emanuel, yes. We also started a program that actually was adopted NIH-wide afterwards which was the young pulmonary investigator award or something like that. Then it became the young investigator award of NIH, and eventually it became the first investigator research scientist something or another. So, we started a number of new programs.

FYE: Of course, the attraction for the academics or the researchers was that these programs carried money with them . . .

LENFANT: Yes, sure.

FYE: They were grants and so it enabled them to pursue their research interests.

LENFANT: Yes. That is probably what got the Heart Association and, I suspect, the College of Cardiology to be a little on edge wondering what was going on there. All that money going into the pulmonary thing. It was not all that much money; actually, it was a very modest amount of money. But you know the perception of people.

FYE: When you wanted to start new programs in the early years, the 1970s--some of the programs you just mentioned, the innovations–how much bureaucracy did you have to go through? Did they have to be approved by the NIH director . . .

LENFANT: Very little.

FYE: . . . or did you have a budget and you just . . .

LENFANT: No, Cooper had to approve that. He was very supportive. I mean, he really had no choice, he was the one who started the whole thing.

FYE: But, for each of the programs you mentioned, would you have to develop a budget for it?

LENFANT: We developed the idea, the concepts, and then, I would go to Cooper and say, "This is what we want to do and can we get the money?" and there was a little bit of back and forth on how much money. But Cooper was very supportive.

FYE: Individuals like that make a huge difference, don't they?

LENFANT: Yes, that is what it takes. Somebody who wants to do it.

FYE: Let me stay on track here [in my questions] rather than going here and there. One of the things I wanted to ask you about was what clinical facilities were available at NIH for lung diseases? What did you develop in the area of the Clinical Center for a clinical program in lung diseases?

LENFANT: Crystal is the one who started the clinical components, really--I forget again if it was the alpha-1 anti-trypsin or the idiopathic pulmonary fibrosis or whatever. Then he got into cystic fibrosis, too; that is where he did the first cases of gene therapy at NIH in cystic fibrosis. We had a ward for the Institute and, basically, if you had a protocol that was approved then you had access to the facilities.

FYE: What would be the mechanics of getting patients to the institute on that ward?

LENFANT: Contacts. Contacts.

FYE: Contacts. So you would know clinicians or clinical investigators around the country and they would contact your office or Dr. Crystal's office.

LENFANT: Yes.

FYE: And is it Crystal? Is it just like crystal?

LENFANT: Yes. He is talking here today.

FYE: What a small world.

LENFANT: Now, he does some gene therapy for angiogenesis.

FYE: Angiogenesis.

LENFANT: Yes. I saw his name, and I do not know if he is the one who is going to give the paper. I suspect it will be him but, anyway, he is talking. He is a big competitor of [Robert] Eisner. Do you know him?

FYE: Eisner. Yes.

LENFANT: Is it Eisner? Most people say Eisner.

FYE: You say it right because I don't know him.

LENFANT: I do not know how it is pronounced.

FYE: Tell me a little bit about the clinical facilities and the Clinical Center. I know that was not your focus but what was it like? About how many beds and who staffed the ward.

LENFANT: I think the Institute had maybe 40-50 beds, 60 beds, something like that.

FYE: For heart, lung, and then eventually blood.

LENFANT: Yes, and metabolic diseases too. We had beds on two floors. I must say I do not know how many beds we really had, but patients were admitted on the protocol. I mean, we needed to have a protocol to bring patients. We cannot take somebody else's [patients?].

FYE: Right, they had to fit into the clinical research that the Institute would support.

LENFANT: Yes.

FYE: Now I am going to go ahead in time and turn to 1982. In that year, in July, you were appointed director of the National Heart and Lung Institute. It was the National Heart and Lung Institute at that point, and not Blood yet?

LENFANT: It was all three. Blood was in 1974.

FYE: 1974, okay. I have that elsewhere!

LENFANT: Yes, I think it is 1974.

FYE: Okay, by 1976, certainly.

LENFANT: It may be 1976. If you want the absolute date, we can find it somewhere.

FYE: It was 1976. I am sorry for this confusion. But earlier in . . .

LENFANT: That is right. They [Blood] came at the time of the re-authorization of the Act.

FYE: 1972 was the act.

LENFANT: I think, for the heart institute, it was in 1973. Cancer was 1972, and we were 1973. The authorization is always for three years, so it has to be, yes. I mean, we could find it . . .

FYE: I know the Act was passed in 1972,

LENFANT: Okay. So, it may be that the re-authorization lapsed for one year which happened all the time, but Blood managed to get their name in the Institute at the time of re-authorization, so they knew [?] the authorization . . .

FYE: Maybe we should talk a little about that. We talked about the dynamics of how lung became a sibling and now there is another little sibling, blood, coming along.

LENFANT: Yes.

FYE: Who pushed that and how did that happen?

LENFANT: The blood community had a lobbyist in Washington who was very effective. His name was John Grugenhof. He is still around except that he no longer works with the blood. He was a former staffer of some member of Congress. He was a very effective guy. So, anyway, the blood name got in there.

FYE: And all of this, of course, is a political process.

LENFANT: Oh, sure.

FYE: That is how these names [of institutes] are changed.

LENFANT: Sure.

FYE: But was there much stimulus from within the NIH to do this or was it mainly from the blood community and this lobbyist?

LENFANT: It came from the fact that even when the institute was only the Heart Institute, we were supporting lots of blood research on thrombosis. For example, the urokinase and streptokinase trials of the late 1960s were done in the Institute. In fact, there was a branch which was called the thrombosis branch in those days. When I came to the Institute, there was an established thrombosis program. And so, by then, there was quite a bit of work that that had generated. There was also a blood [division]. . . In fact, when the Institute's Act passed in 1972, I believe that the blood division was created at that time. And because there was a blood division, the blood community demanded to have its name put in the name of the institute. I think that is how it worked. Yes.

FYE: Did that trigger the same response from the heart community? Then I assume the lung community had a response, although they had only recently joined the club..

LENFANT: I think there was always a bit of anxiety. Of course, now today, 20 years later, it is completely irrelevant because every month somebody wants to have a new institute, but in those days it was much more [controversial?]. . . In fact, I think that what happened to the heart institute was precedent setting because after all that [there were] more and more splits, rather like the stock market, divestiture or whatever you want to call it.

FYE: So 1982 was a pretty dynamic year in Washington in terms of health and research. You mentioned that was the year you were appointed Director of the National Heart, Lung, and Blood Institute. C. Everett Koop was sworn in as Surgeon General that year and James Wyngaarden was sworn in as NIH Director.

LENFANT: Yes, he was. He was early in . . .

FYE: January, I think.

LENFANT: January, that is right.

FYE: Just a few months before you, as you were appointed in July.

LENFANT: Yes.

FYE: I guess from what you said earlier in terms of James Wyngaarden's role in your appointment if it had not been him [as Director], it might have been different.

LENFANT: I certainly do not want to imply that Jim Wyngaarden appointed me just because he knew me. I mean, I do not want to be boastful in saying this, but I really think that my qualifications or experience far exceeded that of Peter Frommer.

FYE: Right. Were there other candidates as well that you recall or were you two more or less the short list at the end of casual inquiries?

LENFANT: Yes, there was another internal candidate who at that time was the Director of the Division of Heart and Vascular Disease. It was a woman by the name of Barbara Packard.

FYE: Oh, yes, Barbara Packard.

LENFANT: But she too had really had no [university experience] . . . I do not want to have to say this but that is what I think was unique about me, because, at that time, you could count on your fingers to identify the people who had had a non-NIH university experience. There were very few at NIH, very, very few. I was one of the rare specimens who had spent 10 or 11 years in academia and advanced through the ranks from instructor to full professor of medicine, biophysics and physiology, in a good university. And in those days there were very few... In fact, Cooper had been in the university, he was professor of pharmacology and chair of pharmacology, actually, at the University of New Mexico in Albuquerque, but I think that he and I were probably two rare exceptions.

FYE: This raises an interesting question in terms of why it should have been that there were so few people from outside at NIH. Is it that there were barriers to getting into NIH or is it that there was not as much attraction for outside physicians to go to NIH?

LENFANT: I think it is the latter. Remember that the end of the 1960s and 1970s were during the time when there was lots of money going to NIH. Do you know that priority scores were 300 or 400 and being paid.

FYE: I am sorry, what did?

LENFANT: The priority scores for the grants. The grants were paid on [those scores]. . . Today you would not even think of paying grants with such a priority score.

FYE: I see.

LENFANT: So I believe that there was lots of money, it was the time when many people who had been at NIH before had gone to the universities, had become the competitor of NIH and then . . .

FYE: Part of it, I guess, is that NIH is a federal program, and it is bound basically by federal salaries is what I am trying to say in some fashion.

LENFANT: Yes, sure. There was that too.

FYE: I mean that you are limited in terms of the government assignment of different government ranks or . . .

LENFANT: Although, at that time, the differences were not as great as they are now or what they were a few years ago. I think the great differential in salaries came during the late 1970s and the 1980s.

FYE: That is when the difference arose?

LENFANT: When I went to NIH, I took a small pay cut but it was not a very significant one.

FYE: I am thinking now again of when you were chosen to be Director in 1982. I imagine that you were asked to describe your vision of NHLBI. Could you recall what your vision was?

LENFANT: Oh, yes. That was really [the time of] the emergence of molecular medicine, and I was appalled to see . . . you see my predecessor was Bob Levy, and Bob Levy, who was, in fact, a very good person, had a very narrow focus of medicine which was lipids. If it was not lipids, it was nothing.

FYE: He saw the world through a lipid!

LENFANT: Sure. And Frommer's vision was really hemodynamics and ejection fractions and things of that sort. I thought that was a very narrow view of the field. I thought that it was time to bring molecular medicine into the Institute, and if you look at the history of the programs, I think that you will see the emergence of molecular medicine.

Side B:

FYE: So, just before the tape turned, you were explaining that your vision was based on and framed around molecular biology.

LENFANT: Yes. To bring molecular [approaches], new approaches, to move the field to the cellular level other than just the physiology. I think that is a fair assessment, and very quickly we put into place a number of programs to implement that.

FYE: Now my perception is that cardiology lagged in terms of moving into the molecular biology era.

LENFANT: Certainly, behind the blood. No . . . I mean, the blood is really the precursor of molecular medicine.

FYE: I am thinking of sickle cell anemia with Linus Pauling . . .

LENFANT: Yes, sure.

FYE: I think that was in 1948.

LENFANT: That is right. And then the lung also moved into that. But not the heart. If you look at the heart at the beginning of 1980, it was all hemodynamics and basically applying physiological techniques.

FYE: This is easily documented, what you are describing now. Why do you think that is? Why is it that blood and lung were so far ahead?

LENFANT: I do not know. Maybe because cardiologists were not trained into that.

FYE: I would wonder if part of it is because there was so much emphasis in cardiology at that time on what might be called macro therapies, in other words, ischemic heart disease . . .

LENFANT: Sure.

FYE: . . . although thrombolytics were a big thing and that is not macro. That is molecular biology. But I am thinking of angioplasty and different things . . .

LENFANT: Well, angioplasty started just at the end of the 1970s, 1978 or 1979, but that was still hemodynamics.

FYE: Exactly.

LENFANT: The idea at that time was to reopen the vessel to let the blood flow go through. Thrombosis was not yet brought into the coronary artery disease process. People were beginning to talk about stunning and things of that sort, reperfusion, and the first thing was the concept of the antioxidant. So it was very slow. I would say it was a very slow process to bring in a new concept and new ideas.

FYE: Did you find it harder to engage the cardiology community in molecular biology? In other words, if you had a vision of molecular biology and theoretically that vision would go in all three directions: heart, lung, and blood. Did you find the communities responded differently?

LENFANT: No, the community was very . . . it worked. If you asked these people, I do not know, but I have the feeling that the community accepted all that. I think they saw that I was very supportive of all that was happening. One thing, too, that I think made me very different from some of the other people is that I was very open to talking to anyone, anywhere, anytime. Which I think is very important to do.

FYE: Communication.

LENFANT: Yes. But many of my colleagues at NIH are very aloof, or they are in charge. I do not do that.

FYE: Who were some of the people that you depended on most in the 1980s after you became Director.

LENFANT: Julius Comroe, [Eugene] Braunwald. I brought Braunwald back to the Institute. There was Elliott Rappaport.

FYE: Elliott Rappaport, San Francisco.

LENFANT: Yes. There was, what is the fellow from Emory? I forgot his name.

FYE: [J.] Willis Hurst.

LENFANT: Willis Hurst. You see I had known them on the [advisory] council. And, of course, John Kirklin, whom I knew for many years.

FYE: Because of your heart-lung research.

LENFANT: Yes. So I would talk to these people all the time.

FYE: So you had a network of people around the country in prominent academic positions and that helped you in your role in Bethesda.

LENFANT: Yes, sure. I may not have been accepted, you see, although I think I was accepted relatively quickly, but certainly within the Institute I was not accepted immediately. But the fact is that I had many more assets than other people had and people in the institute realized that people knew me out there. It is very interesting to see all the dynamics.

FYE: Fascinating. When you say that you were not immediately accepted within the Institute, you mean when you became Director in 1982.

LENFANT: Oh, yes.

FYE: Because you had been there for ten years I would assume that you would become accepted in that role.

LENFANT: But I had left the Institute for 18 months.

FYE: Tell me about that.

LENFANT: Well, when I went to the Fogarty International Center.

FYE: But did you expect to go back? Was that a leave of absence or what?

LENFANT: No. I viewed that as my basically planning to leave the Institute, the NIH.

FYE: What did you do at the Fogarty Center?

LENFANT: I was the Director of it.

FYE: And what did it do?

LENFANT: It is the only international activities [area? place? component?] and, in fact, I was the first NIH associate director for international affairs and I was the contact to the rest of the world really. Then I was part of the U.S. Delegation to the world health assemblies, to the World Health Organization, and things of that sort.

FYE: What was the scope of activity of the Fogarty Center at that time?

LENFANT: It did not have too much, it was very limited. It had fellowships and also brought some scholars to the campus. It was actually pretty lousy. It was supporting some programs that it should never have supported. It was supporting sometime somewhere in Panama, I think it was in Panama, somewhere in Central America, something that was called the Gorgas Institute for Infectious Disease or Malaria. I forget which it was.

FYE: Gorgas?

LENFANT: Yes, I closed it down. So that did not go down very well! And I had the vision of really making the Fogarty Center a bridge between the NIH and the rest of the world.

FYE: That is what the Fogarty Center did, from your perspective.

LENFANT: It should have done, but it was not doing it very well. When Bob Levy left, I have forgotten who encouraged me to apply [for the directorship of NHLBI]. I think Cooper encouraged me to apply. Cooper at that time was at Upjohn. He was the CEO of Upjohn, and he and I had kept a very nice relationship. We became very friendly over the years, and he encouraged me to apply.

FYE: How was the Fogarty Center funded? What was the structure?

LENFANT: Just like NHLBI.

FYE: So it was a part of the NIH.

LENFANT: Yes, sure.

FYE: But you said you joined it as you were contemplating leaving the NIH. Was it that you saw that as a way to meet other people?

LENFANT: Well, I had had enough of [NHLBI] . . . Bob Levy and I did not get along very well, and he was really getting on my nerves. And, in fact, I had left. First of all, I took a sabbatical and spent almost five or six months in China and in Taiwan, mostly in Taiwan, I should say. When I came back, I applied for the Fogarty position, and I said, “It is time that I decide what I am going to do, I am going to go there.” And I got the job. Fredrickson appointed me. Fredrickson was the Director of NIH.

FYE: Donald Fredrickson.

LENFANT: Yes. Then Levy left [NHLBI], and Cooper is the one who urged me to apply so I did it and that is what happened. The chair of the search committee was Ruth Kirschstein.

FYE: Now are we back to the NHLBI Director search committee?

LENFANT: Yes. The chair was Ruth Kirschstein. Right now she is the acting director of NIH since [Harold] Varmus left. She was the Director of the National Institute for General Medical Sciences, and she was the first woman director of an institute. She is very, very good, but, anyway, she was chair. [Steven?] Steve Epstein was on the search committee. And I am sure that Steve Epstein probably was looking at the candidacy of Frommer with a good deal of interest because they are very good friends.

FYE: Interesting dynamics. So then you came back as NHLBI Director.

LENFANT: Correct, yes.

FYE: And there was a period obviously as you say of some rapprochement when you have to get back into that environment!

LENFANT: Yes.

FYE: How was that?

LENFANT: My position was, “Like it or not, I am here!”

FYE: Okay, you were the Director.

LENFANT: That is right. And I became very assertive--I was probably too assertive, but anyway.

FYE: Was there much turnover? Did many people leave in that context?

LENFANT: No.

FYE: Not really? So they adjusted.

LENFANT: Yes. I made some changes very quickly. I created a new division. I created the Division of Epidemiology and Clinical Applications. And Barbara Packard who was a superb lady, really very nice, was not tough enough in my view to make changes, so I took her out of the Division and brought it to my office. And [Eugene] Gene Passamani, do you know him?

FYE: Yes, Gene Passamani

LENFANT: . . . became the Director of it. Gene did a good job there. We had differences of opinion, but Gene is very smart, he is a bona fide cardiologist, and he did well for the Institute.

FYE: What were some of the main challenges that you faced when you became NHLBI Director?

LENFANT: In the first place that was the time when budgets began to have their problems, and then all kinds of new issues came up. That was the beginning of the thrombolytic approach, for example, in coronary heart disease, and the creation of new clinical trials . . . There were lots of things, and then, the lung and blood were growing and pushing things. These were very exciting and interesting years.

FYE: So, even despite the budget crunch which was really a problem because of all the excitement, it was still . . .

LENFANT: But my position was--you are going to get a good laugh about this–that people were competing on the budget which was say $700 million and they would say, “Ah, only $700 million,” and each time I would go someplace I would say, “Okay would $750 be enough?” Never could anyone say what the limit would be. So I would always close the discussion and say, “Well, are you telling me that you could do lots of things with $50 million more, but you cannot do anything with $700 million?” And I mean that!

FYE: Hard to respond to that.

LENFANT: That is right! And I still think that way actually.

FYE: It is interesting. Biomedical science in this country since the Second World War has been an enormous growth industry and a very hungry child, you might say . . .

LENFANT: Sure.

FYE: I mean, very ambitious...

LENFANT: Sure. And it is fun. I have no problem with that, but at the same time I take the position that once it is done, you have to move on and stop complaining all the time.

FYE: Thinking of the first decade that you were Director of the Institute, what are you most proud of? What are some of the accomplishments that you are most proud of in that 1980s decade?

LENFANT: First of all, the establishment of DECA, the Division of Epidemiology and Clinical Applications. The community would not agree with that, but I think that gave a new identity to epidemiology research, to population-based research. I think that there is no question about that. I also think that I really established the prevention programs in the Institute. They did not exist before. There was the National High Blood Pressure Education program, but that was the only one. During that period of time I created the National Cholesterol Education programs, the National Heart Attack Alert programs, and we created a program of blood resources and safety. The asthma education program started later. But, anyway, the Office of Prevention, Education and Control was a unique organization, and now it is an organization which is on the national, if not the international, scene. We established lots of international liaisons, and strengthened some. We certainly brought molecular medicine into the program. We created programs like the cardiology prevention academic award. Lots of things happened during that period of time. We multiplied the number of specialized centers of research. When I became the Director, there was one on coronary heart disease--of course, it was myocardial infarction or coronary heart disease or whatever--but now we have pediatric cardiology, we have hypertension, we have heart failure--what else do we have--we have heart disease in blacks. There are all kinds of programs. We also started lots of studies to look at the health disparities.

FYE: In different populations.

LENFANT: Yes.

FYE: It is a rich record of achievement.

LENFANT: I think the Institute has grown up during that period of time.

FYE: We are going to come back to a number of those programs because I do want to ask you to expound upon, actually, each of the ones you mentioned. Thinking again of the 1980s, you just mentioned some of the things that you are proud of in terms of accomplishments. During your first decade of Director, what were some of your greatest frustrations or disappointments?

LENFANT: I have always been frustrated and still am, actually, by the lack of real interest of the community we serve. They are really like a bunch of little kids who do not get the second serving or more of the dessert. It is that kind of attitude. It is impossible to please those guys.

FYE: By those guys you mean the biomedical research community, the people that you say you serve, the people that apply to the NHLBI for grants, and the like.

LENFANT: Yes. One thing, too, that I found very difficult, but that is America. We just cannot establish priorities and say, “Okay, that is what we need to do and we are going to do it even if it takes not doing something else.” And Clinton, of whom I am certainly not a great admirer, that is what he does, and look at all the flack that he gets from everybody. But as a nation it is very difficult for this nation to do that. There is something in the NIH domain which is much more [?credulous] than that.

FYE: Obviously, you are contrasting that with your experience in France, I assume. But how is it different, what do you think it is that makes this country different, is it the democracy versus . . .

LENFANT: Yes. Sure. One thing that I did that was a very interesting and trying activity was to close the cardiac surgery program in the Institute.

FYE: I have that on my list but let us talk about that.

LENFANT: That was after . . . what was his name, Glenn...

FYE: Glen Morrow

LENFANT: Glen Morrow killed himself. We were left with [Charles] Chuck McIntosh who was a superb surgeon. His techniques were just perfect, but it was very difficult-- he was not even certified. I do not think he was Board surgery certified, he just became a surgeon. I do not know how that happened, but there he was, and for that reason we could not make him the chief of surgery. So we looked around and in those days salaries were bad, and we found a man who had personal wealth and had been, I guess, at one time a reputable surgeon. His name was Richard Clark. He was somewhere in St. Louis, I think. Anyway, we brought him in and pretty soon it became clear that he was a disaster. I understand, although I never witnessed it myself, that he was also a lousy surgeon. So what came from all that is that the program began to flounder. Then we recruited another surgeon who was Julie Swain, the sister of Judy Swain.

FYE: Julie is the surgeon, though.

LENFANT: That is right.

FYE: Is that what you said, Julie Swain?

LENFANT: Yes. She is the identical twin of Judy.

FYE: Identical twin?

LENFANT: Oh, yes. Except for one thing that Judy is a very social animal.

FYE: Judy is social?

LENFANT: Yes! In contrast Julie is a ball of fire. She cannot talk, she can only yell. And she is a surgeon. I mean, it is very interesting. Somehow there must be a little gene someplace that did not work because Judy is married. Julie is not married and is a very temperamental lady. So Julie got in there and she was throwing instruments across the surgery room! Everybody was complaining and then there were a few situations that were a little bit fishy. We did not kill the patients but it was pretty close. And for all of these reasons . . . there was no research protocol. It was all service, and most of the services were by taking patients from Pakistan, India, and all that at great cost for the Institute. So finally we decided enough was enough. We would close cardiac surgery and Edward Cohn [?], who was the Director of the Division, and I confronted that and one day we called the troops and told them. That was quite an experience, I must say. So Julie left shortly after and ?Cohn left and that was it. But now we are thinking of reopening the cardiac surgery program. You know that Betsy Nable is with us. Do you know her?

FYE: I do. It is funny I do know her. She has come back. She is now at the Institute?

LENFANT: Oh, yes. She was never at the Institute [before].

FYE: She was in Michigan?

LENFANT: Yes. She was in Michigan.

FYE: I think she was at Johns Hopkins at one point.

LENFANT: Yes. She was at Johns Hopkins. Then she is a product of Boston, of [Eugene] Gene Braunwald.

FYE: I knew her on the Publications Committee of the American Heart Association. We were on that committee together, but I did not realize she was at Johns Hopkins and I did not know that she had gone to the Institute.

LENFANT: Yes. Her husband was offered a job at the NIH as Director of the Vaccine Center, and he wanted it very much. At that time Steve Epstein had left and clearly our clinical services were kind of floundering It was not going well at all. So we thought of bringing her in as the chief of the clinical services working under Ed ?Cohn, but she did not want any of that so what I did was to split the division. It is one division but with two heads. The dynamics of all that is quite interesting, but she is superb, she is a good person. I do what I can to support her. I hope we can keep her happy.

FYE: Thinking back about the heart surgery program . The focus of it as I understand was on hypertrophic cardiomyopathy and the myectomy and also on valve replacement before you were there but when Glen Morrow got started. Of course, that was the era of prosthetic valves. I read something that you wrote in 1990 explaining the decision to close the heart surgery program in 1990, and you emphasized that valve surgery had become a standard procedure and that cardiac surgery in general was a mature discipline and high quality cardiac surgery was widely available throughout the United States.

LENFANT: You did read my pieces! That is fantastic.

FYE: Yes. You made it clear, however, that the Institute would continue to fund extramural cardiac surgery research. How was that decision received? I can imagine that the people in the Institute who were in the heart surgery program were devastated or upset, but how did the larger community of cardiac surgery respond?

LENFANT: Very badly. The thoracic surgery community objected very much to that, and we made an agreement--I cannot remember the terms exactly--but I offered to the American Society of Thoracic Surgery--yes, that is how it is called--that we would support a fellowship program for a cardiac thoracic surgeon who would want to come to NIH to hear research principles and basic research and things of that sort. We made the arrangement that they could be there for one year, or two years, or two months here and two months somewhere else to keep their surgical skills or whatever. Nobody every applied for that. That was really, in my view, the decline of thoracic surgery research as we knew it in the 1950s and in the 1960s. In fact, we now fund very few research grants in departments of surgery, and I hope that if we can resurrect cardiac surgery at NIH, especially with Betsy, then that would reenergize cardiac surgery, experimental surgery research in the country. But we fund very few. I would say if we give ten grants out of 3500 to surgeons that is about it.

FYE: Do you know the term that has been used for the surgeons who pioneered open heart surgery? They were "physiological surgeons" because they were dealing with hemodynamics . . .

LENFANT: Yes.

FYE: . . . they were trying to fix broken hemodynamics, and they had to think physiologically. Do you think that part of the reason that research in cardiac surgery has declined is that there really are limits to how many things you can fix and how many ways you can fix them?

LENFANT: Yes.

FYE: What things do you envision going forward . . .

LENFANT: Well, there are things which have occurred over the years, the Batista procedure, for example, and then another one is called the Dor procedure. It is a kind of a variation of the Batista procedure. The transmyocardial laser may be something, and, of course, there is the whole area of robotic surgery, then micro incisions and things of that sort. Then also, let me see I understand that, in coronary bypass it went from the saphenous vein to the mammary arteries, mammary . . .

FYE: Internal mammary or internal thoracic artery.

LENFANT: Yes. But now they are using radial [arteries?], and that may be quite interesting. Actually, it may lead to new interesting developments which is to pick up arteries. Of course, another issue where I think there is tremendous potential is in the treatment of aneurysms, which, I believe, is going to be a joint venture between cardiologists, molecular medicine, and surgery.

FYE: When you say aneurysms, do you mean of the aorta?

LENFANT: Yes.

FYE: Not of the left ventricle...

LENFANT: No.

FYE: But vascular aneurysms

LENFANT: Yes, vascular aneurysms.

FYE: Right. I think it is interesting that you obviously do have a vision of cardiac surgery as a potential growth area in terms of research rather than the very narrow vision I put out there as a bunch of--I am reluctant to say this--plumbers that come to fix . . .

LENFANT: Yes. There are opportunities, but we have got to bring a focus and then, of course, God knows what is going to happen with gene therapy. That is an open question right now. Let me put it this way, if thoracic surgeons and especially cardiovascular surgeons remain physiological surgeons then they will not go anywhere, but if they become molecular surgeons or that kind of surgeon . . .

FYE: That is the word I was thinking of when you started. I think you have maybe coined a term. That is interesting.

LENFANT: I think that they will find that there is a new domain for them.

FYE: Have you written about this?

LENFANT: No.

FYE: I would urge you to write about this. I am quite serious. In fact, I would urge you to put "molecular surgery" in the title, or "molecular surgeon," because I do not know if anybody has ever used that phrase but you really built up to that. It makes a great deal of sense the way you have articulated that and it is also very symmetrical with what you explained earlier that you did almost twenty years ago at NHLBI to begin with in terms of shifting the focus to that. I do not want to jump around too much here inchronology but it is fascinating to go down these paths. Now let me see. I am still with your role as director, and there are going to be a variety of programs I will ask you about. Moving ahead into the 1990s, I will ask more or less the same sorts of questions but I will frame it a bit differently. During this most recent decade, what aspect of your job did you enjoy the most?

LENFANT: I think the thing that has been the most important to me during the last, I would say, ten years has been to keep the institute a single entity. Today, there is a great deal of divisions and individual interests which, in philosophical terms you could say, is weighing the individual interests versus the societal interests and in an organization like ours which has so many interests, it is very difficult to keep it as a single organization. Yet I think it is absolutely critical for a very simple reason is that many research areas and certainly the maximizing and some new disciplines and approaches it permeates the entire institute. I think that in the economy of things we just cannot reproduce all of those things all over the place and we have to categorize them on the developing threads which are common in the institute. That is a very frustrating thing. It is very difficult. Then the community has become more demanding in many ways. In the last four to five years the conflict with the epidemiology community, the population-based community, for example, has become quite visible. But you can understand why because the research has moved from the person to the organ to the cell and the genes and the molecules, yet if you are one of these people who want to look at people and tens of thousands of people.... So there are some kind of conflicts that are developing, and it has been quite interesting. We have seen many changes in the institute too, many personnel changes, and that has taken a good deal of my time. I personally think that if the institute can stay together, meaning that the heart group works with the lung group and the lung group works with the blood group, and that we all work together, the institute has the capacity of being a true leader at NIH, because this problem of division, of competition, that I am talking about exists everywhere at NIH. In this regard I think that Varmus deserves lots of credit because he had that somewhat unifying vision of things which I personally certainly endorse. He probably wanted to do it too fast and too soon, but the vision is correct, I think. Then, when you look at the programs, we have seen the emergence of disciplines and approaches that you would never have been thinking about just before. Take pharmacogenetics for example, molecular epidemiology, trying to predict the evolution of disease on the basis of the patient's genetic profiles. I mean, very unique things. In fact I am thinking about all those things a lot because I have been asked to give a presentation on the future of cardiology in this century and that is the kind of thing I am thinking about.

FYE: I wish you well with that! I think it is challenging. I will just digress for a moment. I was asked by Sylvan Weinberg after my book on American cardiology was published to do an ACCEL interview on the future of cardiology, and on the telephone I told him, "I don't do the future, I do the past." But he is a very persuasive person, and kept assuring me he was sure it would be interesting and we could do it. We sat down then many months later in the ACCEL studio to frame the discussion and, finally, he concluded with me that I could not do the future! But I think just from things you have been saying that obviously you are an incredibly visionary person and you can. Do you think part of that is from your philosophy background and your interest in that as a young person?

LENFANT: Yes, probably. Philosophically, I am really not interested in today. Today does not interest me. I am much more interested in where we are going. That is an area where the staff of the institute and I pass each other, we just do not communicate. They do not understand. When an application comes or a big program is being discussed, they all jump up and down and say it is terrific, and I say, "Okay, where's that going to be in five years?" There is silence. Nobody wants to hear about it, and yet I am very interested in the budget impact and how that is related to the other things which may emerge at that time and so forth. Maybe I am too philosophical for what I do!

FYE: Thinking again about the decade of the 1990s, maybe I am asking the same question just with different words, but what are you most proud of in terms of your accomplishments at the Institute, and throughout the whole scientific community for that matter, during the last decade?

LENFANT: I think that what has been done, or what has happened during the last ten years, the 1990s as you put it, is that we have established and rooted very deeply the changes in the approach of research in the Institute. Clearly now the dominant force in the Institute, and I would almost say it is perhaps to a fault--we may have overshot the goal here--is the establishment of the molecular approaches which I believe are very important, but in my mind they must remain just approaches, not the end. I think there may be a tendency in the Institute, certainly among my colleagues, not in this group here, but at the [American] Heart Association, to view that as almost an end. You must be aware of the dispute between the genome institute and the private sector with regard to the mapping of the human genome. Do you know what I am talking about?

FYE: I know about it superficially. I certainly know what the project is.

LENFANT: Basically, the issue is that the Human Genome Institute is mapping the genome, and I think every week they put on the web the result of what has happened during the week. Now, there is at least one private company which does exactly the same thing and seems to be doing a little bit better than the genome institute and they have a different technique. I must admit I am incompetent to evaluate that but, from what I am hearing and seeing, I would think that the private company has a better approach, a better technique. But they do not put anything on the web because they want to patent. It is not patenting a gene today, but they want to patent in order to protect what is going to happen in two or three years from now. That is quite interesting to me as that dispute is about the value of today versus the value of tomorrow, if you want. I think these are very interesting issues.

FYE: This tape is going to run out in about one second.

This is a continuation of the fourth in a series of oral history interviews of Dr. Claude Lenfant, Director, NHLBI. The interview was conducted in Anaheim, California, on 13 March 2000. The interviewer is Dr. W. Bruce Fye.

TAPE 8

FYE: You have just described some of the things in the 1990s of which you are most proud and that you are pleased about in terms of your role at NIH or at NHLBI. Are there other things that you are especially proud of in terms of the 1990s?

LENFANT: I think it is very difficult to single out just one thing. I mean, the Institute is a very complex organization. I hope, and history will judge, that it is a successful organization. But I am not sure that I can single out one thing saying that is what is driving the whole thing. It is really the constellation of all these things that makes the organization work. So, in this regard, it is very difficult to answer your question. It is like the comparison you have of people who think that the only research that is good is breakthrough research. I do not. I think what is more important is sequential, a little step at a time. Of course, everybody wants to have a [breakthrough]. . . but it just does not happen and most of the time these big things which are happening, the big splash, but what is the value?

FYE: You know that Julius Comroe wrote a two-volume book about the top ten advances in cardiovascular and pulmonary medicine and physiology and part of his premise, if not his whole premise, was that so much of medicine and science, medicine in particular, is steps. It is stepwise . . .

LENFANT: Right. He wrote a book, I forget the title, but, anyway, the theme is that it is like climbing a mountain. I do not know if you know the book.

FYE: Yes, I do know the book. I think it has a white and blue dust wrapper, Exploring the Heart, or something

LENFANT: Exploring the Heart, that is it. And that is the way I feel. It takes everybody tied together. So, in this regard, that makes it very difficult to say, "Which is the person that has been the most important or what is the thing that has been the most important?" [What] I view as the most important is the whole enterprise.

FYE: Now thinking back about the 1990s . . .

LENFANT: I think the best testimony--I do not think that the Institute can take credit for what you see here [at ACC meeting]–but we have been a very strong significant component of this research which is reported here. I would be hard put to single out any specific thing that is responsible for the advances, but there are lots of little things which have contributed to this march toward progress.

FYE: Just to clarify when you say what you see here, we are sitting in a hotel in the context of the American College of Cardiology's 49th Annual Scientific Sessions with thousands of presentations of original research. I am sure a very significant percentage of those reports were supported at least in part by NHLBI grants.

LENFANT: Oh, yes. In many of the new medications and things that we hear all the time, you could see our fingerprint someplace.

FYE: I think something that it is very difficult to overemphasize is the role of the NIH and NHLBI, in particular, in terms of cardiovascular research and pulmonary research and blood research. But the thing that I came to appreciate so much when I did the research for the book I wrote on American cardiology was that the creation of academic medicine and then its tremendous growth was all fueled by federal money . . .

LENFANT: Oh, yes.

FYE: . . . most of which came through the NIH, and, obviously, in terms of the specialties that come to this meeting came through your institute. As I say, I should not be editorializing on this, but I feel so strongly that people do not understand or have no comprehension of how we got to where we are in this country with such an enormous biomedical research enterprise.

LENFANT: Correct, yes.

FYE: Do you want to expand on that from your perspective because you came to this country when it was a pretty small biomedical research community and now it has exploded. It must be at least an order of magnitude larger, I would think.

LENFANT: I think the tremendous asset of this country is basically to have granted freedom of thinking and imagination and of acting to the academic community. I do not think looking at the population as a whole that Americans are smarter than the Russians or French or British or whatever. It is the system. It is just like bacteriology. You need to have a medium for the bacteria to grow! It is the same thing here. I think the environment allows that to happen and it does work. And I think our role at the NIH, which is the object of considerable debate with some of my colleagues I have to say, is how do we balance our role versus just being the people who help others to express themselves.

FYE: Now, when you say your role, do you mean the intramural research versus the support . . .

LENFANT: No, I mean the extramural research. Many people in government have the feeling that they are in charge and that they should decide on this program or that program, and I do not totally believe in that. I think that we have to be very careful that for the people who are actually doing the work that it be the expression of their own ideas. That is the basis of NIH. I mean, the motto of NIH is to support the investigator initiated research. And I think that is a very critical thing. But I would take it to an extreme. Investigator initiated research may be impeded by a government agency or government employees, but it can be impeded by an investigator's own colleagues as well. So many times I have seen investigators put down proposed research by others for what was obviously the wrong reason, and I think that is an evolution of our society.

FYE: It certainly does seem to be. Obvious is too strong a word because it reflects a bias I have, but it seems logical that an investigator or anyone for that matter will be more motivated to do something if it is their own interest, it is their idea, they have passion for it and they are interested in it rather than if they are given an assignment.

LENFANT: Sure.

FYE: That is another way of saying what you have said, and the role that you have articulated–that NIH has played--is really allowing people largely to pursue their own interests, their own agendas. They have to fit within certain parameters in terms of the scope of research that is under your purview or of interest in the largest sense to the institute. But is that pretty much it?

LENFANT: Yes.

FYE: Another thing I wanted to ask you--it has nothing to do with the comment you made to me a few minutes ago about the future--but I do have a general question here. I just said, "Your background in clinical research included extensive involvement in cardiac research as well as pulmonary research. It seems unlikely that future directors will have that broad experience. What advice would you give to those persons who are charged with choosing your successor?" What should they look for?

LENFANT: I do not know that I can touch that one!

FYE: Well, they will not ever see or read this until decades after they have made the choice probably . . .

LENFANT: It is an important question, a very, very important question. I feel very strongly that somebody who comes as an institute director to NIH, especially an institute like ours which has such a wide range of interests, must be very open-minded. I see that in other institutes where some of the directors were coming, if they have definite ideas about what is good and what is bad, and I suppose that is why they are picked, and then conversely you could ask, "Why did they pick you then?" But I see a real risk in that. The best demonstration of that is the time that it took [Harold] Varmus to adapt to his job. I mean, when he came if you were conversant or able, and even when he left, actually, if you were not an expert in molecular genetics, you were nothing. And I see that risk as being very great at the NIH. So the first thing that I would say is that it is critical to take somebody who is open-minded and has a true assessment of his or her responsibility. You just cannot say, I believe, "That is what I do, that is what I believe, and that is what we are going to do." I do not believe in that. But I think that whoever oversees that process should be very sensitive to that.

FYE: Now, I would ask you to contrast what you have just said about looking forward with what you told me earlier. I think there is a difference, but I would like you to see if you could tell me what the difference is. You said when you became NHLBI Director in 1982 you had to go in and make some tough decisions and that you had some strong ideas about things. So I think, on the one hand, you are talking about an idealistic view of what would be . . .

LENFANT: Yes.

FYE: . . . optimal but then, on the other hand, you experience something that you have had to be more pragmatic.

LENFANT: I think the answer to that is simple and again, others should attest to that or challenge me, but I think that I wanted to be additive not instead of.

FYE: Additive, rather than instead of.

LENFANT: Yes. And I think the whole distinction, admittedly with time things must be replaced by others. That is the way it goes, but it should be a very progressive process and not almost a caveat. So what I am trying to say in answer to your question is that I think that it is very important that people who occupy this position be very open-minded about what is going on. I would say to you that a purist would say, "That person really has no expertise in anything," which is what some people would say about me, but if you can compensate [for] the lack of expertise by being receptive to the ideas then I think that is okay.

FYE: I understand that . . .

LENFANT: I do not want my ideas to compete with the ideas of others, at least in this area of the management of the institute.

FYE: Talking about the management of the institute, you have served under a number of NIH directors.

LENFANT: Yes.

FYE: I wonder if you could say something about your relationship with the office of the director, in other words with the position. Just what is the relationship and how do you interact with that highest level of authority at the NIH. Then, maybe, you could digress and describe a little from your perspective the different directors under whom you have served.

LENFANT: I have some pretty different ideas on that! My idea is that it is important for institute directors to have a close relationship with the office of the NIH director. I think that is very important. After all, my position is that I am not independent, I work at NIH, I am a component of NIH and therefore, I have to, and basically want to, be a good soldier. If I do not agree, I want to be able to say that I do not agree and why, but I recognize that somebody has to make a decision. That is all well and good, but it works only to the extent that the institute director is interested in that. It has to be a two-way street. And over the years what I have found is that not all NIH directors are open to that kind of approach. There are NIH directors whom I would see all the time and there are others whom I saw maybe once or twice a year. So, [there are] very different personalities. I think the latter is bad, but if that is the way it is, that is the way it is. You have to accept that.

FYE: Could you go through each NIH director, maybe sequentially, and give me just a thumbnail sketch of them from your perspective, anything that you can . . .

LENFANT: The first one was Marston

FYE: Robert Marston.

LENFANT: Yes. Who died what this year or something like that?

FYE: Very recently.

LENFANT: Yes. He was the director of the NIH when Cooper brought me here, and he was followed by...

FYE: Before we go on, maybe could you describe him a little bit for me, from your perspective.

LENFANT: He was pretty good, actually. He was certainly very supportive of the Institute. Now, admittedly, that was when I first came and I had relatively little contact with him. I did have more contact with the one that followed which was Bob Stone. Robert Stone.

FYE: Robert Stone

LENFANT: I forget what he came from, but I had more contact with him because he was very friendly with Ted Cooper. Again, I had a good social relationship with Ted Cooper and often after work–Cooper was living on the campus--I would go and have a drink in his place and I knew his wife and his kids, and Bob Stone would come too. Bob Stone was an interesting fellow. He was not at NIH for a very long time but he was an interesting fellow. Then he was followed by Fredrickson, and Fredrickson and I had a very nice rapport. I knew him very well. Of course, Fredrickson was followed by Wyngaarden. In between, of course, there was an acting director for a short period of time. In this case it was [Thomas] Tom Malone, who was an African-American who was the deputy director under Fredrickson.

FYE: Is that Malone?

LENFANT: Yes, right. So after that there was Jim Wyngarden, with whom I really had a very nice rapport. And, not in the last two or three years, but I see him from time to time, less since he went back to Durham. But, after he left the NIH, he lived in Washington, and we would see each other from time to time.

FYE: Let us just stop with him and if I could ask you to describe, for example, with him, you mentioned that socially you were friends, too, but in terms of your professional relationship with him. How would that work? How would you get to meet with him? Would he set up meetings, would you ask to see him, or how would that work?

LENFANT: I would ask to see him, and he was very receptive. Then we traveled together. He is the only person with whom I traveled, actually. We went to South America together, we went to China, and to Japan together. We went somewhere else in Europe. Yes, that was the time of the one hundredth anniversary of NIH. And we went to Great Britain together. I had lots of contacts there, and we stayed together at the Royal Society of Medicine. And Jim, I was going to say was, but I am sure still is, an art collector and I do collect some art too. So we would talk about this carving or that carving or this thing or that painting or whatever. And sometimes we were discussing NIH and . . . I do not know if you have heard about the program which is called the Merit Award? .

FYE: I have heard of it, but I have heard the term.

LENFANT: Basically the Merit Award is that if you are distinguished when you get a grant for five years, you get the next five years without competition. So, it is a ten-year award. Well, Jim and I designed that program. I wanted to do it, so I went to see him, and we went back and forth. Of course, you have to talk to all the other institute directors, but eventually it was done. So we were very intimate. I mean, Jim was a terrific fellow. We traveled to Egypt together, too--he had a significant other and he was divorced--and we went down the Nile together. In fact, he took care of me. I will always remember that. I was coming back from, I forget if it was Nairobi or Zimbabwe or whatever, and, believe it or not, it was very difficult to go from one African country to another African country in those days. So I flew from Zimbabwe or Nairobi, wherever I was, to Frankfurt, changed planes, and went back to Cairo. And all that without a stop, but I probably ate something bad, and so I lost lots of water. I remember that, finally, when I left the plane in Cairo, there were hundreds of people going through security, and in that part of the world, people are not terribly orderly in their behavior, and I collapsed. I was taken to some emergency room and I remember so well that I was holding on to my bag because I wanted my bag not to disappear! Eventually, I could talk to the physician there, and I told him I was a physician myself and I was going to a hotel. So they put me in a limousine, a fancy taxi, that took me to my hotel. When I was checking in, there was a wedding in that hotel–it was a very nice hotel in Cairo--and weddings in Cairo are no small affair. As I was checking in, I felt myself collapsing again, and I disappeared under the counter. Jim Wyngaarden with his friend came to see me, they knew I was arriving, and literally picked me up off the floor and took me to my room and put me to bed.!

FYE: Amazing.

LENFANT: So, anyway, I did come around and then we went down the Nile together.

FYE: It is not fun to get sick in another country.

LENFANT: Oh, no. That was the second time in my life that it happened to me. Another time was in Hong Kong when I was coming there from Thailand. I know I had eaten something bad in Thailand, and I was so sick in Thailand. That was before I was at the NIH, actually. So, after Jim Wyngaarden came our friend Bernadine Healy and, in between, there was a very nice fellow actually as an acting director. But in came Bernadine Healy. She was some piece of work, I have to say. She was just impossible. And then Varmus. So, I have known six NIH directors.

FYE: In terms of Bernadine Healy, with whom I trained and whom I do know, expand upon your comments.

LENFANT: You know she is now at the Red Cross, and she is turning the place upside down. The blood is leaking!

FYE: Great metaphor. How did you find it impossible? What were the dynamics that led to the frustration?

LENFANT: First of all, she was at NIH for what, about three years, something like that?

FYE: I think that is about right.

LENFANT: And I think I saw her three times, that is all. It was impossible to see her.

FYE: Interesting, too, because, of course, her background is in cardiology so you would have thought that she would have had a special interest in NHLBI.

LENFANT: She was so purposely--although I did not know the purpose--unpleasant with me. It is so interesting because when the negotiations were taking place for her to get the job, that is when I had my back problems. In three months I had two disks removed, one was an emergency one, which was quite an experience, I must say. The other one was planned, and I had it done at NIH in one of their protocols there. But, after that, I went back home and I was flat on my back. And she called me--I knew her very well. I mean, she was in many of our programs--and she said, “Oh, I cannot wait. I hope I am going to get that job. I cannot wait to have it, and you and I are going to work together. We are going to do great things, and I hope you are . . .” Then, maybe one week later, I went back to work and she already had the job. From then on, it was as though I had spoken to a different person. Amazing behavior.

FYE: How was she received by the rest of NIH institute directors?

LENFANT: Very badly. I do not think there was anyone who really supported her very well. But I will tell you, Bruce, it is very difficult. I have been at NIH for a long time. I know many people there whom I have seen come and go and come again and so forth. They all tell you things and it is very difficult to know if what they say is true. I can tell you lots of people seemed to be on good terms with her and, later, I heard that it was not true, it was terrible, and the converse was true. So I do not really know. My perception, again, it is a personal opinion, is that she was not well received at NIH. She certainly left a lot of anger in people, who were not happy with what she had done, mostly because they did not know why she was doing it.

FYE: So it was a communication question in part.

LENFANT: No, it was really her personality. She certainly wanted to establish herself as a great leader, and she would do it no matter what. But then, in contrast, Dick Lewis, the former [ACC] president, he is in her institution. Each time I talk to him about her--not for the last year or so since she left--but before then, for him she was walking on water. But, not at NIH, and then she was followed by [Harold] Varmus, who was another interesting person.

FYE: Followed by Varmus.

LENFANT: Yes. I have known six NIH directors and probably four or five acting directors.

FYE: Now, the directors are political appointments.

LENFANT: Oh, yes.

FYE: So, how big a deal is it for directors of institutes such as yourself that, for example, [after the election] coming up you could have a new administration and they conceivably could appoint a new director. In fact, I guess, you might expect that. . . In other words, how often has it been that when the party changed, for example . . .

LENFANT: Each time.

FYE: Each time it is a new appointment.

LENFANT: Each time, yes.

FYE: I guess, it would not have to be. I mean, theoretically . . .

LENFANT: No, it does not have to be. In fact, right now, when Varmus left he pretty much designated his heir. He gave two or three possibilities but one of them, I understand, I do not know for sure, has been selected by the White House, and he is going through the FBI security clearance and all that stuff. If he clears which is certainly going to happen, then his name would be sent to the Senate for confirmation because any presidential appointment needs to be confirmed by the Senate. The issue is is that going to happen?

FYE: Right. In the time frame because there are only ten months left in the present administration.

LENFANT: Sure. So we will see what happens there. My guess is that it is not going to happen.

FYE: So there will be an acting director until the next Administration.

LENFANT: Ruth Kirschstein is the acting director.

FYE: You are right, you mentioned that..

LENFANT: Yes.

FYE: Obviously you cannot change the process of political appointments, but, from your perspective, would it be better if the directorship were not a political appointment? In other words, for continuity . . . I am just speculating, I am not saying that this should be your answer, but have you sensed or do you sense among your colleagues as directors of other institutes a wish that the NIH was not vulnerable perhaps every four years to some new director who could conceivably change directions, and who certainly would be a different personality?

LENFANT: Yes, I think some of my colleagues would say it would be better. Personally, it does not bother me.

FYE: Well, it is the way our government works, after all. We do that with the president every four years theoretically. As a government agency, I guess, you could imagine why people are comfortable with that model.

LENFANT: It does not bother me at all.

FYE: How much autonomy do you feel you have as director of NHLBI?

LENFANT: Lots.

FYE: Has it grown or shrunk during your 20-year tenure, do you think?

LENFANT: Probably shrunk a little bit, expressed in a different way. But I think the institute directors have lots of autonomy.

FYE: What does that mean, for example, in terms of the budget. It is an enormous budget. How much is your budget for NHLBI this year?

LENFANT: $2.1 billion.

FYE: Billion?

LENFANT: Billion.

FYE: An enormous amount of money!

LENFANT: Yes, that is a nice piece of change.

FYE: More money than probably the gross national product of half the countries on the earth.

LENFANT: Probably!

FYE: That is an enormous budget. Obviously, there is a huge staff in the institute and outside the institute that works on that budget, but how much input do you have? I do not imagine you micromanage $2.1 billion, but in terms of creating new programs now or shifting moneys toward a program or away . . .

LENFANT: Lots.

FYE: . . . from a program. You have a lot of that.

LENFANT: Yes. I do not mean to sound pompous, but I really control [the budget]. The institute directors control their budgets.

FYE: Who do you look to get help in deciding what to do with that budget?

LENFANT: The division directors. The division directors and then... but there are checks and balances. Everybody sees what I do and then we have a National Advisory Council. They know what I do and they can see the expenditure of money, where it goes and so forth. But I can influence [things]. An institute director, at least in our institute, can influence the distribution of the budget very significantly, not to say completely. I think the trick--again, when I am gone, I am sure there will be lots of discussions, comparisons, and things like that about it--but the trick is to do it in an even-handed manner. I hope I have done that. Whether it is perceived that way as well, I do not know. That is the issue, but it is true that I really feel that my biggest responsibility is that money. My staff would tell you that I am very stingy and conservative, and I argue about spending five dollars! They do not understand that, but I do. I do argue about five dollars.

FYE: Let me frame my question differently. Obviously, you do control or influence an enormous amount of money that influences an enormous number of lives, professional careers, and research projects. How much time do you have to spend responding to people that lobby you in one way or another to think about their project? I am not talking about asking for real favors but just saying in the way that everybody who goes to Congress to lobby them says, “Pay attention to my project.” How much of that comes to you?

LENFANT: Lots. When you saw me this morning, I had been trying to go to tell the people that I may not be able to be at the convocation tomorrow since the exhibit had opened! That is why I do not show my badge. I hide it so . . .

FYE: They catch you, and they . . .

LENFANT: But lots of people know me, and it took me at least an hour to get out of that place!

FYE: Let me ask you this. When you come to one of these major national meetings, how much of your waking time is spent responding to people who are trying to influence you?

LENFANT: Nearly all of my time.

FYE: Really. That is very interesting.

LENFANT: But they do not try to influence me. Well, I guess, it is for them and for their program, but I listen to them, and I try to explain what the issue is. Let me see, in February, I was in Abu Dhabi, in the Middle East. It is on the eastern side of Saudi Arabia. It is part of the United Arab Emirates. I was there--it was a meeting on high blood pressure--and there was a man from Los Angeles over there, who really cornered me, and I said, “Maybe you should think about a clinical trial.” Then, I said, “Why don’t you ask for these people to get over there and we’ll try to meet.” Well, he cornered me again this morning to explain to me that he could not get all the people together here [at the ACC meeting] but that he had set up a meeting with my secretary and that they would all come to see me in April [in Bethesda]..

FYE: Well, if you ever would like to use it as an excuse that you have to spend time with Bruce Fye being interviewed! Obviously, you have to spend a good deal of time being familiar with the programs that you sponsor and that you support. There must be literally thousands of them.

LENFANT: I think I know them very well.

FYE: Or at least hundreds.

LENFANT: Yes. We have lots. Just for the research grants that we have, all together we serve approximately 10,000 investigators.

FYE: Investigators?

LENFANT: Yes.

FYE: Would those be just principal investigators?

LENFANT: No.

FYE: Each would have a grant or they would be part of grants.

LENFANT: Yes. We have approximately 3200, 3300 research grants, but we have a large number of centers, we have clinical trials, we have training grants, and so there are lots of¼But sometimes when a name pops out, the name does not make it click, but, if I am given more information, I eventually can locate pretty much everybody. Well, it is the institutional memory if you want.

FYE: How true. And how important too, as well. Let me see. I have some changes [in my questions] here. I think I have talked about the name changes of the institute. We have already done that. This is just a series of questions that relate to the mission of the NHLBI.

LENFANT: I probably talk too much?

FYE: No, you do not talk too much at all. We have to figure out how to make time tomorrow somewhere so you can talk more! But this question is about the NHLBI research mission, and I am sure I got this from one of the institute's publications or perhaps one of your publications. But it indicates that the research mission can be cut into three components: one of knowledge acquisition, two of knowledge validation, and three of knowledge transfer.

LENFANT: Correct.

FYE: I wonder if you could comment on those three components, broadly from your perspective.

LENFANT: I think that all three are very important. Unfortunately, for most of the people only the first two steps are what they are thinking about. I think that the translation or the transfer [of knowledge] is a very important component of our activities. But it is very difficult. It is very real money, and that may be the reason why nobody paid attention to it, but it is a very important thing. For the rest of the day I could give you examples of things that you know because you are an expert. Goods [?] that should be used but are not. That is, basically, I think, maybe because of a lack of awareness or a lack of interest. I do not know. Yesterday, actually, when I was waiting to get into that reception, I was talking to someone about that very issue because I was invited just before Christmas to go to a meeting where I had been asked to talk about [NHLBI's mission?] and goals. I speak a lot about that because something that interests me is the widening gap between what we know and what we actually apply. I think it is a tragedy actually to see that. But that was my story during the talk just before Christmas. Yesterday, I met the fellow who had invited me and he said to me, "We are going to invite you again this year because we thought that your presentation was quite interesting and you gave a fine perspective and that is what we are used to hear[ing]." I said, "But the problem is real and something has got to be done." We talked about it and, finally, this fellow admitted that it cannot be solved unless all the people who are here [at the ACC meeting] pay attention to the problem, and in order to do that look a little farther than just their experiment or their institution. So the translational aspect of what we do is, I think, very important but so neglected. The two other components, they come very easily. There is a bit of discussion between the truly basic research or applied research, validation and things of that sort. People argue about that, but eventually it gets done and it gets done well. The issue is to go from the validation to generalization and application.

FYE: In terms of knowledge transfer, as you mentioned, there is a gap between discovery and recognition of approaches or treatments and their application in practice. What do you see as some ways to improve that?

LENFANT: The will to do it. I feel very strongly about that. Ii is not deliberate neglect, but today is a world of plenty. I mean, it is like going to the candy store, there are so many things that you can do which are exciting. I admit that the translational aspect of all that is not the most glamorous thing to do. You can work in the lab, and if you are lucky you can make a significant discovery during the week. Today, that is the way it is, and that is what people want to do, but it takes, I believe, the experts we have here really to get into the translational aspect and make it happen.

FYE: What do you think about guidelines as a mechanism of encouraging . . .

LENFANT: I think that they are very powerful. The problem with guidelines is that often people are mixed up between guidelines and prescriptions. Guidelines are not a prescription. Guidelines are what they are. A guideline is to provide what you could call a smorgasboard to choose from or you could say it is the landscape in an area but, in no way, does it eliminate the physician-to-patient relationship that leads to the actual prescription. The fact is that in many aspects of medicine there are an infinite number of variations depending on the patient himself. And guidelines do not do that you see. I am a strong supporter of guidelines, but I recognize their deficiencies. The problem with guidelines is that many people think that it is like the Bible, it is like the law, and then, of course, they want to follow it and for that reason. But that is because often we do not present the guidelines very well.

FYE: It is a pretty new phenomenon.

LENFANT: Yes, sure.

FYE: Guidelines, right.

LENFANT: Yes. But I think it is a useful one. It has tremendous value and potential provided that it is utilized appropriately.

FYE: What other ways do you think [the process of] application could be enhanced? In other words, so that you could narrow the gap between knowledge that exists and knowledge that is applied?

LENFANT: I think in the absence of almost a societal turn-around or a different payment system or whatever, it is going to be very difficult to do it. Very, very difficult. I have a fairly pessimistic view of that, but, nonetheless, I think we have to try it.

FYE: Are you thinking of things such as beta blocker usage after myocardial infarction, that sort of thing?

LENFANT: Sure. And that is the worst example that I can give you. We could also talk about the cholesterol lowering or blood pressure control or all of these things. So it may very well be that one of the barriers is the cost. . . I do not know what the extent or the magnitude of the problem to pay for that is. There are a lot of people who can pay and are prepared to pay, but it is not in the right [?path] because it is not the patient, the subjects are not advised appropriately, and they are not advised because it takes time to explain to somebody the need to do those things. So there are lots of competing demands.

FYE: I think we should probably let you go and give you a chance to get to your next meeting.

LENFANT: Okay.

FYE: Thank you so much for this continued discussion.