This is the second oral history interview with Dr. Claude Lenfant, Director of the National Heart, Lung, and Blood Institute, conducted in Atlanta, Georgia, on 9 November 1999. The interviewer is Dr. W. Bruce Fye.

FYE: Let us continue our discussion from the first interview. We were talking about the time in 1959 when you went to the University of Lille. Could you tell me something about how it was that you went there?

LENFANT: Again, when I came back from the United States, it was clear that I had to find an academic position somewhere in France and the only one that I could find was at the University of Lille. But my laboratory was in this hospital, Marie Lannelongue, and so what it basically meant was that I had to commute back and forth from Paris. I think I was going to Lille twice a week. My role was just lecturing and tutoring in the student laboratory. I did not want to leave the Marie Lannelongue hospital. So, I was commuting, taking the train in the morning. It was a very fast train–actually, that was one of the first fast trains in France–and I would go to Lille and spend the day. Sometimes I would spend the night and come back the day after. I did that for a period of time, but it became old very quickly. In addition, I thought the fellow who was the department chairman was kind of a weird character and I knew that I would never make it with him.

FYE: At Lille?

LENFANT: Yes, there was no chemistry between the two of us.

FYE: It is interesting to note once again that these are facts that would not be obvious from looking at your curriculum vitae. In fact, you continued to have your laboratory in Paris and your academic and teaching activities were in Lille at this time.

LENFANT: That is right.

FYE: Were you involved in any clinical activities at this point?

LENFANT: Oh, yes. I was seeing patients every so often, but that was not the thrust of my work. I had become very interested in research, and so the patients were of interest to me as being part of my research.

FYE: When you were seeing patients, were you seeing them in Paris?

LENFANT: Yes, in Paris. In fact, the department where I was teaching in Lille was not in the medical school. It was in, how would you call that, the science university; it was not in the medical school.

FYE: And it was the department of physiology?

LENFANT: Yes.

FYE: Were you teaching general physiology as one of a corps of professors?

LENFANT: Mostly cardiopulmonary physiology. We were giving some kind of umbrella courses, if you want, and, in fact, I enjoyed that. I did enjoy teaching.

FYE: But there was no laboratory in that department?

LENFANT: No, and basically I had no affinity with the people or the place. Lille was not a fun place to be, although I understand it is much better now. The fellow who gave the Paul Dudley White lecture here at the AHA meeting is the head of cardiology at Lille, and I know him, his name is Bertrand. Do you know Michel Bertrand?

FYE: I know the name.

LENFANT: Michel Bertrand is the head of cardiology. He was introduced by Valentin Fuster, who said some very nice things about him. I have met him over the years at the College of Cardiology meeting. It is very interesting that, when I left my hotel to come here for this interview, I was concerned because there was a group of French people who all wanted to talk to me. They are all–it is kind of amusing to me, actually, amazing and amusing–

wondering who I am and what am I doing here. This is because John Ross introduced me to them, and John spoke about the things that I have done at the NIH and so on. That got them curious.

FYE: Because these were mainly younger scientists and clinical scientists and they probably are curious as to what this man from France has been doing in the United States for 40 years.

LENFANT: Yes.

FYE: Turning now to some of your scientific work, we talked earlier about your 1956 thesis, which is a substantial monograph, but, in1960, you were co-author of a classic paper on deep hypothermia with total circulatory arrest. This was one of a series of publications on that subject that you did. This particular one was written with Dr.Weiss and colleagues, some of whom you mentioned yesterday in the first interview and who were certainly in Dubost's laboratory.

LENFANT: When I was the director of the laboratory, Michel Weiss was the second in command. As I told you the other day, when I left, he became director. I think I saw on the paper that there were other fellows.

FYE: If I could ask you to say the names of the colleagues, that would be good.

LENFANT: Piwnica was actually a resident in surgery, and he was working in my laboratory, so that is how I knew him. Incidentally, Piwnica, I understand, has become a big shot in the surgery world of Paris, and Europe. The other one, Sprovieri was from Italy. He was training in my laboratory. Actually, it is very interesting. Sprovieri became one of the biggest art dealers in Europe. He has a gallery in Rome, which I understand is absolutely spectacular. I have never been to his gallery, but I know about it because, two or three

years ago, I was in Italy and I had conversations with some natives. I said, "I knew this fellow," and I was told, "Well, you know... and so on."

FYE: That is interesting.

LENFANT: This one, Daniel Laurent, I think that was his name, became chair of a department of physiology somewhere in France. Philippe Blondeau was one of Dubost's protégés, and I think he also became a department chair in one of the universities in France. So, I remember that paper very well. That was the first application–I think–of the Melrose approach. He was the British man I mentioned the other day. Dennis Melrose was a cardiovascular surgeon, somewhere in London, and his approach to open heart surgery was deep hypothermia. No, let me see, I think what I am saying is wrong because the Melrose approach was not deep hypothermia, it was moderate hypothermia associated with potassium blockage. The application in this paper may have been an adaptation of what Henry Swan was doing in this country. Henry Swan was in Denver, a very brilliant man. Did you ever hear that name?

FYE: I have heard that name and, of course, it has to be distinguished from Jeremy Swan because Jeremy Swan now is much better known, but Henry Swan, the surgeon, was in Denver.

LENFANT: Henry Swan, as happened often to people who were famous for a good cause, really became crazy. He had two problems. He liked women a lot, and he also began to drink. I knew him very well. We had a very nice relationship. Yes, I am sure now that this paper was about the Swan approach. He was a superb surgeon, but he was very rich, a very wealthy man, and he liked sailing so he decided to sail around the world, by himself, that is what was said. As I understand it, he continued to drink and when he came back from his trip he disappeared. I am sure he is dead now.

FYE: But, obviously, he had a remarkable career at that point and was a pioneer of open heart surgery.

LENFANT: Yes. He was a pioneer.

FYE: Interesting.

LENFANT: He was such a handsome man. I mean, he was just a "far west" type character.

FYE: Now your 1960 paper was published in French in the *Annales de Chirurgie* and then in English, at least with a similar title if not the same content, in the *Transactions of the American Society for Artificial Internal Organs*.

LENFANT: This was a society that had just been created. That is correct. In fact, I think the paper was presented either at the first or second annual meeting of the organization which was founded by–is the man's name Scribner or Scriber? He was at Georgetown or George Washington University. He created the society because you see that was the beginning of artificial organs. The impetus to create that society was renal dialysis.

FYE: The Kolff kidney, artificial dialysis?

LENFANT: That is right. So all the people who were fooling around, not with the artificial heart, but with extracorporeal circulation wanted to be at the meeting, and I can remember that we sent someone to it. In fact, it was Michel Weiss who came to present the paper at this meeting. I remember this very well.

FYE: So the three components of this paper, the things that I understand were unique in terms of their being combined in this, were cardiopulmonary bypass with a modified Lillehei-DeWall heart-lung machine and then deep hypothermia that was rapidly induced and rapidly reversed.

LENFANT: Do you know how we did it? By using an extracorporeal system, the Lillehei-DeWall system, that we put in ice.

FYE: Now I want you to go into each of these steps in detail. I want to note the third thing because, as I understand it from the paper, the third innovation was total circulatory arrest for up to 45 minutes. It was the combination of these three steps. Why don't you tell me how the idea came to you and your group, what you think the innovations were, and as much about those things as you care to tell.

LENFANT: Let me see if I can remember the rationale of all that. I think the idea was first to obtain total arrest. With the Lillehei-DeWall machine it was not a complete arrest. I mean, when you use an extracorporeal system there is no complete arrest unless you have an added intervention which is either potassium that really stops the heart or deep hypothermia. Here the idea was to combine the two. As I recall–now you are pushing me–not so much as I recall but as I think about it, the idea must have been that if you were cooling the body then the blood flow did not need to be as high. Therefore we could operate with a much lower blood flow and minimize the issues of hemolysis, clot, platelet agglutination, and things of that sort. Basically, the idea was to have a cardiac arrest period of 45 minutes, and that was a pretty substantial period of time to operate in. You could do a lot, especially if you had fingers like those of Dubost. I think the real thing here as I recall was probably to have a low blood flow, and then to have a quick induction of hypothermia, and make it quite deep actually, by cooling the blood, extracorporeally. The way hypothermia was induced in those days was to put the whole body in ice, and it would take quite a bit of time to lower the body temperature. I do remember that it might take close to an hour to get some reasonable hypothermia.

FYE: I assume there was a little bit of trauma to the skin and soft tissues from all this saturation in ice.

LENFANT: There must have been. I must admit I do not remember that, but I am sure there was. But that was what we did. Eventually I think that our approach was replaced by the Melrose approach, which was not as deep hypothermia and the heart was stopped, not by deep hypothermia but by injecting potassium.

FYE: I recall reading about the very earliest open heart operations where surgeons used various types of occlusion of inflow into the heart so they would have about five minutes to operate. This was before any kind of heart-lung machines, because beyond five minutes the patients had brain damage that was obviously irreversible and problematic.

LENFANT: Yes.

FYE: But this new procedure now gave surgeons not just five minutes. They would have had to have been surgeons like Dubost or some of the other very few people who had tremendous manual dexterity to accomplish anything inside the heart in that short time. They were closing atrial septal defects, but that was about it with this five-minute window. Suddenly, they had ten times as much time.

LENFANT: ... to get to the ventricular septal defect.

FYE: Right, but suddenly with your technique they had ten times as much.

LENFANT: Yes, this is correct. In 45 minutes you could do all kinds of things.

FYE: So this paper was in both a French journal and an English journal.

LENFANT: Let me say that the *Transactions of the American Society for Artificial Internal Organs* was not a journal. Now they have a journal, but in those days it was like a xeroxed copy of proceedings. Again, it was the first or second year of this society.

FYE: So the English version was abridged, I would assume. It reflected the presentation that was made at that meeting. Who made the presentation in the United States?

LENFANT: Michel Weiss. That was the first time that he was coming to the States. Because he was the first author on the paper we made the decision that he would be the one to come.

FYE: Do you remember the response, what did he say when he came back?

LENFANT: He said that people were very interested. I do recall that he had a very positive response to that presentation.

FYE: But, as you mentioned, that society at that point was primarily people interested in hemodialysis, and I guess there were probably a few people pushing the boundaries beyond the kidneys, to the heart, obviously, and other organs.

LENFANT: There was the Lillehei-DeWall [system] and, of course, the DeWall system. I think I have this proceedings. Well, I do not know if it is the proceedings or just the paper that we published but I could look into that.

FYE: I think I actually have the paper, because, acknowledging my inability to read French, I got virtually everything that you ever published in English, not throughout your entire career because I could not carry it all, but certainly I do have copies of your published papers at this point. You have touched upon the other individuals who were listed on the paper but I wonder if you could explain to me a little more about the different things that each of these people did. What did they contribute to the development of this new approach?

LENFANT: That is difficult. It was really a collective work. It was a pretty tightly-knit group of people. We were all coming from different viewpoints but the group was working very well together. It was a very unified group, and everybody contributed. I suppose that Weiss, Michel–how do you say that in French, not Weiss, it is Veiss–I cannot remember, that is terrible. But, anyway, we were working together all the time, and he was very good. He had a very good sense of clinical problems. He was older than me and he had

an extensive clinical medical training that I probably did not have. Piwnica, was a resident or something like that of Dubost, and basically he was aspiring to making a career for himself, which he did. The other guy Laurent I must admit I am a little bit at a loss at this point to know where he was coming from and all that. Sprovieri was an Italian who spent a couple of years or possibly longer than that with us. And Blondeau was really the Dubost sidekick. He was the man who was opening and closing the chest after Dubost had done his repair on the heart.

FYE: Now tell me a little about how this work was moved from the animal laboratory into patients.

LENFANT: Dubost was coming to the laboratory almost every day. Each time he was at the Marie Lannelongue hospital, he would come to the laboratory to see what we were doing. It was not to boss us around. Dubost, as I tried to convey to you the other day, had a very interesting personality. He was like a Roman emperor, a Caesar, but in a very nice way. Again, he and I had a very nice relationship.

FYE: I would like to digress a moment and ask for your perspectives on the importance of personal relationships with mentors or peers. Thinking about that time in your life and career, how important were those personal relationships?

LENFANT: They are so critical. I have always found somebody with whom I had a nice personal relationship. Dubost was certainly one. The other day I mentioned to you this man who headed the laboratory of pulmonary physiology...

FYE: Rahn?

LENFANT: No, Pierre Dejours. That was the other research laboratory in the Marie Lannelongue hospital. Eventually, I worked in his field, but there was never any warm rapport between us. He was like a stick in the mud all the time. Now Rahn, whom you

mentioned, he and I had a fantastic relationship. When I went to the University of Washington . . . first, I was in an affiliated hospital and my boss there and I did not have a good relationship. Yet, it is interesting, his son married one of my daughters, and not too long ago I went to the wedding of my granddaughter, their daughter. My old boss was dead, but his wife was there. You know I was so amazed to see that there was today this sudden lack of chemistry that existed at that time. But, in contrast, when I went to the university hospital I had a superb rapport with Robert] Bob Williams when he was alive and then with [Robert] Bob Petersdorf. Petersdorf and I really had a nice chemistry together. Looking at all the people in my current surroundings now, first, [Theodore] Ted Cooper, who was the director of the institute who brought me to NHLBI, he and I had a nice relationship. After he left, we continued to have a very, very nice relationship. But with [Robert] Bob Levy, who was the successor of Cooper, it was not as good. Very interesting.

FYE: So from your point of view, these personal relationships are critical to creating a context in which you could grow as a scientist.

LENFANT: They make you blossom. I am not the kind of person who would grow very well in a refrigerator!

FYE: Where?

LENFANT: In an icebox, I need to have that kind of warm relationship. I find it to be very stimulating. I think it is common. There is nothing unique about that. Everybody is the same, I am sure of that.

FYE: Now, thinking again about the 1960 period when you had this remarkable innovation and it was appreciated, you mentioned Weiss came back from the United States and

was pleased with the response he had heard there. Obviously, the idea was presented in France and Paris presumably and people around Europe got to hear about this.

LENFANT: Yes.

FYE: Did the group have any sense of how historic and important this combination of things was in terms of ultimately moving open heart surgery forward? Was there immediate feedback that this was big news?

LENFANT: Yes, let me tell you. We were a little conceited, there is no question. We knew that we were ahead of everybody else in Europe; whatever we were doing we were always ahead. There was a continuous flow of people coming from everywhere in Europe. Let me just give you an idea of how conceited we were; after the first ventricular septal defect closure the title of the paper reporting this case was "the first case of ventricular septal defect closure in Europe." We were not modest!

FYE: You had no reason to believe otherwise, but you figured you would stake out the ground no matter what.

LENFANT: Yes.

FYE: Fair enough. I can imagine though that you would obviously get that kind of feedback if people were coming through your laboratory to see what you were doing. That implies that they thought that this was an innovation and something they could use.

LENFANT: Yes. I do not know about foreigners but as for other centers in France, I remember the man in Marseilles, in particular. Do you know where Marseilles is?

FYE: I am going to get your map out again.

LENFANT: It is on the Mediterranean Sea. Marseilles is somewhere here [pointing to map]. So he was one visitor, and there was also a man from Lyons. I remember that every so often we would drive to Lyons to show them how to do it. Anyway, this man from Marseilles,

whose name I have completely forgotten, would come to Paris and he would be with us for a few days and then he would go back to Marseilles. Pretty soon he would show up again and he said, "I cannot make it work, I cannot make it work." We really thought we were pretty unique!

FYE: You raise a very good point though, because Lillehei, in reflecting back 35 years, emphasized how his heart/lung machine, or his approach to extracorporeal circulation and the bubble oxygenator, was simple for people to use. But there surely is more to it than just having the apparatus. You need a whole group of people that understand how to use it. You need a surgeon to use it to advantage. Do you want to comment about that? That you had a team obviously was part of the reason this worked so well.

LENFANT: Yes. But you see we had this laboratory of experimental surgery where, basically, everyone, including Dubost, was going and dirtying their hands working on dogs, and learning how to do it. I do not know how many dogs we did surgery on in that place but there were truckloads of dogs coming in all the time.

FYE: But Dubost had the advantage of having this laboratory where the techniques could be practiced and, as you mentioned, the man from Marseilles, I assume, did not have those facilities.

LENFANT: No, he did not. And he was very imperial in his behavior, like many heads of departments in those days in Europe, but in a bad way. You can be a good emperor or a bad emperor, I guess he was a bad kind. And this fellow here, what was his name?

FYE: From Lyons.

LENFANT: I do not remember. I continued a very nice relationship with him, actually. After I was in the U.S., he invited me a number of times to France because we really helped him a lot. Well, I do not remember, it is too far back.

FYE: Did you have people from the United States coming over to look at your approach?

LENFANT: Yes. I remember two or three–I became very friendly with one of them, his name was Jobe. I cannot remember where he was from, but he came and spent some months with us.

FYE: His goal in coming was to learn to use this innovation clinically?

LENFANT: I don't know. Maybe he was a rich American wanting to have a sabbatical some place or whatever!

FYE: In Paris?

LENFANT: Yes! He was a very nice fellow actually.

FYE: But you certainly got the feeling that most people who came to work in the laboratory were interested in this particular approach.

LENFANT: Yes, they were interested in what we were doing.

FYE: Did you have a sense how long it was going to take for this to be used clinically in other contexts?

LENFANT: I don't know.

FYE: You could not tell?

LENFANT: No, I could not tell.

FYE: But you had the sense that this was important, Dubost, of course, had the sense, and your group had this sense. But you could not predict that it would be six months or a year or five years before this would be used clinically?.

LENFANT: No, I could not.

FYE: I will digress briefly in terms of asking you to try to predict the future back then. Sylvan Weinberg asked, because of my knowledge of history and having written the book on cardiology, to do an ACCEL interview with me trying to predict the future. I told him I

thought it would be very difficult because I am a historian. I view the past, and I appreciate the value of history as you try to go forward, but, finally, we concluded that it was a different thing.

LENFANT: It was very difficult to predict the future because the field was moving so fast. The future was every day really, and almost every day there was something that would actually be new. . . Do you see what I mean?

FYE: Did your group feel that? Was there an excitement in the group that reflected that dynamic field that you were working in?

LENFANT: Oh, yes.

FYE: Who was the biggest cheerleader of all that? Who was the one that was most excited about all this?

LENFANT: I think Dubost was a great leader.

FYE: Do you think that was in part because he was the surgeon and could actually take it into the operating room and change people's lives based upon this technology?

LENFANT: Sure. Undoubtedly, he was the life saver, and all of us were satellites so to speak. But that was okay. We were very proud, too, of what we were doing. I think the laboratory helped him a lot in the sense that we identified and answered important questions. But I was not driven by the desire to be sitting in the front row. Now that I am getting older, I think that I blew it. I should have been more aggressive and assertive.

FYE: Now, in terms of your role, by this time is it fair to say that you had made the transition in your own mind from a surgical career to a physiological research career?

LENFANT: Yes, that is fair to say.

FYE: Again, in the work that led to this paper in1960, were you doing any of the technical things or what was your role?

LENFANT: We were working on biological questions. I remember one thing which was of great interest to me in those days was related to pulmonary circulation and coronary circulation.

FYE: But, in terms of laboratory work per se, when the dog work was being done would you have been involved in that?

LENFANT: Yes.

FYE: You were physically involved?

LENFANT: Absolutely.

FYE: Tell me something about that.

LENFANT: I was doing most of the surgery in dogs. Surgery residents or trainees would come and participate, but I was the one truly in charge of that operation. I was doing the work on dogs and then making sure that the whole operation was... It was a fairly comprehensive set of activities.

FYE: One of the terms that I use to try to describe the early cardiologists in this country, in the United States, is physiological cardiologists, and I viewed them as having a distinct intellectual tradition. It was not crisp and clear, there were lots of blurry edges there, but the term surgical or physiological surgeon is a corollary. I would think that Dubost must have been a physiological surgeon. In other words, he was not a general surgeon who took out gallbladders and repaired hernias.

LENFANT: As a matter of fact, I would say no.

FYE: No?

LENFANT: But he had a very open mind about it. He knew that the play of his fingers was not the only thing that would make him work. I know that! There is no doubt in my mind that his glory was to go and do the surgery, to be in the front row, to give interviews and all

of that stuff. But he knew that all that was dependent in part on what was going on in the laboratory.

FYE: But the type of surgery he was doing...

LENFANT: At the same time I knew, basically, that the full value of what I was doing in the laboratory was through him. I believe very strongly that research must not be for the enjoyment of the scientist but for a purpose. I feel very strongly about that. I do not know that it is such a popular view these days, but there is no question that I think first about the patients and the public health implications of what we are doing.

FYE: These are things that in later interviews we will have to go into in detail. I am very interested in what might be called the sociology of research communities and their success and failure, and also all of the very curious and indescribable components that go into that. So I am looking forward to when we can discuss that. We should think, as we have been doing in different phases of your career, about how all those things came to be. I can understand that there was a sense in your laboratory in Paris that not only were you doing research at a basic level–understanding blood flow and understanding the implications of rapid hypothermia and warming–but you had a vision that this did have potential practical implications for knowing where heart surgery was and where it was trying to go.

LENFANT: Yes.

FYE: In terms of the chronology, we are now at 1960, and that is the year, I believe, when you came back to the United States.

LENFANT: Correct. You see, I received this letter from Bob Williams. In fact, I was looking for that letter, and I just do not know what I have done with it.

FYE: I hope you have it because it obviously was such an important part of your career.

LENFANT: It was such a turn in my life. First of all, I was astounded to receive this letter. I had never met the man; I did not know where Washington State was. I had toured the country, but I did not go to Washington State.

FYE: I am thinking that it is ironic that for Washington, of course, one could assume Washington, D.C. Now, you are basically in Washington, D.C. but you went to the other Washington and not by mistake.

LENFANT: Yes. So, I was astounded. But I did not hesitate a minute. I mean, there was no hesitation. This country had a tremendous impact on me. There is no question. Admittedly, the last part of my stay in this country, when I was with André Cournand, had not been enjoyable. But I can put out of my mind things that I do not like. I can do that quite well. In fact, it is almost cynical to do that. But I have a fantastic recollection of working with Herman Rahn, being in the laboratory, and doing all of these things, and what Bob Williams wanted was somebody who wanted to do research. I suppose he had heard about me from other people. I suppose that was the case.

FYE: So you did not know anyone at the University of Washington directly that could have spoken to him. It would all have been network in some fashion?

LENFANT: No. I did not know anyone that I had met in Buffalo or New York who was from Washington State or had any contact with them. There may have been, but I do not know that.

FYE: What position did Williams offer you?

LENFANT: The true bottom of the pile, to be an instructor in his department.

FYE: But in the Department of Medicine?

LENFANT: Yes, and Physiology. In fact, it was Physiology and Biophysics, as that was a combined department. The chair of the Department of Physiology at that time was [Edward] Ted Ruch, a big name.

FYE: Yes, a very big name.

LENFANT: You know Ted Ruch was the . . .

FYE: Primates.

LENFANT: Yes, and he was the successor of John Fulton, the Yale physiology work. Ted Ruch had trained with Fulton, and he went to Seattle to create the Department of Physiology when the school was created, which was in the 1950s, I think, or something like that, just after the war. That was when the medical school started there. And, for example, the Fulton textbook of physiology became the Ruch and Fulton and eventually the Ruch textbook. I wrote a few chapters in it, actually.

FYE: I know a fair bit about John Fulton, but I did not follow Ruch's career and understand that relationship. Ruch was the head of the Department of Physiology and Biophysics?

LENFANT: Yes.

FYE: So, of course, he would have had to approve your appointment in collaboration with Williams.

LENFANT: Yes, absolutely.

FYE: What did you actually do in Seattle?

LENFANT: I went to an affiliated hospital which was called at that time Firland Sanitorium. My boss there was a man by the name of [James] Jim Martin. There was a research laboratory, and that is where I was doing my research. I was seeing some patients from time to time, and then doing some teaching at the University of Washington.

FYE: And this was a tuberculosis sanatorium?

LENFANT: Yes, but they were doing much more than tuberculosis. I mean, tuberculosis was a big part because in those days if you had tuberculosis, you were locked up. That is the way it was. But it was a former military compound and they had all kinds of wards which were for COPD [Chronic Obstructive Pulmonary Disease], and other pulmonary disease.

FYE: Was the focus of the hospital pulmonary disease?

LENFANT: Yes.

FYE: It was exclusively pulmonary disease?

LENFANT: Yes, entirely pulmonary.

FYE: And the research obviously would have been fundamentally pulmonary physiology and related research?

LENFANT: Correct.

FYE: What was the attraction for you? Why did you take the job?

LENFANT: I would say primarily that by then, as I told you earlier, commuting between Paris and Lille was getting old. The enjoyment of that was to be on the train and have a drink in the train restaurant twice a week That became passé pretty quickly. So, conversely, I envisioned being in an environment that would be different. I would live and do my research and teaching all within a few miles perimeter, and I had developed a great affection for this country. I think I was more attracted to being in this country than perhaps because of the work environment.

FYE: What was it that attracted you to the United States at that point: 1960–John Kennedy's era?

LENFANT: Well, the people, the attitude. . .

FYE: So it was really the people. You crossed the country; you drove across the United States. Were you alone in that drive?

LENFANT: No, I had my wife with me.

FYE: This is when your wife was here, and you had children already?

LENFANT: The children were in France.

FYE: That is right. You mentioned they were still in France.

LENFANT: Yes.

FYE: So you and your wife drove across the United States. Where did you travel? Now, this is not the visit you told me about before where you went to specific cities to see programs, this was just a cross-country trip.

LENFANT: By car. We had bought an old Studebaker and we did our trip between our stay in Buffalo and that in New York. We took one month off, just to travel in the country. I remember from Buffalo we went to Chicago, up the Michigan peninsula. In fact, we crossed the Mackinac Bridge . . .

FYE: The upper peninsula of Michigan to...

LENFANT: The day after it opened.

FYE: Oh, for heaven's sake.

LENFANT: Yes, and it was a gorgeous day, like here today, it was just magnificent. We drove down through Wisconsin. We went, I forgot if it is north or south, back up to Pierre, South Dakota...

FYE: I think that is right.

LENFANT: Then to Yellowstone Park and from there down to San Francisco. After San Francisco, we went to Yosemite Park and the southwest, and then back to Buffalo. We sold the car someplace, I forget where. The car was getting tired!

FYE: What an ambitious trip! You were part tourist but, at that point, had you already decided that you wanted to come to the United States?

LENFANT: Yes. But I do not think I ever told anyone that if I could find a job, I would come back. I do not think I ever said that.

FYE: Do you mean in France or do you mean here in the United States?

LENFANT: Here in the United States. But people knew that I was very enthusiastic about this country.

FYE: How did your wife feel about all this?

LENFANT: She was very much in favor. In fact, she was a bit of a driving force behind all that. I think I told you she had what was said to be multiple sclerosis and it was very clear that all the symptoms had in part disappeared when she was here. However, they reappeared later after we moved here to live.

FYE: So was Seattle the end of your driving journey?

LENFANT: No. I did not go to Seattle.

FYE: You did not know that the job in Seattle was going to exist?

LENFANT: No. I went to New York to spend my time with Cournand.

FYE: Okay, so this trip was a year earlier?

LENFANT: Yes. After Cournand, I went back to France.

FYE: But I remember that you said moments ago that it was the driving tour that added to your enthusiasm for the United States because you enjoyed the people that you met. These were just ordinary people, I assume, at gas stations and restaurants.

LENFANT: Yes.

FYE: How was your English then? When did you start to learn English? I mean, obviously as a child, but when did it become a . . .

LENFANT: No, that is not true. The first word of English I ever pronounced was when I came for my first trip to the United States.

FYE: That is extraordinary. Obviously you had to work hard to learn English in a hurry.

LENFANT: Yes. I was working hard in those days!

FYE: But, truly, I mean, to become a functioning active participant in the laboratories in which you worked. Now, you could easily communicate with Cournand in French. I assume you spoke French with Cournand?

LENFANT: Yes.

FYE: But still the personalities did not click, but otherwise when you were . . .

LENFANT: The first time that I spoke English was when I came here for two or three months to visit the surgery units in this country. That was the first time I spoke English in my life. I took German when I was in school. That was during the war so everybody was taking German in those days.

FYE: I took German, too. That is why I do not know French, I am sorry to say. But that must have been quite a challenge to come to the United States and travel all around only knowing French.

LENFANT: Yes, it was.

FYE: But people were kind and understanding?.

LENFANT: People were nice.

FYE: When you went to Washington, for example, Bob Williams obviously knew that you were learning English . . .

LENFANT: Yes.

FYE: And that you obviously were not fluent in English.

LENFANT: No. By then, you see, I already had spent three years in Buffalo.

FYE: Of course, and in New York City. So, by that time, you were fluent in English.

LENFANT: I do not know if I was fluent, but at least I could find my way and do things.

FYE: Had they had any other experience at the University of Washington with scientists or clinicians from France? You were the only one, I should think.

LENFANT: Not that I know of. There were some [Europeans], [Hans] Neurath, the professor of biochemistry was from Germany. [Edmond] Eddie Fischer, who got the Nobel Prize with [Edwin G.] Krebs two or three years ago, was from Switzerland. There were some foreign-born people there.

FYE: In fact, in many respects, there probably were many more foreign-born Europeans in science and medicine in the United States then than there are now.

LENFANT: Yes.

FYE: Because, obviously, after all of the turmoil in the 1930s, 1940s, and 1950s as recovery was still going on a number of people who wanted academic and scientific careers came to the United States. So, in that sense, you probably were in a large critical mass of European scientists and physicians.

LENFANT: But, it is very interesting, I do not really associate very much with foreign nationals. By design in a sense. For instance, at the NIH or in Washington, I understand there is an association of French people, there is an association of Italians and of Chinese, and what have you. I do not belong to any group. I do not even know where they are. I know they are there, but I do not know where. And I have no interest in that at all.

FYE: When you came to the United States in 1960 to go to Seattle, did you know that you wanted to transfer your life and your career to the United States?

LENFANT: I do not know how it is today but in those days you had to be a resident for five years before you could take the nationality. At the end of my five years and perhaps five days, I was a U.S. citizen.

FYE: I know 1965 was the year of your naturalization.

LENFANT: Yes.

FYE: I know that from your curriculum vitae. So you had a plan not only for your research and your career but also for your culture, your country, and where you lived.

LENFANT: Oh, yes. I remember I really had no tie in France because at that time I still had that very cool relationship with my parents. When my parents heard that I was coming here, they viewed that as just another one of my irrationalities!

FYE: Little did they know.

LENFANT: Well, I don't know.

FYE: That is interesting. Tell me a little more about your life in Seattle. You were at the sanatorium and you were doing research there.

LENFANT: Yes.

FYE: Describe a little of the research that was going on when you got there and how it changed while you were there.

LENFANT: That was the beginning of ventilation perfusion ratios and gas exchange. At the place in Seattle, most of the work was really in ventilatory distribution and blood flow distributions and gas exchange. It was something that I had done in Buffalo, and so I was interested in that, but what I did, and what nobody else was doing there, was to apply that to patients. I started measuring these things in patients and I also became very interested in measuring all the three gases in blood, that is oxygen, carbon dioxide, and nitrogen. I do not know if you have heard about a fellow by the name of John West.

FYE: Oh, yes. I know him. Because of my history of physiology book.

LENFANT: John West and I were competitors. By then, he had come to this country too. I think he was in San Diego. But we were competitors, and there was a race. Actually, I had begun this work in France. But there was a race to be able to measure those three gases in

arterial blood samples because it was very clear that measuring these three gases would allow us to view, to measure the distribution of ventilation and blood flow in the lung from the diaphragm to the top of the lungs. There were three groups working on that. There was me, there was John West, and then there was a fellow from Buffalo by the name of Leon Farhi. He was very well known. I know I was the first one to do it, but I published it in French. And others always ignored this citation.

FYE: Conveniently!

LENFANT: I don't know. But the fact is there. I mean, all you have to do is read it.

FYE: It cannot be disputed.

LENFANT: My paper was published in France as an abstract in the *French Journal of Physiology* and the next publication was in the *Journal of Applied Physiology* in this country, but six months later.

FYE: The dates speak for themselves.

LENFANT: Yes, the date is very clear on that. Now, it no longer matters because I do other things and others do other things–but, for years, we have had this competition . . . You see, there was a network that developed during the 1960s, a worldwide network–there was a club, in fact, it was called the V/Q Club.

FYE: The VQ or V/Q?

LENFANT: V/ the ratio, V/Q Club. I was one of the most active persons in this. When I was at the University of Washington, just across Puget Sound was a Navy torpedo station and they had a hyperbaric chamber. I had designed a study that would allow me to measure the distribution of ventilation and perfusion in the lung by using a change of barometric pressure while at the same time measuring the three gases. So I would spend a great

deal of my time at the torpedo station in the hyperbaric chamber. The paper from this research is one of the papers which I actually think is a classic in its field.

FYE: I am not as familiar with pulmonary physiology as I am with cardiac physiology but tell me a bit about the experimental design of this. What were you using in the hyperbaric chamber, for example, and then the other parts of that research at that time?

LENFANT: One of the things I had done–you see, I had to measure the three gases, and that was really also a technique that I had developed–was to use gas chromatography. Basically, what I was doing during descent and ascent was taking blood samples from these Navy men. After all these blood samples, they were not very happy when they would see me coming.

FYE: With a separate stick each time, or with a catheter?

LENFANT: No, I had a catheter. Yes, but sometimes, have you ever been in a hyperbaric chamber?

FYE: No, I have not.

LENFANT: It is rather confining and sometimes the catheter would come out. I mean, it was a mess. I was measuring the three gases with gas chromatography at all these levels, and this allowed me to develop completely the distribution of ventilation and perfusion in these subjects. This was an interesting time.

FYE: That is fascinating, because if you think about it, if the Navy had not had that hyperbaric chamber there, your research would have to have gone a different way.

LENFANT: That is correct.

FYE: You would not have had that technology.

LENFANT: That is correct.

FYE: And that, interestingly enough, I am sure, was defense department driven, probably left over from the Second World War, or maybe earlier.

LENFANT: No, they were using this chamber to train divers.

FYE: But I would have thought that it would have been developed during the Second World War.

LENFANT: Yes, it was a demand of the Second War. The purpose of that was to train divers to go and attach mines on enemy ships.

FYE: On submarines?

LENFANT: Submarines or other ships.

FYE: But then they allowed you to use this chamber for physiological research.

LENFANT: Yes, I developed very close contact with the Navy.

FYE: How did that work? Did they approach your program or did Ruch go to them?

LENFANT: No, I did that myself. I got in touch with the head of the torpedo station–gosh, that was a long time ago–and he allowed me to come. I worked with the petty officer, and then they were all very friendly. They thought I was a crazy guy, but I got a very nice rapport with them. In fact, that led me to hyperbaric research where I did lots of work on the hyperbaric tolerance and things like that. That is what led me to get to know Cousteau, for example.

FYE: Jacques Cousteau?

LENFANT: Yes. Then I also got to know another person who was a really a fascinating man. His name was Karl Schaeffer, and Karl Schaeffer was the chief medical officer in the German Navy for submarine warfare. After the war, he either was told to come to the United States like [Werner von] Braun, the man who developed the rockets, or he did it as a no choice, but, anyway, he ended up in charge of the hyperbaric chamber at the Navy base of Groton in Connecticut.

FYE: Oh yes, Groton.

LENFANT: I knew him, and he and I became very friendly, and with the Navy and Jacques Cousteau, we went to do some deep sea diving, . . . you are going to think I am crazy.

FYE: Not at all, unless you tell me you could have been a treasure hunter and stayed on your path of physiological research! This is fascinating, go right ahead.

LENFANT: We went off to Monaco where I spent a couple of months doing some deep sea diving.

FYE: Tough assignment!

LENFANT: Yes, we suffer! That was really something.

FYE: How did your friends back in Seattle view that little sabbatical?

LENFANT: Petersdorf was so tolerant of me because I would say that in the eleven years I was in Seattle, I must not have been there more than seven or eight. Yes, Petersdorf was very tolerant, but he liked it, you see. I produced things, and I think he was proud that there were papers coming from his department. He always was very protective of me. He nominated me to the young Turks [American Society of Clinical Investigators] and the old Turks [Association of American Physicians], and I suspect that he helped a great deal in my election. I was elected the first time round, in these two groups. Today, most people have to be nominated two or three times before they get in there.

FYE: Yes, it points out the importance of networks at that point in American medicine and science, because these people–I know Bob Petersdorf and have for years, certainly only as a mentor, you know younger/older scientist. Be that as it may, I do know something about the networks and how critically important they were for helping careers advance or preventing career stagnation.

LENFANT: That is how I met [James] Jim Wyngaarden. Petersdorf would take me to the clinical meetings in Atlantic City.

FYE: In Atlantic City.

LENFANT: All these higher ups in American medicine spent hours in the bar, and Petersdorf would take me with him and introduce me to a few of them. That is how I met Wyngaarden. Actually, Wyngaarden offered me a job when I was at the University of Washington which I did not take, but when he came to the NIH, he was the one who appointed me as Director of the Institute.

FYE: Wyngaarden, of course, was at Duke at the time when you first met him, and he offered you a position at Duke, but you stayed in Seattle . . .

LENFANT: Yes. He was at Duke.

FYE: Then, later on, he kept after you. We will get to that; that is fascinating. So Petersdorf, probably more than anyone else, if I understand you correctly, was responsible for pushing you and getting you known around this country by this network of other clinical investigators and scientists. What could you say about the Atlantic City meetings because they do not exist anymore for all practical purposes?

LENFANT: No, it is dead.

FYE: Could you say something about those meetings and how they affected your career?

LENFANT: They were fantastic. They affected my career a lot, because they were the meetings of the young Turks and the Association of American Physicians and by then, during that period of time, I had begun to do some work in hematology. For example, I did some work, not too much, in red cell metabolism. I had drifted into the blood by then and . . .

FYE: I like that, "Drifted into the blood."

LENFANT: Yes. The man who was the head of the Division of Hematology at Seattle was [Clement] Clem Finch, a big name. In fact, you have probably heard of his cousin who was at Yale. There was a Finch there. You said you were at Yale?

FYE: No, I just know about Yale.

LENFANT: Okay.

FYE: Because of Cushing and Fulton and those others . . .

LENFANT: Anyway, Clement Finch is in the same category as [Maxwell] Wintrobe, the big names of hematology, in the world, actually. And Clem is the one who got me interested in red cell metabolism, especially the role of organic phosphate on the hemoglobin molecule and how it combined with oxygen. I applied this observation to various conditions. One of them, for example, was angina when I did some work in cardiology. But I mostly did a great deal of work in hematology and in the role of the organic phosphates in diabetic patients. And I was invited to present at the plenary session of the old Turks or the young Turks and that made me known, people saw me. For example, it is very interesting that, a few years ago, the Association of American Physicians celebrated its one-hundreth anniversary or something like that and they published a special book for it.

FYE: Right. Green in color?

LENFANT: Yes.

FYE: A. McGehee Harvey.

LENFANT: Yes, and I am mentioned a number of times in it, which is kind of nice, I have to say. Yes, Clem was in part responsible for my spending so much time away from Washington because we then decided to look at the role of the organic phosphates and adaptation to altitude, so he and I spent lots of time in the high mountains of Peru.

FYE: So under the water off the coast of Monaco and in the Andes in Peru.

LENFANT: Yes, and I also went to the Himalayas. I had a good life, you know.

FYE: It sounds like it.

LENFANT: I have nothing to show for it, but I really had a good time.

FYE: Was the Peruvian expedition again looking into the effects of altitude?

LENFANT: Yes.

FYE: Not the barometric pressure of being submerged, but altitude.

LENFANT: Yes.

FYE: It strikes me that the transition from pulmonary physiologist through the role of the blood cell, of course, and oxygen transport, then the heart as part of this, is all symmetrical and acknowledges what we will get to probably months from now in terms of NHLBI. It does all truly fit together, doesn't it?

LENFANT: But, at the same time, the thing which will interest you–I have never really given any thought to all that until now–but I moved into new areas. I became very interested in adaptation to stress. That is what brought me to studying the effect of altitude and that is also when I developed this idea that the best way to study stress was probably to look at species which experience in their normal habitat situations that are very stressing to humans. Actually, by then, I had met this man from the Department of Zoology on a boat in the bay of Miami; this is where we got together and planned doing work on penguins, on Weddell seals, on snakes in the Amazon River, and so on.

FYE: What was his name?

LENFANT: Kjell Johannsen. You should look at my curriculum vitae.

FYE: He was a co-author with you on papers. I know I saw the work on, from my perspective, slightly more esoteric topics.

LENFANT: Right. But it was actually part of a grand scheme that we had.

FYE: Fascinating. Of course, there were other people that worked in these areas, not only full-time, I mean, zoologists and physiologists would have...

LENFANT: But what I think I was bringing into it was my medical training. You know zoologists are not physicians. And physiology is kind of a remote discipline for them. Zoology is a very descriptive discipline.

FYE: Yes. So you were sort of a physiological zoologist for a time?

LENFANT: Yes. You probably never heard of it, but we worked on a species of fish that is called the lung fish. They have a lung.

FYE: I have heard of it.

LENFANT: There are three species in the world, one in Australia, one in South America, and one in Uganda in Africa. Guess what, we went to all these places to do our work!

FYE: Really, you went to all three to compare the different species that were obviously dispersed across the globe.

LENFANT: Yes. But they each have different features because–I mean, the people who are interested in evolution will tell you that the lung fish is the link between the underwater and the terrestrial life.

FYE: Mammals on the surface.

LENFANT: Yes.

FYE: I can see them climbing out with their little back fins in these animations.

LENFANT: Yes, but the one in Africa is especially interesting because it crawls out of the water early at the beginning of the dry season and stays in the mud until the rainy season. Eventually the drought comes in, and they get buried into the mud in some sort of a cocoon where–you have heard the word hibernate?

FYE: Yes.

LENFANT: Well, they estivate.

FYE: What is that?

LENFANT: Estivate is the contrary of, the opposite of, hibernate.

FYE: I have never heard the word. That is fascinating.

LENFANT: But that is what they do. They get themselves in the mud and they stay there, and, believe it or not, studies on that species became of tremendous interest to NASA.

FYE: For trips to the moon and elsewhere?

LENFANT: Not the moon, but to Mars and so on.

FYE: Great distances.

LENFANT: Yes. For how to put people asleep for two years and then wake them up.

FYE: And have them be in one piece.

LENFANT: Right!

FYE: Again, it is interesting how defense-related or space-related, which I think is ultimately defense related, research gets married with very basic physiological interests.

LENFANT: In the lung fish, my great competitor is somebody whose name I am sure you know, if not the person. It is Alfred Fishman.

FYE: Oh, I know him quite well. Fishman and Richards, of course, wrote...

LENFANT: Fishman and I are basically the only groups, I believe, which knew a lot about the lung fish.

FYE: Interesting.

LENFANT: But I beat him to it. He does not like it when I tell him that, but I knew I beat him to it.

FYE: You were a pretty good racer. You finished first a lot of these times.

LENFANT. Sometimes.

FYE: Now, in the earliest years that you were in Washington state, you were often at the Firland sanatorium. I mean, that is where your focus was.

LENFANT: Yes.

FYE: Did you move to Seattle at some point?

LENFANT: Yes, I moved to the University of Washington. They were five to six miles apart, or something like that. I moved full-time to the University of Washington when the decision was finally made to create within the department of medicine a division of respiratory disease which did not exist before. All the people who were handling respiratory disease patients were, in fact, either in allergy or infectious disease. So they brought from San Francisco a person who had trained with Julius Comroe. His name was John Butler, and he was British. After his fellowship in San Francisco at the Cardiovascular Research Institute [CVRI], he decided to stay in the United States and eventually found a job in Seattle as the chief. What Petersdorf did was to carve space for the division of respiratory diseases, and this is when I moved to the university facilities.

FYE: It is interesting to try to speculate how tuberculosis–really the pharmacological treatment of tuberculosis–changed all of this because, as you pointed out, the people that became respiratory physicians or pulmonary physicians had either primarily a tuberculosis and infectious disease background or an allergy with various allergic respiratory problems background. But, suddenly, there was a lot more opportunity, I would think, for someone in respiratory physiology because it was not only tuberculosis. There was this growing concern about emphysema and chronic lung disease aside from tuberculosis which had all the headlines. Could you say a little about that?

LENFANT: You put it very clearly. I think this was the result of some of the work that was done in several centers in the country. Certainly, in Buffalo. I think that we in Seattle contributed to putting all this work on the map, as did San Francisco at the CVRI, and maybe in Denver. Pulmonary function tests became more routine than they were

before, and this brought a new focus as did the measurement of blood gas. Remember, in the 1950s or in the 1960s, the measurement of blood gas was an event.

FYE: A big deal.

LENFANT: Absolutely a big deal, and there was no way to measure oxygen tension. This is one thing too that I worked on a lot. There was a researcher at the University of Cincinnati by the name of Clark, his first name was Leland; he had invented what is still known as the Clark electrode to measure the partial pressure of oxygen in the blood. It was a very cumbersome piece of equipment, and I worked on it over the years and refined it. It is very interesting that, at that time, I had a friend in Denmark named Paul Astrup.

FYE: He was very well known.

LENFANT: Astrup was very shrewd. He used many of his developments to work with a company or even eventually created his own company which has built blood gas measurement devices and pH meters which are all over the world.

FYE: And has an eponym.

LENFANT: I did lots of work on the oxygen electrode.

FYE: Again, it must have been a very exciting time because your work could have gone in any number of directions. That is what I sense as we are discussing the very few years from1956 when you published your medical degree thesis to1960 when you are in Seattle with Bob Petersdorf, who shortly thereafter is touring you around the country on his arm. You were doing all of this work in the 1960s and your research could have gone in any number of ways, because, of course, open-heart surgery was continuing to explode, but you had more or less moved away from that by that time. Is that right?

LENFANT: Yes.

FYE: I mean, your identity was as a pulmonary physiologist.

LENFANT: Yes, but with no consistency. In fact, when I summarize my life I say that I have been everywhere but got nowhere!

FYE: I do not think I will even respond to that. You have been everywhere, and you are where it is at, it seems to me.

LENFANT: No, I mean, it is very interesting that the research work that I have done did not get me very far.

FYE: But you chose a different path.

LENFANT: Yes.

FYE: Eventually, we will get to that, but it strikes me that you chose a path that included more administration and more leadership in that way than continuing your research.

LENFANT: But there was a reason for it, I mean, a very specific reason. That is where Hugh Smith comes into the picture, actually.

FYE: We will tell it twice.

LENFANT: He was a fellow with us at the University of Washington, and when the Heart Institute [at NIH] became the Heart and Lung Institute, Ted Cooper, who was the Director of the Institute, sent a letter to all the divisions of respiratory disease in the country. At that time, Butler was somewhere on sabbatical, so I was the acting head of the division. Ted's letter said, "You all asked to have an identity at NIH and we now have a lung part in this institute, what do you think we should do?" And Hugh Smith and I were very friendly. We spent hours preparing a response to this letter. We were going back and forth. We would each go home and write a new draft and show it to the other one in the morning. I mean, we really came up with a national plan of what should be done that spanned education, training, research issues, and things like that. I sent the letter to Cooper who immediately called me and said, "I've got to talk to you." I had never

seen the man and did not even know that he existed. So he invited me to come to Bethesda a few times, and we would spend hours talking. Then, eventually, he asked me if I would come for a couple of years to start the National Pulmonary Research Program. By then, my family situation was pretty difficult and I said yes. I went to Bethesda on a leave of absence for two years and I am still there.

FYE: Would that have been about 1965, or was it later than that?

LENFANT: No, it was in 1969, 1970.

FYE: Okay. I am just looking at your curriculum vitae. From 1968 to 1971, you were still at the University of Washington as an Associate Professor of Medicine and Physiology and Biophysics, and you became Professor in 1972.. Then I see that from 1969 to 1970 is when you were a member of the Physiology Study Section at the NIH. That must have been about the time that you are describing just now where you probably took the letter more seriously than anyone else. Not only did Cooper like your ideas but it sounds like you put a lot of effort into articulating a vision.

LENFANT: Yes. We did, and it was very interesting to do it with Hugh Smith. I am not surprised to see where he is now, the CEO of the Mayo Clinic, because he is quick on his feet and he is a very good person.

FYE: That is comforting to me. That will be obscure to people listening to the tape, but it is nice to hear that. That is my own impression as well, by the way. Was 1969 then the first time you really had any firsthand experience with the NIH in Bethesda?

LENFANT: No, I was on the study section. In fact, I had forgotten that I was on the study section. Also, the National Heart and Lung Institute, as it was called then, would very often call me to go on site visits. In those days all training grants had site visits. I was going here and there to do site visits for training grants. But that was my contact. The study section

had nothing to do with the Heart Institute really. It was run by the Division of Research Grants as it was called in those days.

FYE: Petersdorf was still chair at the University of Washington? He was chair well into the 1970s?

LENFANT: Yes, he left after I left.

FYE: So, as you mentioned earlier, he was happy to have you doing this research and broadcasting the name of the University of Washington in physiology circles.

LENFANT: I suppose so.

FYE: I suspect also he was happy to have you being part of the study section because then you were carrying the University of Washington flag around the country.

LENFANT: Yes. And I was invited to many foreign countries to give talks and do things like that.

FYE: Even in the 1960s?

LENFANT: Yes.

FYE: Based now upon, I imagine, your increasing identity as a pulmonary physiologist.

LENFANT: Yes.

FYE: We are now in the late 1960s in the chronology of your career. By that time, your being invited to give talks probably did not have much to do with the Dubost work, if anything at all. You had gone in a different direction, is that correct?

LENFANT: Yes. By the late 1960s, I think I was associated mostly with my work on blood and also the work that I was doing in comparative physiology. I had traveled extensively in Australia. I stayed there for almost six months, and then I had traveled in South America going up the Amazon River from Belem and crossing the whole South American continent. I had been at the South Pole. I had been at the North Pole.

FYE: This is fascinating because we talked early on about your childhood and, as a young person, you moved around of necessity largely because of the dynamics of the Second World War and its aftermath. When did you decide to become such a traveler? I mean, this must be a passion. Some people would find all this exhausting, and yet you seem to thrive on it?

LENFANT: No, I do not know that it is a passion. First of all, all the travel that I did at the University of Washington, except when I was invited to do something, was driven by the wish to study something.

FYE: You had a purpose. There was a reason for being in those places.

LENFANT: Yes. I had a purpose. In fact, I do not travel like a tourist. Just to give you an idea, a few weeks ago I was asked to go and give a speech in South Korea. I got there on Sunday night, and I was back here on Tuesday. It is not uncommon that I would go to Europe. I leave Washington at 5 o'clock, I get to Paris or Frankfurt or London between 6 and 7 in the morning, and I am back in Washington the same day. So I am not a tourist. You know that some of my colleagues at the institute when they go somewhere they disappear for one or two weeks!

FYE: I suspect though that part of that relates to your success over decades. Frankly, it is your capacity and your willingness to do these very rapid trips in and out so you can have an effect or learn something, whatever the purpose of the trip may be. But then you are back at your home base planning the next place to go and learn or teach or do something. Truly, many people would think that would be absolutely exhausting and would not want to do that.

LENFANT: Yes. Well, it does not bother me.

FYE: No. But I would suggest that probably it has been part of your success. Part of what has allowed you to become what you are and have the impact you have had is your willingness to do all that traveling.

LENFANT: I do not know.

FYE: I mean, you have got a worldwide network, I am sure.

LENFANT: Perhaps!

FYE: It is one thing to meet people through correspondence or over the telephone, but it is another to visit them.

LENFANT: However, I cannot remember their names!

FYE: That is also reassuring. I know we talked about these things. I was going to ask you specifically about a 1965 publication out of the Firland Sanatorium. When you were in the Institute of Respiratory Physiology there, you published a paper with William Pace, Jr. He was your co-author, and that was on the ventilation perfusion ratio in emphysema.

LENFANT: Yes. Bill Pace was a practicing physician. I think that was a clinical study, do you have the title of that here?

FYE: Let me see. I do not have the title. I just summarized the paper.

LENFANT: I think it was probably the very first study in applying basic physiological concepts to the study of the dysfunctionality of emphysema. Bill Pace was a practicing physician, he had a practice in town, but every so often he came to the laboratory to work. By then, I was no longer too good at putting catheters in arteries, so he did that. By then, I was seeing fewer and fewer patients, and I realized that it was important to have a real doctor . . .

This is the continuation of the second oral history interview with Dr. Claude Lenfant in Atlanta, Georgia.

FYE: I was curious about the dynamics of how you met Bill Pace.

LENFANT: I think he was a friend of my boss at the University of Washington. Yes, it has to be that way.

FYE: You and he obviously saw that you could collaborate in some fashion.

LENFANT: Yes, he was kind of a fun guy to be with. Actually, he had a great passion for women! I can remember that very well.

FYE: He would come into the laboratory where you worked, but his fundamental identity was as a practicing physician with an interest in pulmonary disease.

LENFANT: Yes. He was a chest physician.

FYE: Would he have been about your age or older? He could not have been much younger than you at that point.

LENFANT: He was probably a little older than I was.

FYE: Was his background in allergy or tuberculosis or infectious diseases?

LENFANT: No, he probably was a tuberculosis guy who then . . .

FYE: Became a lung specialist?

LENFANT: That is right.

FYE: I have a great series of changing titles for the American Journal of Tuberculosis. I am not going to get them all right, but it was the American Journal of Tuberculosis from about 1917 until about 1953. Then, in about the next seven or eight years, it went through three more titles to become the American Review of Respiratory Diseases.

LENFANT: Yes, and now it is Respiratory Diseases and Critical Care.

FYE: So the evolution continues.

LENFANT: Yes.

FYE: To continue to be sure that you have a specialty home.

LENFANT: Yes. I fought very hard for all the chest physicians. It is very interesting that they did not get their identity from their specialty in pulmonary medicine. But they hooked up to critical care and that has really given them a much better identity.

FYE: Presumably through the ventilator, their sophistication with ventilation and taking care of critically ill patients many of whom are on ventilators, so they were in and out of there. Pulmonary physiology triumphs again. You published a paper with Pace–I do not know where it falls in the sequence of your publications on ventilation perfusion abnormalities–that caught my eye. It struck me that it was quite important in terms of the clinical implications for understanding emphysema, chronic obstructive pulmonary disease.

LENFANT: Yes. I do not know how much we were doing in all that, but the issue was always basically to explain the hypoxia or CO2 retention. The explanation came from our ability to recognize that the blood flow and ventilation are not evenly distributed in the lung. There are parts of the lung where you have a very high VA/Q ratio and other parts where it is a very low VA/Q ratio, and that is where the hypoxia is. So the distribution of this ratio has been the object of considerable research. When you look at the evolution of medicine, say 100 years ago, the diagnosis was the result of physical examination. That was what it was. The physician would look at somebody from right to left and up and down and whatever and through this process of examination would make a diagnosis. Then, I think, what we have seen after the war is a focus on dysfunctionality in disease.

FYE: Physiological pathology?

LENFANT: That is right. What is happening now is that we are using molecular and cellular medicine to understand what causes the dysfunctionality. It is very interesting. I am saying this because this is what I said this morning when we talked about heart failure and this is really what is happening. We are trying to study at the molecular level what we know about disease, from its appearance to the most exquisite intimate disorders. Anyway, what this paper was about was to provide an explanation for the hypoxia or CO2 retention in these patients. You probably do not remember this, or you may not ever have heard of it, but all these patients with emphysema were described as either blue bloaters or pink puffers.

FYE: Blue bloaters or pink puffers!

LENFANT: This was the way to explain what was the difference between them.

FYE: Actually, I graduated from medical school in 1972, and so all of this work was front page news when I was in medical school and physiology and in clinical medicine. I did my residency at New York Hospital where there were pulmonary physicians. [William] Bill Briscoe was a pulmonary physician.

LENFANT: Yes, Bill Briscoe.

FYE: He was with Cournand.

LENFANT: I mentioned him the other day, Bill Briscoe.

FYE: That was the same Bill Briscoe who was at New York Hospital? That has had laryngeal cancer?

LENFANT: Yes, he died from a cancer of the larynx.

FYE: And Ted King was from . . .

LENFANT: Japan or Korea, something like that.

FYE: Southeast Asia.

LENFANT: I understand that he is still there.

FYE: I was hearing this from people who worked with Cournand or were disciples of people who worked with Cournand, and so I had some sense of the excitement and this thrust at that point in my career. Once again, you moved from a very exciting area–my cardiological bias keeps me thinking that open heart surgery is a little more exciting than emphysema–but, nevertheless, these were extraordinarily important advances in understanding the pathophysiology of an extremely destructive and prevalent group of diseases, all of these chronic lung diseases, and you were there on the cutting edge. Which was more exciting? Is there a way to talk about that? You shifted focus and yet you were still in a new and growing dynamic area.

LENFANT: It is very easy for me to find things exciting, so I do not know that I can say which area was more exciting than another. I think everything is exciting.

FYE: I do not know how to frame the question, but were you still watching open heart surgery? I do not mean physically, but were you watching such surgery evolve as a result of the work you did or had you more or less transferred all of your intellectual curiosity to other areas?

LENFANT: No, that was past.

FYE: Heart surgery was developing somewhere else and you were not part of that.

LENFANT: Yes.

FYE: You were off on another trajectory and that was lung disease and pulmonary physiology.

LENFANT: Yes. And then the blood. The blood was a big thing in my life, really.

FYE: Now I have a paper to refer to published, I think, in 1969. It is #77 on your curriculum vitae. You published a paper titled “The regulation of hemoglobin affinity for oxygen in

man." Interestingly enough, that was in the Transactions of the Association of American Physicians, so that would have been the old Turks of the Atlantic City meetings. In the summary there you simply talked about the tissue hypoxia that regulated the affinity of hemoglobin for oxygen. Could you say something about that research?

LENFANT: That was pH related. I guess it was the interplay between the pH and the organic phosphates, the 2-3 DPG [2-3 diphosphoglycerate].

FYE: Yes, sure.

LENFANT: Basically, this interplay would shift the oxygen dissociation curve one way or another and in so doing would make the transport of oxygen either much more efficient or less efficient. This is the thing that I was interested in. Actually, it was, in part, the result of the work that we did in high altitude where, because of the altitude-related hypoxia, hyperventilation occurs and the CO2 goes down. So this was the kind of thing that we were interested in. What was quite interesting in the early 1960s was that there were a couple of biochemists at Columbia University by the name of Benesch [Reinhold and Ruth]. The wife, Ruth Benesch, was...

FYE: Involved with Cournand? Wasn't she involved with congenital heart disease in some fashion?

LENFANT: No, they were biochemists.

FYE: There must be another woman with a similar name that I was thinking of.

LENFANT: They were the ones who discovered organic phosphates in the red cells. They were actually both from Germany and migrated to this country after the war. They had shown how the pH–they were doing all that in vitro–was actually changing the organic phosphates, and in so doing changing the affinity of the hemoglobin for oxygen. What Clem Finch and I did was to use that observation in the test tube and take it to humans.

We were the first ones to publish on the 2-3 DPG in humans, and the way we wanted to magnify this was to do it at high altitude because there you have these major changes in pH and in oxygen saturation. Basically, what we described was that the change in pH was compensated by a shift in the oxygen dissociation curve which was caused by the 2-3DPG. That was it. That was a very interesting observation because, believe it or not, it led to using Diamox. Do you know what Diamox is?

FYE: Yes, I do.

LENFANT: It is for adaptation to high altitude, and here, too, I am sure that we probably lost an opportunity to become famous!

FYE: Well, I would trace this oxygen dissocation curve tradition, the intellectual tradition, back to Haldane and Barcroft . . .

LENFANT: And Bohr and all these big names.

FYE: Right, but then, of course, you took it in a new direction.

LENFANT: What we did was to explain its regulation. Basically, this shift of the oxygen dissociation curve is an adaptive mechanism and this was what led us to look at species which are hypoxic for a long period of time such as seals or whales. This was what led me to take blood samples from whales.

FYE: That is fascinating. Paul Dudley White listened to their hearts, and you took their blood. Tell us about the whale story.

LENFANT: That was in Seattle when they captured the first killer whale. Do you remember it?

FYE: I do not remember that.

LENFANT: That was in the 1960s when killer whales were viewed as ferocious animals–eating people, fishermen, seals or whatever. I think it is true that they sometimes eat seal pups, but I do not think they eat people.

FYE: Sounds like Moby Dick.

LENFANT: Yes. They had been accused of eating people. But, anyway, it was very interesting to get arterial blood from them to see what we could do in vitro and to see how this blood would respond to change in acid condition or acidity and things of that sort. So I went to get some blood in the flaps of the whale.

FYE: What is it called?

LENFANT: Flaps, which is their tail, if you want.

FYE: Okay.

LENFANT: Yes. There is an artery that you can see. If you orient yourself you can see the artery just under the skin, and what I did was to go ahead...

FYE: Now does this go back to Jacques Cousteau?

LENFANT: No, this was in Seattle . . .

FYE: Did you use one of the self-contained aqua lung things or how did you get under water to get to this whale? Or did you have it out of the water?

LENFANT: We had the whale in the water, but in such a way that I could get the blood from the tail without getting underwater myself.

FYE: Without getting wet. In fact, that reminds me, I wanted to ask you quite a while ago about the barometric chamber, I am blocking the name.

LENFANT: Hyperbaric chamber.

FYE: Were you actually in the hyperbaric chamber with them?

LENFANT: Yes.

FYE: So you were physically in this little confined space with these naval people?

LENFANT: Yes, I took blood samples at a depth of 200 feet.

FYE: That is quite amazing.

LENFANT: I had a good time doing that.

FYE: This was still considered pulmonary physiology, wasn't it? I mean, the 2-3DPG, the oxygen dissociation curve, and the understanding that you were contributing to that. This was not hematology research, it was physiology research.

LENFANT: But, you see, hematologists were very interested in that, especially the blood bank because this played a role in it. I must admit I do not quite remember all this but it had some implication on how functional you would keep the stored blood.

FYE: How well it would work after a period of time.

LENFANT: That is right.

FYE: I can understand why that would be important to those people.

LENFANT: The blood bank in Seattle, for example, was absolutely fascinated by this work.

FYE: So, at an early stage, you had woven together pulmonary physiology, cardiopulmonary physiology, hematologic physiology . . .

LENFANT: Cardiac surgery, all that stuff.

FYE: I mean, it was all woven together in a very short period of time, within about a decade. It was less than that really because the paper on the hemoglobin affinity was in 1969, and that was less than a decade after you had been with Dubost at the end of late 1960. Now, at this point in 1969 and 1970, you were a member of the Physiology Study Section at the NIH and, from 1969 to 1971, you were doing all of these different researches. At the same time, you were a consultant at the University of Washington.

LENFANT: No, I went to the NIH. In fact, I think I got to the NIH in the week after Thanksgiving of 1970.

FYE: 1970?

LENFANT: Yes, because my first pay from the NIH was in November. I do remember that I drove across the country to come to the NIH, and I remember driving during Thanksgiving. But I was on a leave of absence from my position at the University for two years.

FYE: From the University of Washington?

LENFANT: Yes. My position at the NIH was Associate Director for the Lung Program. This was the title that Cooper gave me. For about a year or so, in fact, more than that, I was commuting to Seattle very often to finish up my laboratory work, and also my family was still there. It was a kind of disunited family but they were there. I was going maybe every other week to Seattle, something like that. When I had left Seattle for two years at the NIH, I was on the cycle for being full professor.

FYE: Of physiology and biophysics.

LENFANT: Yes. And medicine.

FYE: It was always a joint appointment.

LENFANT: Yes, to be full professor. I was tenured at that time, but as it was in Seattle there was no tenure salary to speak of at the University of Washington. I became full professor while I was at the NIH, and shortly after that Petersdorf said, "When do you come back?" I had a lengthy discussion with Ted Cooper and, by then, I had created a research laboratory at the NIH. I created the pulmonary branch of the intramural program. So I was doing some clinical work there. In fact, I was seeing more patients than I had seen for a while.

FYE: This was at the NIH Clinical Center in Bethesda?

LENFANT: Yes. I created the pulmonary function laboratory. All that was quite exciting and then again my family life had really turned sour, so I decided to resign from my appointment at the University of Washington.

FYE: So professional and social things came together to encourage that career move at that time.

LENFANT: Yes. And it was really an exciting time. Ted Cooper was a fantastic guy, and I liked him a lot. Unfortunately, he left a year after that, but . . . so be it.

FYE: Tell me how you met him? You have mentioned him several times.

LENFANT: That letter, you see.

FYE: He spontaneously sent a letter. That is right.

LENFANT: To everybody.

FYE: And you and Hugh Smith responded.

LENFANT: That is right.

FYE: And he was very impressed.

LENFANT: I suppose. But it so happened that Ted Cooper had a life which was very parallel to mine. We were exactly the same age, our birthdays were within one week of each other. He, too, started as a surgeon and then went into pharmacology. He did his pharmacology at the University of New Mexico and was brought back to NIH by . . .

FYE: Shannon, maybe?

LENFANT: No, not Shannon. It was the director before Marston. I forgot whomever this was. Wait, it may have been Shannon. Shannon may still have been there. Anyway, Cooper was brought back . . . I do not know who the director of the Institute [NHLBI] was then– let me think, it was Fredrickson . . .

FYE: Donald Fredrickson.

LENFANT: Yes, he was the director of the Institute for a short period of time and when Fredrickson stepped down, then Cooper was appointed.

FYE: I want just to be sure that we have painted a fairly full picture of your years in Washington. We have talked about a number of your research interests and we have talked about the institutional context. What other things would you like to say about those years in Washington because you were there quite a while?

LENFANT: All I can say is that those were extraordinary years which saw the convergence of all kinds of things. Not least, and perhaps more importantly, the extraordinary tolerance of Petersdorf to my rather lunatic behavior to go all over the place and be away very often. And the fact that I connected so well with Clem Finch and Bob Bruce, the chief of cardiology.

FYE: Robert Bruce.

LENFANT: [Robert] Rushmer was there, too.

FYE: The physiologist.

LENFANT: Yes, and we had a very nice rapport. My stay there was basically a little more than ten years. I think, intellectually, my career did explode, and I did not have to look for things to do research on. They really all came to me. My involvement with the Navy, the work that I did on shock that the Navy called on me for. I was a consultant for the Navy. In those days it was the beginning of the idea that maybe hyperbaric hyperoxygenation would be good for the treatment of some cancers, and I did lots of that work at the Swedish Hospital. Now it is called the Hutchinson Cancer Center.

FYE: What is it?

LENFANT: Hutchinson. I think this is what it is called.

FYE: But it is in Seattle.

LENFANT: Yes, it is in Seattle. It used to be the Swedish Hospital in those days, and I would go there. We were treating leukemia and Hodgkin's disease by doing irradiation while the patients were in a hyperbaric chamber and sure enough it worked, but there was a big problem. It was time to publish this work when I went to the NIH but I never did. I threw the research data out, actually. Over the years I must have thrown out maybe five, six, or more books of data that were never published. But the reason why we stopped this research was because the patients were all developing pulmonary fibrosis. So we had treated the Hodgkin's disease or leukemia or whatever, but the patients had very serious pulmonary fibrosis that was due to oxygen exposure or oxygen toxicity.

FYE: As a result of the hyperbaric oxygen, which had an unexpected effect on the lung. I have to say from my perspective as a historian, having studied the whole twentieth century quite intensively, clearly the 1960s were the golden era of academic medicine. I do not mean that it had hit its peak or that it did not hit its full stride, but it was a period of opportunity that no one could ever have imagined a decade earlier. We have not talked about your grant sources, but I suspect that Petersdorf had no problem supporting these endeavors because money was so easy to come by.

LENFANT: Yes. But Petersdorf never gave me any money. I got a grant from the NIH and from the NSF [National Science Foundation].

FYE: Directly?

LENFANT: Yes.

FYE: So, at that point, you always generated your own grant support?

LENFANT: From the day I got to Seattle, I applied for research grants and I got them. One of my fellows who was working with me still has my grant at the University of Washington.

FYE: Really?

LENFANT: Number 30 or 35 something like that. It is one of the oldest grants of the institute [NHLBI].

FYE: Remarkable. Is it fair to say, however, that, in that era, it was easier to get research grants and ideas funded?

LENFANT: Yes, that is right. The NSF supported me to go to the South Pole and do work on penguins.

FYE: We have talked about whales, we have talked about the hyperbaric chamber, what is the penguin story?

LENFANT: There I became very much interested in egg physiology and, of course, the penguins dive, and so it was very interesting to see the role of hypoxia. I used newborn penguins to look at the function of fetal hemoglobin which penguins keep for a few months after they hatch.

FYE: That is what it is there for.

LENFANT: Yes. So I worked on penguins and also on seals. Then I was also very much interested, it is crazy really, in ice fish. You have probably never heard of ice fish.

FYE: No.

LENFANT: Ice fish are fish which have no hemoglobin or hemocyanine.

FYE: None at all?

LENFANT: No, because they live in water which is so cold that lots of oxygen is dissolved in their plasma and they do not need any hemoglobin or pigment. So I published extensively on that.

FYE: I have to ask you a question. Do you fish?

LENFANT: No.

FYE: I mean, you have all these experiences with these extraordinary types of fish.

LENFANT: No, I do not fish. I drew a curve one day which was very interesting. Let me see if I have it here. It was hemoglobin concentration versus cardiac output and using all the species . . . Oh, this is something else too. At one time I spent some time in Colombia in Medellin

FYE: Now the drug capital of the world!

LENFANT: Yes, the drug capital of the world. I was practicing medicine there, believe it or not, and treating women or, I should say, studying women who were suffering from hookworm anemia.

FYE: Hookworm anemia.

LENFANT: Yes. I will always remember seeing a woman one day who was pregnant coming to the clinic with a hemoglobin concentration of 3 grams. You could see through her. I mean, she was almost transparent. It was amazing. But I established a curve including all these women and the various species. Some of these women I am talking about they were somewhere here [on the curve], and here, at almost zero hemoglobin concentration, was the ice fish.

FYE: Remarkable. That is how it had no hemoglobin, but it survived. I think we will bring this interview to a close with discussion of this Washington state period today, but are there other aspects of the experience in Washington that you would like to remember.

LENFANT: No. All I can say it was a fantastic time for me.

FYE: What was it like to live in Seattle?

LENFANT: I loved it. Three of my children still live there.

FYE: I think we will stop for this session.

LENFANT: Yes, you must be tired of me.

FYE: No, not a bit, but I think that we should close. Thank you.