Richard DeWall
Narrator

David Rhees
Interviewer

October 28, 1998

The Bakken Library and Museum
Minneapolis, Minnesota

DR: This is David Rhees. I am here with Dr. Richard DeWall on October 28, 1998, at The Bakken Library and Museum. Once again, it's a pleasure to have you here, Dr. DeWall.

RD: Thank you.

DR: I wonder if we could start by just getting some general biographical background, your early upbringing and education.

RD: How early?

DR: Where were you born?

RD: I was born in Appleton, Minnesota, and, I guess, I was three and a half when my dad, family, and mother moved to Morris. So I had all my formative years, grade school and high school, in Morris and grew up there.

My dad died when I was fifteen, I think it was, at the age of fifty-three, from a surgical blunder. A local G.P. [general practitioner] in the hometown fancied himself a surgeon, but he didn't know what he was doing. My father had a bowel obstruction. It's a long story in itself. But he had an appendectomy from, again, another G.P. surgeon fifteen years before in Appleton.

At that time they were using talcum powder for gloves. Of course, talcum powder is tremendously scar-formative. It will get a great inflammatory reaction. So apparently that's what it was. After the appendectomy and putting the intestines back in the abdomen, there was a lot of talcum powder on the intestines. That got him all knotted up and he had a bowel obstruction.

I suppose that was part of my thoughts about medicine, because I was so incensed about that. As it turned out later on down at the University of Minnesota, right at that time Dr.

[Owen] Wangensteen was getting a worldwide reputation for the treatment of bowel obstruction with his nasal gastric tube and his other therapy.

This is five hours away via car, but that—I hesitate to call him a doctor, even—operated on my father and didn't know what to do. So he just closed him up and put him up in a bed and let him die. He was still obstructed, of course, because he didn't know how to deal with it. But, of course, they were doing it on a regular basis at [the University of] Minnesota at that time. So that probably got my thoughts started. The directions of medicine certainly came to mind more so.

DR: Did you have any early interest in science?

RD: I was a pretty good science student. I was a pretty decent student all around. I liked everything. I liked the science.

In high school I was pretty active in all the activities, in band and chorus and athletics, football and track particularly. I was president of the senior class and of the Student Council. So I had a very exciting [time in] high school, other than the fact that my dad had died a couple of years before. So that was always kind of a bitter pill to carry along.

DR: Did you have brothers and sisters?

RD: I have one brother thirteen years older and two sisters nine and ten years older. So I was kind of the baby of the family. I often wondered whether I was an afterthought or a mistake. But whatever the case, after I came, there was nothing they could do about it except put up with me, so—

DR: So you went to the University of Minnesota then after that?

RD: Well, I was a little young for my class. I just turned seventeen by the time I graduated from high school in 1944. So I went immediately down to the University and I got in almost a year before I was eighteen and was drafted. I wanted to go into the V-12 program, the Navy and so on, but I couldn't pass the eye physical. They said I could go to the ASTP [Army Specialist Training Program] Army program, but I decided I didn't want to do that. And fortunately, I didn't, because six or eight months later they abandoned the ASTP program, and everybody that was in it was sent to the Philippines. So that was a pretty bloody engagement for everybody that was in the ASTP program at that time.

So I just waited until I was drafted, and went in the Navy. I had almost a year of undergraduate [work done] in those two quarters, summer and fall quarter at the University, and went in January of 1945 and then was in the Navy. I was scheduled to go into the RT program, radio tech program, there. I was only in the Navy about nine to ten

months, something like that, and the war was over. So it was just getting everybody out.

DR: And then you came back to the U?

RD: Then I went back to the U, and I just went straight through from that. Let's see. January of 1946 I went back again and went straight through without taking the summer off for vacation or anything until I left Minnesota, I guess, in 1962, stayed right on through. There didn't seem to be any point in taking off. Of course, I had the good fortune of having the vet[eran]'s program paying college tuition. So I had a good bit of my education at government expense. So I didn't have to take off and work in order to survive. It was enough to get me through, and a little subsidy from home. So I had a very fortunate time of it and I could concentrate on what I was doing.

DR: What did you major in as an undergraduate?

RD: I was really sort of pre-engineering, because that was kind of my inclination at the time. I like that kind of discipline, except I really wasn't that good in math. So I took the easy way out and went into biology and premed ultimately and medical school. I guess ultimately before I went into medical school I was a bacteriology major, but that was only for a short time. Of course, in 1948, when I went into medical school, the fall of 1948, all the vets were back from the war and it was quite competitive, so I wasn't sure I was going to be able to get in or not. But I did get in. I guess things turn out the way they're meant to be, because it's worked out pretty well for me.

DR: Did you have any mechanical-type hobbies that got you interested in the pre-engineering program?

RD: Not by then. I was just too—well, I did, before, I suppose you might call piano and music a hobby. I was in the University band before I went into the Navy, the University marching band, and after I got out of the Navy I took piano lessons during my undergraduate years, just for the pleasure of playing. I was a terrible musician, but I enjoyed it. I just couldn't really practice enough or concentrate enough on it to develop a good expertise. But for personal pleasure I guess you'd call that a hobby.

DR: So you went straight through and got a Master's degree en route to the M.D., is that correct?

RD: Yes. It was part of my residency program. But I had enough credits for both the Bachelor of Arts and the Bachelor of Science degrees. So I got the double degree there. I suppose another aspect of it—that was a little later on. I finished medical school. The Korean War was on. I'd known of a number of people—if you didn't have two years of service in World War II, you were called back into Korea, and I didn't have two years. So as an M.D., I thought I sure as heck was going to be called back, so I might as well do

what was more attractive to me. So I took a United States Public Health Service internship, which at that time was a uniformed military service that took care of Coast Guard and Merchant Marine and Public Health Service duties, and I was at Staten Island at that time. When I was in the Navy—this may be relevant, I don't know—I developed rheumatic fever.

DR: Rheumatic fever?

RD: Yes. And was in a convalescent hospital for two months with mitral valvulitis, which never amounted to a whole lot. Part of that, also with a combination of my father's demise—that and during my internship, one of my patients was a young Portuguese seaman, must have been in his early twenties or late teens, was literally dying of rheumatic valvular disease and it was uncontrollable. Of course, then there was nothing you could do about it except treat them with the usual useless pills. I suppose I was thinking about that then, that you have a heart, it's a mechanical organ, and you have a valve that's defective, it's a mechanical problem, there surely has got to be an answer to this kind of a situation. So I suppose that thought stayed with me. In fact, I know it stayed with me.

When I got out of the service, I had expected to stay on for another year or two, but they were just pretty well letting everybody out of the service, just cutting back all of their personnel. So I ended up coming back to Minneapolis without really a job or anything. So I got a job up in Anoka, working with another group of general practitioners up there, the Mork brothers in Anoka. I was with them about six months.

But I was just thinking of a problem that was on my mind, the mechanics of the heart and valves. So I thought, without realizing how to get there or find the final solution, but at least looking at designs, I took a piece of plaster of paris and carved what I thought would be a possibility for a prosthetic heart valve. I took that over to [Dr. Richard] Dick Varco at the University, who was one of my professors during medical school. I remember sitting on a litter outside the operating room where I was able to track him down and talk to him about it, and showed him my little carving, and told him I was interested in laboratory medicine. I really wasn't looking at a residency at that time or anything. I was just kind of interested in, say, laboratory medicine.

He said, well, he thought that he might be able to help me out there. This was in late February or March of 1954. So he said, "[Dr. C. Walton] Walt Lillehei had a research lab doing laboratory medicine and a couple of residents working down there, Herb Warden and Morley Cohen." And he thought I could maybe go down there and work if I wanted to.

The interesting thing about it was at that time of year and with all the other factors, I wasn't on the top of my class, certainly, in medical school. There was no residency

position available, especially at that time of year. So he said they didn't have a residency budget to pay me, but they could put me in the laboratory on an animal attendant's budget. That paid about $250 a month. The residents were only paid $50 a month or something like that. So I thought that was great. I was married and had my first child, and it gave me a subsistence living. It was ideal. So I went into the laboratory.

By then, Morley Cohen had moved on into the residency program. So I worked with Herb Warden on all the cross-circulation research and so on. He taught me a great deal about perfusion physiology. Also E.B. Brown at that time, respiratory physiology, and how it was relating to the donor situation and the patient and the cross-circulation. So I was running the pump, and I actually ran the pump on the cross-circulation, I think, for all of the cases except for the first two. So I was essentially the first full-time perfusionist, I guess. So that was my way on into it.

Walt, I mentioned. I don't even remember when it was. 1954. Maybe early 1955. Maybe it was late 1954. It might have been the last half of 1954. After a good number of cross-circulation patients, Walt—that's Walt Lillehei—just mentioned to me one time that it obviously would be nice to have an artificial donor instead of using a live person. You would eliminate a lot of blood-matching problems and just the general problem. He said if I was interested in it, I might look into it.

DR: How did that problem come up?

RD: Well, we were just talking one day, I guess, about running the pump and how it was going and how the set-up of it was going. Of course, I had to sterilize it and get everything made up before every case. Walt—I don't remember when he mentioned that, but he said he had a friend of his that was making polyvinyl tubing for the food industry, and if there was some way that I could use plastics in an oxygenator system—actually, the term "oxygenator" came up—to replace the donor, I might look into that possibility.

Also there was quite a bit of discussion up to that time, about the use of a bubble system. Oxygenators had a very bad reputation. They were historically very bad for a number of years. It just didn't work. Apparently people couldn't get the bubbles out and so on. I don't think that was the whole problem. Later on, I think it was just a lack of understanding of perfusion physiology that contributed to their failure. But I don't know. I wasn't there.

Anyway, with this admonition that bubbles were bad news, I tried to find some innovative ways for filming, because at that time that was all that was available, the two possibilities to get a blood-gas interface were filming and bubbling. In other words, laying a layer of blood out on the film or else blowing bubbles, so to speak, with oxygen.

So it occurred to me that you could avoid a lot of the difficulties of filming and you could

have a real thick film of blood if you would expose it to three atmospheres of gas, three times normal pressure of oxygen, because this would provide enough oxygen and solution in the plasma to account for the normal arterial venous difference, and you wouldn't even have to worry about the hemoglobin and oxygenating the red cells. They would soak it up through the plasma very quickly. So it would be an easy way to get the oxygen into the blood.

DR: The first idea of using pressurization, pressurized oxygen?

RD: Yes. That's what I did.

DR: Nobody had used that before?

RD: Not that I know of. I haven't heard of any since either. So, the quantities, pieces of hose, I had a six- or eight-foot length of hose, put an adapter about at the junction, in the middle and the lower third, that would serve as an oxygen port or a gas port that could put oxygen into the system. And I put a valve on it, a pressure valve, just tied it on to an ordinary D tank of oxygen and put a pressure valve on it so they could regulate the pressure.

Then I fixed a vent at the top of it to let the gas off of it but still keeping it pressurized. That way I could have a flow of oxygen that had three atmospheres of pressure to this exposed film of blood. Of course, to have it in a long hose and climb the hose to about thirty or forty degrees so the blood would run down it. I didn't have to pump it down or anything else; it would just flow down by gravity, and kept the lower third of this tube full of blood in order to have a reservoir to pump it out, because if you kept it right down at the bottom, you might pump out air. So you had to have a reservoir at the bottom that you could control in order to evacuate the tube as your oxygenation was completed. Of course, the vent at the top was not only to decompress the oxygen, but to vent off the excess carbon dioxide. So it was to carry out essentially respiratory, most of the respiratory needs of the blood, for that short-term purpose.

I was able to do that. I had the system working pretty well, except the problem came that when I decompressed the oxygen to go into the animal, the whole thing would end up with a little bubbling, because your oxygen would come out of the serum and it would bubble. But then I noticed that as it was being decompressed, when I took the pressure off the system, the bubbles would rise to the top of the layer of blood in the reservoir and the reconstituted blood would slide underneath. Of course, bubbles within the blood had a lighter density than reconstituted blood, so it would float. So it would actually skim it off, just a separator.

So then I figured, well, if you're going to have to deal with bubbles, might as well learn how to live with them. So, using that as a concept, this long hose as a bubble trap, I

thought, well, I'll just blow some oxygen into the blood and bubble it in the ordinary fashion. I think this is fortuitous also, because a lot of the previous bubblers had used micro bubbles, putting oxygen through so-called sintered glass filters. So you'd just get a foam being produced, and that's much more difficult to deal with than large bubbles. So I took, I think it was either twenty-two or twenty-three hypodermic needles and put about fifteen of them through an ordinary black laboratory cork and fixed a little chamber that I could just pump oxygen under ordinary pressure through these needles and, of course, put that cork into the bottom of a long vertical tube and that would be my bubbler, the oxygenator. That way I could get the blood into it. Then I'd dump it all into this inclined hose, but being seven feet long, six feet, whatever it was, it was clumsy, so I just put it in a coil, which was the same difference. So that's how the helix came about.

Once I got that system thought out and working, it seemed to work very well. I tried it on ten dogs. Opened the chest, put them on a complete bypass on it, isolated the heart, and ran it for a half hour, and when it was done, I'd disconnect the system and sew them up again. The first ten dogs I did lived and looked very good. After that, I told Walt about it and, of course, he was very interested. He came down and looked at the dogs. We got them all out of the cage and marched them around, and actually I think they took some movies of these ten dogs. I have no idea whatever happened to it. Maybe Walt still has the movies. I don't know.

DR: He probably does.

RD: Yes. So it was very encouraging. Again, that was the great thing of working with Walt. He would never tell you what you had to do or when you had to do it or how you had to do it. He just said, "Here's an opportunity. If you're interested, try it." And that was the sum limits of my directions.

And, of course, my financial needs for equipment and so on was very limited. I had the scientific apparatus shop make the adapter for the oxygen input when I was using the hyperbaric system, and they made other simple adapters. But this was a minor expense. Of course, all the plastic hose I used was pretty cheap disposable hose.

Another aspect that turned out to be very fortuitous, at that time glass had a very adverse effect—I'm not sure of the details now, it's been so many years since I've thought about it—an adverse effect on the clotting mechanism of thromboplastin in the blood and it would destroy it. So, people that had used bubble oxygenators before, using glass systems, of course, they're destroying the clotting mechanism as part of it also. But the plastic, the polyvinyl, did not have this effect. So I lucked into avoiding glass and getting into a plastic system, so I didn't have that problem to deal with.

DR: Were you aware at that time of this problem with glass?

RD: Shortly after. It wasn't immediately. That wasn't a concept in mind when it was first put together, just ultimately putting odds and ends of thoughts together that it started to stand out.

Also, using the large bubbles that I was making and putting them into another tube, the large bubbles, I could do it, they could get in there, but it would be sometimes a little difficult to get the bubbles to flow into the coil, because at that time the only hose that was available was a one-inch diameter, internal diameter. I'm not sure how it came about, I know it was a paper by Frank Gollan, and who mentioned the use of a defoaming agent, Silicone A. Or Marv Gleibman, who was working with John Lewis in the laboratory next door. Actually, I think Norm Shumway was working with them, too, there. Marv may have mentioned it to me. So I don't know where that thought came from. But I wouldn't discount that maybe Marv led me on to it or maybe it was just from the Gollan paper.

But whatever the case, I did incorporate a silicone coating on the inside of the upper end of the chamber just to coalesce the bubbles and get them in. Now, that wasn't debubbling completely, but it was at least coalescing them enough to get them into the tube.

DR: So silicone is the same as the antifoaming agent A?

RD: Yes. That's Dow Corning Antifoam A, it was called.

After that, the blood banks started to—and there's no cause-and-effect relationship. I think they came upon it themselves, and maybe that's where I heard about the thromboplastin problem. But the blood banks started to use silicone-coated glass bottles in order to give a barrier between the glass and the blood that was donated. This was before they were using plastic bags for the collection of blood.

DR: So that would both prevent the problem with the thromboplastin and also coalesce the bubbles?

RD: No. The blood banks weren't concerned about bubbles. No, they were concerned about foaming. It was just a protection from the thromboplastin. And I don't think they were using Dow Corning Antifoam A. It was another silicone type of coating that they used, but it was the same difference. The Dow Corning Antifoam A was a very heavy paste. So in order to get a surface to expose the bubbles, ultimately I just got some stainless steel Choreboys.

DR: Choreboys?

RD: Yes. Out of the grocery store, that you scrub pots and pans with. These scratchy steel sponges. And cleaned them all in ether to make sure they're totally grease-free, and dipped them in a silicone solution. You'd take this paste and make a saturated solution in

ether and then you would dip your sponge in it and pull it out of the ether solution and let them dry. So you'd have your sponge well coated with the Antifoam A. I stuck that in the chamber and that helped coalesce the bubbles more quickly and to get into your helical reservoir, which would trap any residual bubbles that you had. That worked out to be a pretty safe system.

The first system I worked on, I restricted the amount of flow that I thought I could process. I think it was 600 cc per minute, a minuscule amount of flow. I thought I could take, maybe, an infant up to six or eight kilos or something like that, a small child, an infant. I talked to Walt about it. He said, well, he was ready to go, and give it a try. We got a few little infants. The first couple died. One of them because it was a transposition of the great vessels. There's never been a success in that operation before, a transposition of the great vessels. That one died.

The second one, as I recall, was a ventricular septal defect with a balanced shunt. Pulmonary artery pressures were equal to the systemic pressures because of the communication within the septum. Of course, that's very destructive to lungs. I think both of these infants survived the procedure, kind of started to wake up afterwards. The transposition [patient] died fairly soon, but I think the pulmonary hypertensive patient lived for a day or so or something like that. But anyway, it proved the fact that they could survive the perfusion. That didn't discourage Walt, and he went ahead with it.

Now, I'm sure—I was never privy to it, I never talked to Walt about it but I know then if you went and did something out of the ordinary, you just didn't go do it on your own; it had to go through approved steps. And I'm sure probably Walt cleared it up with Wangensteen and the staff. I know before they did the cross-circulations it was cleared through a committee of professors and so on. I'm sure Walt probably cleared it properly also before we went ahead with the cross-circulation. The main point being is, you didn't really need the FDA [Food and Drug Administration] to tell you how to do it honestly and right. Of course, it was a lot less expensive and very expeditious.

But I had a system calibrated that I could do, on a predictable basis, a perfusion of 600 cc per minute. And Walt said, "Well, I have a child now and we're going to need a perfusion of about 1000 cc per minute or 1200 cc per minute. Can you do it?" I said, "I don't know. I haven't tried that yet. Give me a few days."

So I went back to the lab and just sized up the whole system that I thought would accommodate it and ran some dogs at that higher perfusion rate, and that worked fine, too. So that's the way it went right on up to adult size. Every time we had an increased size demand, then I would try to titrate the system up to meet that.

It gave you a feeling of hands on. You knew what was happening. You weren't going off into tangents that you couldn't predict.

DR: So there really weren't too many problems with the bubble oxygenator?

RD: No, not really. There was some question later on, if you were careless in putting the silicone in or putting it on too thick that you might have small bubbles of the silicone go off into the patient as emboli, and, of course, that was bad news. But that was avoidable. So, yes, there was that type of complication, but other than that—which is bad enough in itself—but it was a learning lesson.

DR: Now, of course, there were some other extracorporeal circulation devices going around at that time. I wonder if you could just talk a little bit about those, the one in Philadelphia and then the one at—

RD: John Gibbon initially, in the late thirties, started experimentation—maybe in the early thirties he started. I don't know the dates right at hand. Somewhere in that range. 1935. He began some experiments in that basis, of an oxygenator. He had, I think it was—it was not a vertical screen. I think it was a cylindrical screen. It's been so long since I've looked at the details.

DR: It was a vertical cylinder, I think.

RD: Yes. It was a cylinder screen and he was filming the blood down the screen, and he did have some animal experiments and had some success with that. And to carry that on farther, after World War II he started working at that again, and he developed a functioning system. He did a patient, I believe, an atrial septal defect in 1952, I believe it was, and that patient lived and it was a success and did fine. Well, he did a number of patients after that and they all died, so he quit. He just never did another perfusion.

Another aspect of it, Viking Björk in Oslo, at the Karolinska Institute, devised a rotating disk oxygenator, again in the late thirties. These were a series of metal disks on a central axis. It would rotate in a pool of blood. This would film onto the disks in a respiratory atmosphere. He had some success with animals at that time, and then the war came along and things were put on hold.

To carry that disk theme on, after World War II, probably in—it might have been—I don't know the date, maybe 1955, 1956, Fred Cross in Cleveland resurrected that concept and began popularizing a disk oxygenator. Pemco was the manufacturing group in Cleveland that made the disk oxygenator in 1956, 1957. I don't remember the full date. That was a useful tool, but, again, that was not a disposable unit. You had fifty of these eight-inch disks, something of that sort, that you had to wash and clean and sterilize between every case, and on the axis there are so many nooks and crannies for blood to get hung up there. So it did have some problems in maintenance and servicing. And that worked. It functioned. A lot of people were operated on with that system.

Actually, to go back farther, and I think it was in the twenties again—I didn't come prepared with the dates in mind. Let's see. Lindbergh.

DR: Charles Lindbergh?

RD: Yes.

DR: And Alexis Carrel.

RD: And Alexis Carrel. I think Lindbergh had a sister or somebody or other with a heart problem, and he put his mind to work on how to devise a pumping system for them. So he and Carroll devised a so-called heart pump at that time, but I don't think it ever got into any oxygenator type of activity. But he had a reasonably successful pump.

I didn't complete my thoughts on the screen oxygenator. After World War II, I think Gibbon went back, redid his patient. He had switched his oxygenator system to a vertical screen. It was like a screen on a window, put a half a dozen screens together in close relationship and drain blood over the top of it. It would film down these screens. He had IBM [International Business Machines] subsidize it. He had some connection with IBM, I'm not sure what it was. So they subsidized the development of this machine, I understand, to the tune of about $100,000, this beautiful stainless steel unit. Again, that was in the late forties, early fifties. Then the Mayo Clinic took on the Gibbon concept, again in the early fifties, I guess, after Gibbon had his first success, and they did a lot of fine research down at the Mayo Clinic with John Gibbon and his group, physiology, there. They had some good success with their vertical screen oxygenator.

Again, that took a lot of blood to prime the thing, and you're getting a lot of mismatched—the problem with matching up of a lot of blood, and you had cleaning problems. All of these screens, how are you going to clean all the protein and such off all these screens between cases of patients of different blood types and so on? So while that was successful in their hands, it didn't achieve a lot of popularity, because that really was very expensive and cumbersome.

People would come to the University. They would go down and look at the Mayo system functioning, and [Dr.] John Kirklin doing a very credible job of taking care of patients with this very large machine and a crew of physiologists and engineers and everything else running it, and very impressive. And they'd come up to Minnesota to look at our little Rube Goldberg activity that cost fifteen bucks to put together. One fellow would do everything, myself. So they were starting to match costs. Now, most of these people around the world couldn't see $100,000, but they could see maybe $50. So we had a lot of followers in that regard.

One thing that I found was always very rewarding to me was, I think at an American Surgical meeting, and again I don't know the time of that, was about 1958, maybe, there was a fellow from UCLA. I've forgotten his name. I don't remember who that was. He gave a paper on his experience with my system in dogs, and it was a very negative paper. He couldn't keep the dogs alive. Everything went to pot. It was terrible. And there was a little discussion about how my system was a fraud or only I could make it work or whatever the case it was.

Denton Cooley got up in response to the paper. Denton had gone through the lab and looked at our system, and I'd gone over it with him and he worked it out in his own lab. And, of course, he's a superb technician, had a good crew together.

DR: And he was located where?

RD: Houston, Texas. Texas Heart Institute. Now, he might have been at Baylor then with [Michael] Debakey, but that's before he split off. But he said, well, he had just done 250 patients with my system and it worked fine. So that was the end of the discussion. [Laughter] So I had a little justification then that my work wasn't totally in vain.

Then, of course, subsequently, to put together a system out of laboratory odds and ends and hand-make it every time, it was cumbersome, obviously, and so forth, the way that it had to go. Vince Gott was working with Baxter Laboratories and Ted Geweiky, I think his name was, Dr. Geweiky was the fellow from Baxter and they put together the so-called sheet oxygenator, a plastic manufactured version of what I had, and that was quite successful for a number of years. And then Inge Rygg in Copenhagen put together a similar type of package in Europe, very simply done with some plastic sheeting, kind of copying what we had done, and that worked out pretty well.

So, as you would hope, you make a few difficult early steps and you get over the first few hurdles and hopefully you help people understand what they're doing more and more, and you'll get some good engineers that do it and other factors involved that will explore on in other areas.

DR: It must have been very exciting for you to have contributed this innovation which really opened up heart surgery to a much wider audience, didn't it?

RD: Oh, absolutely. Oh, yes. I met people from all over the world. And then, of course, the obvious thing would come to mind, if you have a filming oxygenator and a bubble oxygenator, well, what is the human lung? Well, the human lung is neither a bubbler nor a filter; it's a system of capillaries. So can you devise a capillary system that would work in that fashion? So that was my next step. I devised some prototypes, a capillary system using silicone rubber tubing. I think I was able to get some extruded silicone tubing. Silicone rubber had a very good diffusion rate for oxygen. Polyvinyl and some of the

other plastics had a real barrier to oxygen. But silicone rubber would diffuse oxygen.

So I had these capillary tubes, and I built some prototypes of that and had a patent on it, actually, of the capillary oxygenator. This was subsequently taken over by Dow Corning. Of course, the kidney is a variation of the lung for a capillary bed that's being exposed to systems. So Dow Corning took the patent rights from the University of Minnesota. I mean, that's my patent, but the University held the rights. They made the first capillary kidney, and that was very successful for a long time until the patents ran out and everybody got into the act and was subsequently put into its original concept as an oxygenator system. So there are a half a dozen people making capillary oxygenators out of it. But I never got around to write anything about that other than my patent, and I never really talked to anyone. In fact, I don't think even Walt knew about it. I was on my residency and doing other things, and I was distracted. So I didn't really have a chance to follow that through very well.

DR: Did you patent the bubble oxygenator?

RD: Yes.

DR: Was that you alone?

RD: I think Walt might have been on the patent just because he was chief and was at the University at the time, and that's how the patent office worked at the University.

DR: So was the bubble oxygenator commercialized?

RD: Yes. Baxter made their variation of it.

DR: Their variation that Vince Gott had been involved in?

RD: Yes.

DR: Did you work with Vince Gott on that subsequent version?

RD: Not a whole lot. No, not a whole lot. I was on to other areas in my residency by then. There was a little difficulty in getting me a residency position there, but Walt went to bat for me and so did Dr. Wangensteen, and they overcame a reluctant dean, I guess, and so I was able to finish a residency.

DR: Did you mention that you had run into Earl Bakken somewhere in the fifties at the University?

RD: When I was working in the lab, we always had equipment problems, transducers, all

kinds of different things that you'd need electrical support for, and I didn't know anything about the electronics of it. Earl would come by periodically just to talk and to see if there was any area that he could help us in, electronics, and we'd have little tasks for him to help us out with. I had a couple of little projects that he helped me with.

Actually, when I was making my first prototypes of the capillary oxygenator, I was making it vinyl at that time just to get the prototype and the pattern hooked up, and the way I would do that, I had a couple of steel plates with a lot of steel pins, wires, actually, between the two plates, I dipped the whole thing in a solution of a vinyl mixture and then put an electric current on each of the wires. This would give you the deposit onto the wires of the vinyl to create your capillary. Then when you take it apart, you'd pull wires out, take the plates off, and so you'd have your prototype of your capillary oxygenator.

Earl helped me put together the electronics to get these poles wired so that I could heat up all of these wires in that system. That was one thing that immediately comes to mind. So I was a speaking acquaintance with Earl. And, of course, he was working with Walt at that time, ultimately on into the pacemaker problem.

DR: You got into some fairly sophisticated engineering, but you weren't trained in that area. How did you manage to do that?

RD: Oh, I wouldn't call it sophisticated engineering. I'd say just tinkering.

DR: Well, you were good at it, at any rate.

RD: It's a matter of knowing where you're going and being observant to do something. No matter what you do, but do something and learn from it. Then you can do something else. As they say, the credo of a surgeon, I always remember from my good friend [Dr. Richard] Rich Lillehei, "I may not be right, but I'm sure." Because a surgeon can't stand around and wait until he's right all the time, because you're in many situations where you don't know what right is, but you have to do something. So at least if you do something, you have a starting point. You know, well, it's either going to work or it isn't, and if it doesn't work, you can move on from that point.

DR: Right. I like that quote. So the modified version of the oxygenator that you developed was commercialized by Baxter?

RD: Yes.

DR: Was that a pretty successful device?

RD: Yes. The University got a great deal of royalties from it. I don't know. Walt never told me what it was. He kept it all. I have no idea what it was, but I'm sure it was

appreciable, and I have other indirect evidence that that's the case.

DR: And that kind of oxygenator was really the standard for a long time, wasn't it?

RD: Pretty much so. Well, at that time, you had quite a few followers of the disk oxygenator, because people were still worried about bubbles. They never learned how to live with them. The screen oxygenator, the Gibbon, kind of fell by the wayside. I doubt if there were more than half a dozen ever put into any kind of use, and that didn't last long because it was just too difficult and expensive. The disk had its own difficulties, but it wasn't quite as expensive to deal with and operate.

DR: The Baxter version was used up until when? What was that supplanted by, if anything?

RD: Well, actually, after I went to Chicago from Minnesota, I had some other thoughts. In order to have a very flexible bag that you had to take out of a sack and shake out and put on a frame and hang up, I thought it would be nicer if you had a so-called rigid shell oxygenator, something you just pull out a package, hang it on a rack and plug it in.

So I had some design concepts for that, and I worked with Jim Bentley. Jim Bentley was an engineer that was working with Lowell Edwards on the [Alberta] Starr-Edwards valve out in Newport Beach, California. I didn't know Jim, but I went out and talked to Lowell Edwards to see if he would be interested in making this rigid shell oxygenator, a concept that I had, and he wasn't interested in it because he was all involved in valves. But Jim Bentley, one of his engineers, decided, well, he'd like to take that on, out on his own and start a company and make that. So I worked with Jim for a while until we got that in good working shape, and Jim developed a very successful company, Bentley Laboratories. Subsequently sold out to one of the other big houses. So I had the so-called hard shell called Temptrol, that was the name that was stuck on it.

DR: Temptrol.

RD: Because it had a chamber in it that would control the temperature. The sheet oxygenator had no temperature control intrinsic to it. You had to have an external system to put the blood through in order to maintain control, and some people did and some didn't. The heat loss in a patient was a real problem in these things, because you're blowing actually cold gas through the system. You can take a lot of heat out of the blood. So this is something that I devised with Jim, we had a cylinder in it that would serve as the heat exchanger and could maintain blood temperature. So that's where the "temp" comes in, Temptrol.

DR: That's while you were in Chicago?

RD: Yes. And that was quite successful for quite a while until the capillary replacements came along. When was that? I came to Dayton [Ohio] in 1966. Maybe about 1974 when the capillary oxygenators came into being, roughly, give or take a few years.

DR: So, the sheet oxygenator was pretty popular for several decades, then.

RD: Yes. Ten years or so until the Temptrol, I think, took over from it. The sheet oxygenator was from about 1956, I would say, to 1962 or 1964, maybe 1964, 1965, thereabout. Yes, it's been close to ten years, maybe. Then the Temptrol took over for another eight, ten years until the capillaries came in. Now that's pretty routine. I can't imagine there are any oxygenator systems in operation today that aren't of the capillary type, unless it's in Third World countries or something.

DR: As I recall you saying earlier, you were working on other things and didn't really keep involved in developing new oxygenators or other devices.

RD: Well, not really, because I had to complete my residency in thoracic surgery. That was pretty time-consuming and heavy duty in itself.

DR: Sure. But you did work on the Temptrol when you got to Chicago?

RD: Well, that was after my residency when I was in Chicago. Yes.

DR: But then after Chicago, you didn't really continue with that line of work?

RD: No.

DR: Can you talk a little bit about the other people who were at the U? You've already talked about Dr. Lillehei. But maybe say a little bit more about Owen Wangensteen or Richard Varco or Vincent Gott, the other people that you were working with or studying with at that time, and what the atmosphere was like at that time.

RD: It was a very exciting atmosphere. There were always foreign visitors coming in and trying to see what you're doing, and you'd spend as much time as you could with them and show them everything you had. I think Dr. Wangensteen almost went out of his way to show his appreciation for what we were able to bring to the department. He was very supportive and kind. I remember being on his service, both as a junior and a senior resident, and he was very delightful to work with. He was a real dream of a fellow. Great temperament. I never saw him lose his temper or get too disturbed or upset about anything. He just had certain expectations and you knew what they were and you carried through for him.

Dick Varco was around. He was just around. I was on his service for a while. He was

certainly a master surgeon, a great operating-room mechanic, so to speak. You'd have to talk to Walt about any contributions he might have made to the open-heart surgery program, because I wasn't privy to them.

And, of course, Walt was tremendous to work with and, of course, my senior mentor. Very helpful and supportive all the way through. Vince Gott was my contemporary. In fact, actually he was a little behind me, I think, in the residency program or at least he was behind me in the laboratory, his laboratory experience. See, I started out my work over there in the laboratory, and Vince started out his work there as an intern, I think it was, or a junior resident. So I was in the laboratory before he, and Vince followed on. Vince came through. It's my feeling that he had done a moderate amount of work with the pacemaker routine. I don't know how all that shook out.

Vince has always been a good friend. We were kind of working, of course, when he had the sheet oxygenator. I reviewed it with him and looked it over with him.

DR: How did he get involved with Baxter? You said he was working with Baxter. Do you happen to recall?

RD: I don't know how that came about. I don't know if Baxter initiated the contact or what. Maybe Vince would remember. I don't know. Vince or Walt.

DR: I'll find out. I'll ask him.

RD: Vince might know. I don't know.

DR: Well, there were a lot of innovations coming out of those labs.

RD: Yes. It was just a lot of people over there. Of course, I never worked with Norm Shumway, but we went kind of through the program together. He was a little head of me in the program. I always enjoyed his friendship, relationship. He was a very humorous, wonderful fellow, always had great commentary. Great humorist. If you want a Shumway anecdote….

DR: Sure.

RD: One that I recall, Don Ferguson was a senior resident on Owen Wangensteen's surgical service, and Norm Shumway was the intern. So one day Dr. Wangensteen was doing a herniorrhaphy.

DR: Pardon?

RD: A hernia operation.

DR: Oh, yes.

RD: Dr. Wangensteen had never had any surgical training as such. There were no surgical training programs. Everybody was sort of self-taught or had an apprenticeship or so on. But he was always very proud of his work, which justly he should be, but it wasn't quite the same standard that those of us that were more fortunate to have training in it. But Don Ferguson was a very meticulous, fine surgeon, very precise.

So when Dr. Wangensteen finished his herniorrhaphy, he looked up over his glasses, as he was prone to do, and said, "Well, Dr. Ferguson, what do you think of this operation?" And Don, being very candid, said—I don't know how he phrased it, but ultimately it meant that it didn't look too great to him or words to that effect, which kind of shook the chief up a little bit, because he thought it was pretty good. And he said, "Well, Dr. Shumway, what do you think of this?" But Shumway is always very quick with a response. And, of course, he had to keep the chief happy because he was chief. And Don Ferguson, he couldn't annoy because he was working for Don. So he said, "Well, I'll stand with the majority." And I thought that was a marvelous way of walking the tightrope. [Laughter]

DR: Sidestepping the answer.

RD: Yes.

DR: That's great.

RD: He and John Lewis were always coming up with comments of that sort on rounds or grand rounds or wherever. It was a very enjoyable, pleasant experience. I don't think I've ever in my life enjoyed going to work as much as I did those years.

DR: Was that in Operating Room C? Do you remember that?

RD: J was the heart room.

DR: I think that room is a children's pediatric clinic now. I went down there to look at it. Could you kind of briefly summarize the rest of your career, which was chronologically much longer but, of course, we wanted to focus today on the Minnesota portion.

RD: I went down to Chicago as chairman of surgery at the Chicago Medical School. It was a school that had a difficult history to it. It was not part of any university system and was, I think, only one or two, two or three schools in the country that was not associated with a university system. At one time, there were a number of medical schools around that weren't really of very good quality, and they were the so-called Class A and Class B

schools. Chicago Medical School was a Class B school up to that time until some regulations came along. I don't know, I think the educational systems or somehow or other deemed that every medical school had to go through a certain amount of standards in order to stay in existence. So, Chicago Medical School was only one of three, I think, of these quite a dozen schools, that was able to make the transition from a B to an A school. So it did not have a stellar history, but it made the grade.

When I went there, again this was ten years later or more, fifteen, I don't know, and making a lot of promises that they were going to really try to become a first-rate school. So I spent four years there as chairman of surgery, and they never really quite made the grade. It was not a satisfying existence for me. They were happy to have you stand around and do your administrating if you didn't annoy them too much, the private practitioners, if you didn't get into their way. I was trying to build a heart program, and I couldn't really get any support for my heart program there. They'd give you a little word of mouth support but no real support.

I was consulting with NIH [National Institutes of Health] on the artificial heart program. I was at one of the meetings and I met the organizer from the NIH that was directing this review session, just let it be known that I was not happy where I was and I'd be happy to move on to anything.

There was a fellow there, Paul Kezdi, who was a cardiologist, who was also consulting on the program, and he brought the word back. He was a cardiologist at Northwestern, before he went to a new hospital organization at Kettering, Ohio, and they developed the Cox Heart Institute, or were developing it at that time. So he let it be known down there that I would be happy to move. They contacted me, and I looked at it for a while.

I looked at Dayton, Ohio. There was no real credible heart surgery program within a 150-mile radius. Cleveland Clinic had a decent one. But Columbus, Cincinnati, Indianapolis, Louisville, Lexington, I didn't think any of them had a credible program. In other words, I thought it would be a very good place to get started.

They also had the new Cox Heart Institute. I could have a research lab and work there. Now, Kettering, Ohio, this is on the estate of Charles F. Kettering of the Sloan-Kettering organization of Boss Kett. And Jimmy Cox, this is the Cox newspaper group. They're out of Atlanta. Jimmy Cox's two sisters are two of the wealthiest women in the country right at the present time, one living in Atlanta and one living in Hawaii. But this was the Cox family that helped fund the Cox Heart Institute. So that gave me a chance to start building a heart program there, which I did.

Actually, I'm quite proud of one achievement there. As soon as I got there, Dayton, again, was removed from Cleveland and from Columbus and Cincinnati, and it was pretty well ignored. I was looking at the membership of the county medical society. Less than

half of the society had been trained in medicine in Ohio. They were either foreigners or coming from elsewhere. I thought they had some excellent hospitals in Dayton and they had some good residency training programs. They also had a new university there, Wright State University, which was just started in the late fifties, I think, a few years before I got there. Of course, a medical school was the farthest thing from their mind at the time.

But I got there and I looked it over, and I spent about three years organizing a proposal for a medical school there. I was contacting everybody I could think of, to the neighboring medical societies, from the various legislative members. Of course, it took me a little while to get a contact at the university, because, after all, you have to have the university's support. So I figured a medical school on its own is not going to go well. They had had a new medical school at Toledo that the legislature just dumped into Toledo without any undergraduate context, and it's never really gotten off the ground. It's always been kind of a second-rate school, as far as I can tell. So I knew to have a credible school you really had to have an academic background associated with a major university.

Well, from various contacts, I did have a chance to meet with the president of Wright State University, and I apparently sold him on it enough and he turned me over to his dean of engineering. So I worked with the deans of engineering to work my proposal into a format, of the university's format, and, with our local legislators, lobbied it through the legislature and we began a medical school there. So it's about twenty-five years old now or better, and I've always been very proud of that. Ever since it started, from the first class on, its students have been at least in the upper 50 percentile of performance for the whole country, and I thought that was pretty good. They do a very credible job. Well, anyway that was part of it.

I built a heart surgery program there. Of course, Boss Kett, Charles F. Kettering, died before I got there, but his son, Eugene Kettering, and his wife, Virginia, had moved back to Dayton. And, of course, I was working at the Kettering Hospital, which was built on the estate, Boss Kett's estate, just over the hill there, and he had helped build the hospital originally himself. So, of course, the Ketterings were interested in having a program going, and they gave us very good support. Virginia is still alive now, at the age of ninety-one. Interestingly enough, she helped get the heart program going in Dayton, and it's a very credible program.

One time, when I was busiest, between my partner and I, we were doing about—I think we did about 900 cases just between the two of us a year. That was a lot of work. But anyway, the point being that she had helped get the whole program started. Gene [Kettering] died a number of years ago. She ended up getting her own aortic valve replaced here a year ago at the age of ninety and she got through it. She was concerned about going ahead with it, but her choices were a little limited. It was pretty soon death or

to do something. But she liked to live, and she's been a tremendous philanthropist around here.

So we built a good heart program there until 1988, January of 1988. I was approaching my sixtieth birthday, and for a variety of reasons that came together at that time—six months before I retired I wasn't even thinking about it. When it came closer and closer to my sixtieth birthday, I decided what's the point. Let the younger people get in on the act here. I was tired, burned out. So I quit. It was a good time for me to get out. And been very happy with it every since, my decision at that time. I'm glad I don't have to deal with it now. I don't know anybody over there in the medical profession that's happy practicing today. It's a very unhappy medical scene. You no longer practice medicine; you're an administrator of health care with the rules given to you by the HMOs, by hospital administrators, and by the government and by everybody else under the sun except your own self and your own conscience. That's maybe an overstatement of it, but it's—

DR: A different world than it was in the fifties.

RD: Yes. It would be very hard for me to do today.

DR: You are associated with some medical device companies locally, is that correct?

RD: Yes.

DR: You sort of keep your hand in.

RD: Years ago, while I was still in the laboratory, of course, I was interested in replacing defective heart valves, again, back to my original inspiration here. He had an engineer in the lab at that time by the name of Bob Kaster. I knew Bob. He was working just down the hall from me. He devised a heart valve, which ultimately was called the Lillehei-Kaster valve. In fact, I was somewhat associated with Bob through his wife, because his wife used to work for me, she was a technician running my lab when I was in the research lab. So Bob married Sharon.

But then Washington Scientific was making the Lillehei-Kaster valve for Walt at that time and they decided they wanted out of the heart business. So another fellow came on the scene and formed Medical Incorporated and started to make the Lillehei-Kaster valve. It was a very credible valve. Actually, just a few months ago I saw a patient, I had put a Lillehei-Kaster valve in. It must have been 1967. A mitral Lillehei-Kaster. He's coming along very fine. Doing fine all this many years later.

So that company was in existence out there, and they did a pretty decent job of making the Lillehei-Kaster valve, except that the president of the company and the chief manager

of it was, to put it politely, a scoundrel and a bit of a con artist, and drove the company into bankruptcy after spending an incredible amount of money.

DR: Was that located in the state of Washington?

RD: No. Washington Scientific was a company that was the producer of the valve in Minneapolis. They're located out in Inver Grove Heights, about a half hour south of the airport. So that first company bellied up, went bankrupt, and then it was taken over by a fellow by the name of Adel Mikail, a Ph.D., pharmaceutical chemist or something like that. But he was the medical director for the old medical company and for Lillehei-Kaster.

DR: Medical Incorporated?

RD: Yes. And their subsequent developments, the Omnicarbon and the Omniscience valves. So he resurrected the company and they're going along with it. As I have a vested interested in the valve, basically, and the fact that I was one of the original investigators and I have a lot of patients with those valves in, just to justify my own work of putting these valves in people, I've still been associated with the company, coming along. It's resurrected and making some good progress.

DR: So you're on the board of the company or you're a scientific advisor?

RD: Yes. On the board now.

DR: That's interesting, because you got interested in doing research. You started with the concept of a prosthetic heart valve.

RD: Yes. Still dealing with valves. And so is Walt Lillehei as medical director of St. Jude.

DR: Right.

RD: Interestingly enough, Demetre [Nick] Nicoloff was one of Walt's students at the time. He was a little bit behind me in the program at the University. But he left the University after a number of years and was organizer of probably the largest heart group in town here. I think there are eleven surgeons in the group. But he was instrumental in developing the St. Jude valve, and then he and Manny Villafaña got out of that, and subsequently Manny and Nick and some others began the ATS [Advancing The Standard] company and so they're in another valve business. So Walt Lillehei has a chain of events from himself or his students of one sort of another that devised a number of different types of heart valves through the years. I'm kind of peripheral on that scheme of things.

DR: You're right. Quite a few of the medical device companies in Minnesota have some kind of link with Walt Lillehei or Richard Lillehei. So, yes, an interesting group there in the fifties.

RD: So that's why I come back to Minneapolis often. It's still always fun to come back here, to walk around the lakes and just to enjoy the city.

DR: Thanks so much for your time. I appreciate it.

RD: You're most welcome. Thank you.