

INTERVIEWEE: Dr. Walter Wolfe  
INTERVIEWER: Dr. James Gifford  
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WOLFE INTERVIEW NO. 1

DR. JAMES GIFFORD: ...Durham, North Carolina, July 14, 1994. This interview presents Dr. Walter Wolfe, director of the Pulmonary Physiology Laboratory, Department of Surgery, Duke University Medical Center. The subject of this interview is Dr. Wolfe's career and the work of that laboratory, the structure of the Department of Surgery now, and some comparative material with the evolutionary developments of the Surgery Department earlier. This interview takes place in Dr. Wolfe's office on the fourth floor of Duke Hospital South. The interviewer is Dr. James Gifford. [Pause] Dr. Wolfe, let's try again to have you say how you came to get to Duke. Duke.

DR. WALTER WOLFE: Alright. Well, as I said, I went to Denison University and then on over to Temple Medical School. And while I was there, I met some people, and people were talking about residencies. And one of the men said he wanted to go into brain surgery and was thinking of coming to Duke to interview. We were going on a fishing trip to Florida, and I said, "Well, on the way down, maybe I'll stop and interview, too." And I did and was impressed with the place. And I remember interviewing with Dr. Anlyan and Dr. Nashold at the time. And then after I finished that senior year in medical school, I already had secured an internship at Philadelphia General. And I noted one of the 90 interns was from Duke. His name was Jerry Jones. I looked him up, and he had gone to State and then to Duke Medical School. And I told him I was impressed with Duke. And he said, "I wouldn't want to go down there. They make you repeat the internship." And obviously he'd had an unfavorable experience.

That made me get on the *Silver Meteor* from Philadelphia one Friday night, and come here to Raleigh, and then over to Duke Saturday morning to see Dr. Gardner, who was chairman at the time. And had an interview which I have to say I left feeling somewhat flat and negative. A few days later back in Philadelphia, I got actually a telegram saying that I could have a junior residency at Duke. And so immediately accepted. Finished the internship at PGH and then came here in July of 1964.

GIFFORD: Which was just a matter of months before the administration changed.

WOLFE: Right. That's right. Dr. Saveston officially, I guess, took the reins over September 1<sup>st</sup>.

GIFFORD: Let me check this one more time. Dr. Wolfe, I think maybe we'll do things a little bit out of order. I think I'd really like you to begin by telling me, if you would, what was the Duke department like at the time you first came? What were its strengths and weaknesses? How was it structured? Anything that strikes you as being an outstanding characteristic of what was going on?

WOLFE: Well, I think there are a couple of things: I think I ought to, in the main historically, in here just say that I had been here twice, interviewed and been around and had met some residents and had not recognized the wards were still segregated. And I have to admit that surprised me immensely, that I had arrived here and found that. (The wards were integrated later in 1964.) The residency was very well structured. It was very well organized with a man entering every four months. It was a very well-structured thing. The residency was small, and the faculty was small. And I would say those were two of the great strengths that were here, if you want to look at it that way, for Dr. Saveson because there was room for growth. And I think that was impressive.

The second thing that was I think strong about it was there was already a feeling that the resident would enter the laboratory for a period of time, though it might have been a very short

period of time. And you asked me what had gotten me interested in surgery. I had had surgery rotations as a student, and I had worked with Dr. Davila. And he had told me that the thing to do if you're going to go into heart surgery—you've got to remember this is heart surgery in 1961, which is really in its infancy—he said, "You really need to get into the laboratory and get research experience in that." I had always remembered that. And I have to admit that when I was on the campus here at Duke in July and August and September '64, I had no idea what kind of a research commitment would become involved, you know. I mean most people at the time I arrived were spending a year or maybe a few months. But I think those are the main strengths right there: was the critical services were very strong, the department was small, there was a lot of room for growth, and the residency really was well organized and structured. And I think those things really were a plus for Dr. Saveston when he came on board.

GIFFORD: What do you mean by saying the residency was well structured?

WOLFE: Well, it was small. And the way people entered was such that he could change things without totally disrupting. In other words, he was going to make some changes. Within the first year he was here, he said, "Well, I'm not going to appoint anybody to the senior residency this year. Everybody's going to go in the lab." And that caused some defectors, you know. Some people left, and it was felt they would go elsewhere.

GIFFORD: He was Dr. Gardner?

WOLFE: No, Dr. Saveston said that.

GIFFORD: Oh, I'm sorry. I wanted to go back to before Dr. Saveston.

WOLFE: Okay.

GIFFORD: We'll get to that other part. Your impression of the residency that existed before Dr. Saveston?

WOLFE: Well, as I say, it was very well structured, and the teaching, the clinical teaching, was very well structured. Dr. Gardner had his conference every week. He came to clinic every day. It was a clinical program, structured in that way, to teach excellent clinical surgery.

GIFFORD: Okay. Now was there any laboratory component in it?

WOLFE: Well, that's what I'm saying: The laboratory component was small, but it was there. There was an understanding of it. I think Dr. Anlyan already had a laboratory here. Dr. Sealy and Dr. Young had a laboratory here. Of course they'd done a lot of investigation already. You know Dr. Brown was getting more and more active in the hypervaric areas and things of this nature. So there was....

GIFFORD: Dr. Grimson.

WOLFE: Grimson, of course, was GI laboratory.

GIFFORD: And Bill Beard

WOLFE: Right here. Well, I didn't know much about Dr. Beard, of course. He was across in the Bell Building. That was an area we didn't—I wasn't aware of. Then there was—like I say, it was small, it was structured. But the attitudes were here, let's put it that way. I guess I'll put it that way.

GIFFORD: Okay. Now let me just push this structured word one more time. A person entering that residency would do what?

WOLFE: Well, he would walk through in a series of rotations, with some laboratory experience, probably depending on him. Now, I wasn't here to see a lot of this, so I don't know. But some people spent zero time in the lab. Some people spent four months or eight months. But they entered, and they walked through in a very structured rotation and ended up as chief resident.

And one man graduated every four months. That was *the* chief. And then another one would ascend.

GIFFORD: Okay. So there was a single residency program for the whole department?

WOLFE: Right. There was a single for general and thoracic surgery.

GIFFORD: Okay.

WOLFE: A single residency. It was not divided as it is now.

GIFFORD: Were all of the other residency programs at that time organized the same way, every four months?

WOLFE: No. I think brain surgery, the subspecialties, were different.

GIFFORD: Okay.

WOLFE: And they were under the direction of the division chiefs.

GIFFORD: But you felt when you got here that the faculty that was here before Saveston was encouraging to laboratory research, even if there wasn't yet a great deal of it.

WOLFE: Absolutely. No question about it.

GIFFORD: Okay. That's something that I hadn't been able to elicit from most other folks.

Okay. Now....

WOLFE: If you think back on what Ivan Brown was doing the hypervaric chamber and that sort of thing. Dr. Sealy and Dr. Young with hypothermia and stuff in that area. Dr. Grimson and \_\_\_\_\_ and gastroenterostomy for ulcer disease. Like I said, I didn't know much about Dr. Beard's work.

GIFFORD: What was Dr. Anlyan doing?

WOLFE: Dr. Anlyan had already started on his business with lymphoedema, Venus disease, clotting problems, thrombosis. All that stuff was here sort of probably needing to get the fire

under it a little more; let's put it that way. It was on the stove. It wasn't cooking as much as it could cook yet.

GIFFORD: Okay. So a resident would spend from zero to a year.

WOLFE: Maybe. I can't speak for a lot. I'd have to go back and think about those residents, but they didn't spend a lot of time. Certainly not the intense kind of time that we started in '65.

GIFFORD: Alright. Let's talk about the residency as you experienced it. Can you talk about how it was structured, what its stages were, whatever other categories you see fit to use?

WOLFE: Dr. Saveston left the residency pretty much intact except he made adjustments with the year, so to speak, to expand it, expand the rotations. After the junior year he had people go into the laboratory for one or two years. At that time I went in for one year. And during that year he asked if I would like to go to California for a year, which I obviously agreed—wanted to do very much. So then I came back into the residency for a year as a senior resident. Then left again and went to California for a year. Came back here again, and then got back into the residency. So he built flexibility into what I began to tell you was a very structured organization, so that people had ability to do different things or pursue areas that they wanted to pursue.

And about that time he then changed the end of the residency as far as how people finished. In other words, there was a group of people in the pipeline who were in the old system. And then there were people like myself who were coming into a new system that was—which I guess he was actually developing probably, is the truth, or knowing in his mind he was changing it and developing it as he went along. And about the time I was in California, I guess, in '67 or so, he got the Scholar Program in in research and, the Academic Scholar Program for Residents. And then we had the year which we—you've got also in the time context that heart surgery is starting to grow now. At the end of the sixties it was starting to pick up. Research was picking

up. The NIH was fueling, you know, public money out a little more now. And the whole place was starting to grow. And we put in what we called the “Super Chief” then. And that was a year, the last year, which was a year of only heart surgery.

So the residency changed within those—from ’64 to ’68—in that way. Very subtly but very obviously. Until now instead of having a man being coming out every four months, we had a final year where a man spent one whole year doing nothing but heart surgery. And that was called the Teaching Scholar. But it was named by the residents the “Super Chief.” And that started with Brad Hattler being the first Super Chief. And then Newland Oldham and then Bill Gay and Bob Klein and then myself. And then Bob Anderson. So the end of the sixties and early seventies, the big developments were the two years in the laboratory and the Teaching Scholar Program and the Super Chief part of the residency. That was the big growth of the sixties, I think, to get that organized. Now heart surgery started to.... You have to remember that the faculty had not grown a lot then. Paul Ebert had come and now left to go to be chairman at Cornell.

So now the next thing I see Dr. Sabiston doing, the way I put it in my mind, is once we decided to have a—we started building the faculty. And Sam Wells came on the faculty. Dr. Oldham came on. And I came on, Bob Anderson came on. So there were additions to the faculty to strengthen. And like I said earlier on, the fact that it was small initially I think that was a plus. Because once Sabiston had things in order, he had room to grow and room to expand the faculty.

GIFFORD: What paid for it?

WOLFE: That was the seventies.

GIFFORD: Okay. What paid for the growth?

WOLFE: Two things, I think: One was the research funding and the ability to get research funding to help support people, all these clinical investigatorships and stuff that people got which supported their salary. And the other thing was the growth of heart surgery. In the middle seventies then, coronary artery surgery really took off. Plus results of all surgery improved immensely. The days of the sixties when the mortality rates were high for congenital and adult heart surgery were over. And in the seventies the mortality rates started to fall. And a lot of these things happened. So there became income or there became revenues from the growth of heart surgery. We went from one heart three or four times a week to two hearts five times a week to—in this day and age we're talking about six, seven hearts a day.

GIFFORD: Why were the death rates so high in the sixties?

WOLFE: Well, there was a tremendous learning curve. There was no training in heart surgery. I mean you learned how to do heart surgery by doing it. The heart-lung machine was just developed in '54 and used—and it still had a lot of quirks in it. Blood matching. Better oxygenators. I mean just so many things happened in the seventies which improved the situation: Anesthesia, ventilators, knowledge. Plus, by the seventies, people had been trained in heart surgery. You know when you think of Dr. Sabiston being trained in heart surgery, he sort of just was there and watched it and did it. By the time I was finished, I had helped people do heart surgery. Now there was nobody to help in those days back when they started doing it. They just started doing it. So there was—the training of surgeons became, you know, much more sophisticated as far as heart disease goes.

GIFFORD: Now we've mentioned the fact that in the Department of Surgery before Dr. Sabiston came, there were very few laboratories. But that there was a seed there that could be

fertilized. Now, every division has laboratories. There are a couple of dozen independent laboratories. What led to that growth? Just the increase in the database? Or how did that work?

WOLFE: Well, I think I should go back and say some of the subspecialties had laboratories, too. I mean, you know, urology and neurosurgery. Dr. Odom ran a laboratory. When I speak about the laboratory growth, the ones we talked about, I'm thinking about general thoracic surgery. So don't misunderstand me. The subspecialties had several laboratories which were active. Certainly neurosurgery was one, you know. And certainly neurology, I would say, but I wasn't familiar with that. No, I think the thing that stimulated, of course, was his—I don't want to say his demanding—but his very intense feeling that this was a very important part of the training program. And not only that, he had a pretty intense interest in training people to be in teaching and academic institutions. And the background for that is his background, which is the Halsted Laboratory at Hopkins. And Dr. Blalock's insistence that people have, you know, go to the laboratory.

So I think he came here and put that down as sort of a road map for progress to make goals in academic surgery, and consequently you went there. So that was the first thing. If he hadn't of said, Hey, you've got to do this, you know, maybe people wouldn't have done it, or done it as much. Most of us kind of wanted to do it because we thought we were going to do this. You have to realize I had no background in research when I arrived here. And only when I went in Dr. Sabiston's lab did I get my first opportunity to do investigations. Now we've got medical students spending almost a year already getting an investigation background. So that's due to Dr. Sabiston's intense feelings that this is very much a part of the educational process.

Gifford: You've mentioned the Halsted Lab and Dr. Blalock. When Dr. Sabiston was training and earlier on, did going to the laboratory mean in those years what it does today? Was the focus

of a laboratory the same kind of really basic investigations of processes? Or was it something slightly more practical?

WOLFE: Well, there are a lot of practical questions in surgery which were worked out or answered in the animal laboratory. No question about that. But there were also some basic questions. I'll just use Dr. Blalock's experiments on shock where he proved that it was volume, you know. Replace the volume rather than these other theories about why somebody went into shock. So there were basic research projects or basic questions answered in the laboratory, but probably not as much as now. I mean they weren't working at the molecular level back there a lot, let's face it. They were trying to figure out how they could do operations to change physiology which would be acceptable to the human organism so it would be successful as far as an operation to correct the problem.

GIFFORD: So they studied physiology from the angle of vision of the blade.

WOLFE: And from the vision of anatomy. In other words, they hooked things up to see it worked as far as being satisfactory to the organism.

GIFFORD: And they studied physiology in order to understand what they needed to understand to do the hookup?

WOLFE: Right.

GIFFORD: Okay. Now, when does that begin to change? When does the change come in surgery when the molecular level and the basic process research really begins to take hold?

WOLFE: Well, if I could give you a date on that, I think, you know, we started even in '65 I started looking at things like surfactant which is a molecule. I don't consider myself a molecular man about anything. But certainly in the seventies a lot of things changed. Like just take the experiment cardioplegia where we started looking at the effects of potassium and this sort of

thing on the cell. And we started looking at things like ATP and their effect on the heart. The lung: We're still dealing with this problem of surfactant, artificial surfactant, the alveolar level. In malignant disease, of course, we've just pushed right on past the anatomical situation; they're now looking at oncogenes and all these things which are clearly molecular biology. So all this has happened. And the explosion in molecular biology I really think, in my mind, is reasonably recent. I mean I'd say certainly the late eighties. But somebody might disagree with me, a better scientist might. But the last five or six years have been an explosion in molecular biology.

GIFFORD: Well, when you were in the lab yourself as a resident, what did you do?

WOLFE: Well, I worked on a project that Dr. Sabiston had begun in Hopkins, which was venous thrombosis and pulmonary embolisms. And the main thrust that first year was in pulmonary embolisms which kind of led on to, you know, obstructive disease of the arteries to the lung, which led to, like I say, problems with surfactant and ventilation. That whole year's background kind of sent me then to California. When I was out there, I worked basically in airways, about the effect of obstruction of the pulmonary artery on the airwave and broncho-constriction and changes in compliance and resistance. That whole experience kind of led me to oxygen toxicity and the effect of oxygen on the airwave and on the alveolars, and an effect on ciliary function. Other things of that nature. And that step sort of pushed me from oxygen toxicity to just lung injury. Oxygen's a way of injuring the lungs also. The next thing you know we're in the lung injury in a very big way.

So it's interesting. Dr. Canroë where—I was with Dr. Canroë and Dr. Nadel—Dr. Canroë was a very well-known physiologist, of course. He wrote a book, you know, saying that Nobel Laureates, what was their first paper—and I can't remember the figures and fact are—most Nobel Laureates did not win the Nobel Prize on what they started their research on. And his

point was that the most important thing is to get the kid in the laboratory, get him doing something. Maybe that he works on ant legs or whatever or fruit flies. And you'll end up winning the Nobel Prize doing something like Wren did. So, you know, you just—it just emphasizes the importance of giving people the opportunity in the research laboratory and let them grow and let them take it to where it takes them.

GIFFORD: When was the Pulmonary Physiology Laboratory established?

WOLFE: Well, that's a pretty fancy word. I wouldn't even use that. It's called Cardio-Pulmonary. I didn't realize that was in there. I guess after I came on the staff, Dr. Saveston had ways of putting people in areas which he wanted to strengthen. And I became the man who was going to strengthen our pulmonary and cardio-pulmonary investigations. That was when all this oxygen toxicity and the baboons and all the work with the primates started back in the late seventies. So I guess that's about when it started.

GIFFORD: Can you describe the evolution of the work in your lab?

WOLFE: Well, I kind of touched on that as I went from pulmonary embolism to airways to surfactant to oxygen to lung injury.

GIFFORD: Yes.

WOLFE: I'm just kind of looking through here to see how to answer that question, when did the laboratory really work? And I'd say, you know, I would say probably in about 1973-74. I came on the staff in '72. So about that time I would say that laboratory was pretty well established.

Like I say, a lot of the work then was done on the primate to solve some of these questions or at least to try and answer some of the questions with regard to lung injury.

GIFFORD: Now, in addition to oxygen toxicity, you do work on septic shock?

WOLFE: Right. As the lung was a target organ many times in septic shock. In other words, the lung becomes the primary problem.

GIFFORD: Are there any other topics that are currently investigated in your lab that we need to talk about?

WOLFE: Well, there's been a lot of changes in the last three or four years. Like I say, we've gotten more molecular ourselves. We're into a lot of stuff with oncology and cancer and oncogenes and P-53, the molecule of the year. We've been pretty busy with a lot of things with \_\_\_\_\_ function and clinical studies of that nature. Dr. Dewey \_\_\_\_\_'s done a tremendous amount of work with regard to the lung. I'll give you some of the most important things here. I can't seem to find it.

GIFFORD: Well, we will have another opportunity to do that. Let me go back to a phrase you used a little bit earlier: You talked about Dr. Sabiston has ways of doing. Can you talk about Dr. Sabiston's ways a little bit?

WOLFE: Well, I guess—I think...let me finish up the last few sentences there before then.

GIFFORD: Alright.

WOLFE: As the investigations are concerned, it's been heavily towards oncology and carcinoma of the esophagus and our interest in that and carcinoma of the lung. And a lot of this is where the molecular thrust is being made now, is to look at ways or what patients should have chemotherapy and radiation perhaps and which ones would not be good responders. There's a lot of stuff on the horizon right now which is really brand new. Dr. Sabiston has ways of doing things. Well, that's a tough question. I hate to put this on the tape, you know. I wouldn't want to be quoted on this.

GIFFORD: You'll see whatever gets off the tape.

WOLFE: He is, you know, you've heard him say, and he uses the Osler quote about the master word "work." And he's an example of that. There's no man who pays more attention to detail and works any harder and does his homework any better than Dr. Sabiston. And I think those who wanted to succeed in the system copied those ways. I mean I certainly copied a lot of his ways. And what you—your performance is geared at meeting pretty high standards. And his ways—if you want to put it—in doing that are number one, giving you the resources, offering you the opportunity. And then I think the subtle thing about it is expecting you to succeed. And I guess maybe—hopefully—I got.... Maybe he does load the gun a little by choosing people who he knows will succeed, which has certainly by the case around here; so I guess it starts with the decision that this person can handle it, and then expecting him to do more than the person thinks. So those are the three ways I'd look at it. He certainly offers you opportunity, he certainly gives you resources. But then he expects you to be successful. And that puts a fair amount of pressure on you.

GIFFORD: In talking by mail with a number of the people who graduated from the program, they were overwhelmingly positive about their experience, except at one point. And that one point was that there was a substantial minority of people who felt that the workload was too heavy, that it broke up families, that it just generally was destructive to human relationships. Is that something that you were conscious of as a student or have been conscious of as a faculty member?

WOLFE: Well, I'd almost disagree with that. I don't think the workload was too high. I've always said that the—and my wife hates to hear it—the best years in my professional life in many ways were in the residency. And I'm speaking as man who got divorced after I finished the chief residency. I used to jokingly say the reason the first marriage held together I was only

home every other night. It would've ended faster if I'd been home every night. It's not a good thing to joke about those things. But I think, you know, when you go through that kind of trauma and tragedy in your life, you've got to have a sense of humor someplace. I don't think this residency or the demands that were placed on me.... The facts are that most of the demands placed on myself were placed on by me. I mean you could've dropped off and did something else when you finished the residency.

So I don't agree with that. I don't know how other people see it. A lot of women—in fact even my first wife—our problem wasn't because I was at the hospital working all the time. That wasn't the reason for conflict within our relationship. So I don't think the fact that you work hard or are in the hospital every other night is an excuse that for why a marriage has problems. It may feed on it. I'm sure you'll get other arguments. But I would pretty much stand down on that personally. So I don't agree with that. I thought that when I was resident here, there was a tremendous amount of camaraderie. Even on nights off I would many times come over here and see John Porter or Bob Anderson if they were on, you know, down in Baker House, and sit around and talk. It may have been different in the eighties. I don't know. But in the late sixties into the seventies, there was a strong bond between most of the people here, and I've enjoyed incredible friendships with many people. In fact, I just got back from a rafting trip with four other residents who were under me over the years. We get together with the families, and everybody's had trauma in their life and stress in their life. It isn't the residency that's brought it out. It's the way that life is.

GIFFORD: Yes, I've got to be careful with that one because I'm not going to have the opportunity to do the in-depth interviewing about that that I—I'm not studying breakups here. I'm studying surgery.

WOLFE: No, but I'm telling you....

GIFFORD: Yes.

WOLFE: My marriage broke up, and I don't blame the residency on it.

GIFFORD: Okay. Doctor, I usually end these interviews by asking what question I should have asked that I didn't ask that would have been more explanatory than what I did? Is there something that I haven't covered that I should have covered that, to your mind, gets at the essence of the history of this department in the time you've been here?

WOLFE: Oh, I've got a lot of things that come to mind; but some of them if you won't ask, I won't talk about them.

GIFFORD: Oh, no, no, no. Please bring the whole list.

WOLFE: I don't know. That's a tough question with the tape recorder going. I think the system personally was strongest in the seventies and middle eighties. I think toward the end of the eighties the residency grew. Dr. Sabiston decided to enlarge it some. And I personally wish that hadn't happened. You know we went from two to three to now four people in the heart program. I think that took away some of the things I talked about earlier, the esprit de corps. As I said to Dr. Sabiston, if you've got two residents, you've got two great guys; you've got four residents, you have one through four. You know what I mean? There's a change in, you know.... Maybe I was romantic and a little old, and I didn't want things to change. And I don't like changes. You know I hate the change, I guess. I kind of wish it had stayed the same. But, you know, I know things are going to grow, change.

So I think, in my view, with coming here in '64 and being here in the sixties and the seventies, I think the end of the eighties we started having things occur in the residency which perhaps have influenced family relationships. I don't have the slightest idea. But I think Dr.

Sabiston made the decision; he thought this was the way things ought to go. And I certainly would never disagree with his decision. I would back his decision. But my own way of thinking, I think it changed the system in such a way that the relationships with people, as I used to like it, has changed. I think it changed with regard to his relationship with the residents. And also in those later eighties, you know, his clinical impact lessened, rightfully so, you know. He became 65 when you shouldn't be operating. Don't put that in. You know, I mean he stepped away from the clinical side of things more, even though he was very active as a teacher. I mean there's no better academic or didactic teacher than Dr. Sabiston. I won't argue that. But from the clinical stand, he backed away. And that changed the relationship with the residents with him somewhat. Because it's a little different when you're in there helping the guy or he's helping you, you know. And you're up to your butt in alligators, and he's not.

So all these things in the last years, and certainly I would say the last ten years maybe, there's been slowly a change in the residency as far as the esprit de corps and how he administers it. And I think that's unfortunate. But I think it's an example of when you have a man who's been here 30 years, that's going to happen. And I accept that. But it's not sometimes the way I wish it would have been personally. [Break]

GIFFORD: Note to me, not to go on transcript: The issue is the relationship between the professor and the chief resident and how intense it could get; and how destructive negative comments could be, depending on the personality of the resident. [Break] Second note: Repeat this interview in September. [Break] Book by Dwight Harkens, H-A-R-K-E-N-S or I-N-S.

WOLFE: E-N. He was a surgeon at Peter Bent Brigham at Harvard.

GIFFORD: Surgeon at Peter Bent Brigham at Harvard. Book about the history of heart surgery early on. Note particularly comments about surgeons working closely together and falling out.

WOLFE: He was the division chief of thoracic surgery then, and I guess there was a little play for power. And one of it was, as I went back to early on, remember that we were doing like one heart a day in 1964-65, okay? At the most. Those operations were being done by Dr. Sealy, Dr. Young, Dr. Brown, Dr. Sabiston. After a period of time—and this just sort of epitomizes how important the residency was to me, but Dr. Sabiston made this residency what it is—was he believed in the Halstedian system which by now, of course, is dead, in a sense. Medicare and other things have slowly killed it off. It's probably good that it was killed off. When I was chief resident, you had almost complete power, you know, and called attendings if you wanted. Now everybody's admitted to an attending. But when Jim Lee was chief resident—he became chief resident finally— Dr. Sabiston took a case and helped Jim Lee do it. The first time a resident had ever done a heart valve by himself.

And that started this system where the resident was to do the case, and a senior man would help. And I think that was a very big strength of this residency, when clinical experience became hands-on rather than you just helping all the time, you were actually starting to do some of these heart surgery cases. We don't know how that influenced the relationship. But Dr. Sabiston took control of what was called in those days the resident service, staff service or the public service, or whatever nonpolitically correct name would be used in this day and age, and then started assigning a resident to do these cases. And that strengthened the residency immensely because it added another dimension. Residents were doing cases before then, but not of this magnitude. I'm not saying that was the reason for the conflict. But I think there was too— Dr. Sabiston trying to get control of the system the way he wanted it, and Dr. Sealy, of course, being an established person. And it's a very unfortunate thing because Dr. Sealy made tremendous contributions while he was here. His laboratory took off in the seventies before he

retired. And all that stuff \_\_\_\_\_. It's really a shame that somehow or another they couldn't have worked better together. Dr. Sealy's had another excellent career actually. So, you know, in a way it might have been—it turned out good for both of them, I guess. It certainly has turned out okay for \_\_\_\_\_.

GIFFORD: What about Ivan Brown?

WOLFE: Ivan Brown was a guy who was as smart as they come. He would see something, though, and jump on it. And then once he'd see where it was going, and then he'd jump off, you know. He got into hypervaric chambers and said, "I don't think this is going to be any good," and he left and took on something else. There's a story, if you want to turn that off, and then you can decide if you want it on. I don't think we will.

GIFFORD: You were talking, sir, about the evolution of loyalty in the department. Can you say a little more about that?

WOLFE: Well, when he first came here, Dr. Sabiston brought Paul Ebert, when he finished the residency at Hopkins, here as really the first man that he would be identified or be closely with, seeing all the other faculty at that time were here were Harts. In the early seventies then, Paul left to go to Cornell to take the job there as chairman. He brought Newland on, and then after Newland came myself and then Bob Anderson and Sam Wells. So he brought on some of his own trainees then as the faculty started to grow, which insured him a degree of loyalty amongst the faculty which he really hadn't enjoyed. [Very strange noises on the other side of the tape; no distinguishable voices heard. Seems to be blank after that point.]

[End of Interview]