

INTERVIEW WITH DR. ROBERT H. JONES

INTERVIEWER: Dr. James Gifford

DATE: July 14, 1994

PLACE: Dr. Jones's office;

Fourth Floor;

Duke Hospital

DR. JAMES GIFFORD: -- there we go. Durham, North Carolina, July 14, 1994. This interview presents Dr. Robert H. Jones, Director of the Surgical Radioisotope Laboratory, Department of Surgery, Duke University Medical Center, Durham, North Carolina. The subject of this interview is Dr. Jones's career, his research interests, the origins and development of the laboratory, and the current cutting edge in the field, and where he sees the field going, followed by whatever comments he chooses to make about the structure of the Department of Surgery as he has experienced it, and the style and accomplishments of its leader. This interview takes place in Dr. Jones's office on the fourth floor of Duke Hospital South. The interviewer is Dr. James Gifford. Dr. Jones, perhaps -- Dr. Jones, perhaps you could begin by saying something about your personal background, and how you came to be a surgeon of the type -- in radioisotopes -- professor of the type that you are, and how you came to be at Duke.

DR. ROBERT H. JONES: Well, I am at Duke because (*sound of chair moving?*) I was at Hopkins, and when I was at Hopkins, I worked for Dr. Sabiston. And he was clearly my reason and motivation for coming to Duke, I would not have even considered coming here had it not been for working with him there. I came here one year after he did. And having worked with

him in the lab at Hopkins, I think I have a lot of insight into that transition period. I had worked with Philip Bard when I was a second-year medical student. Dr. Bard was the Professor of Physiology at Hopkins, and in discussing my future career in surgery, he had really strongly recommended Dr. Sabiston. This was very much to my liking, because I had gone to an evening session which they had at Hopkins, where the young faculty presented their research once a month, and heard Dr. Sabiston present his animal model of pulmonary embolism. And I knew at that time he was working with Dr. Henry Wagner on lung scanning for pulmonary embolism. And his physiologic thinking was really fairly impressive to me, as a surgeon, and in fact Dr. Bard had emphasized to me that Dr. Sabiston had a broad base in basic disciplines, and a strong orientation to the importance of science in clinical practice. And this was the reason he felt Dr. Sabiston would go far. And clearly, Dr. Sabiston did bring that to Duke, and that has been a major feature of his presence here, and we can return to that discussion. I think it's important to understand the heritage from which this came, and that this was the milieu of Hopkins. At that time, Dr. Sabiston actually developed his own career, and he had gone through this experience with Dr. Blalock, having spent time at Walter Reed, in lieu of serving in the military during the Korean War -- he was actually in the military, and with Dr. Gray at Walter Reed, and then time in Oxford, in England. And it was really that time where he had been with these individuals with this basic understanding of science that had bridged into his own interest in coronary disease, pulmonary embolism, and other things. And Dr. Sabiston was not alone, there were many others who were doing this at the time, at Hopkins. And this was strongly related, really, to the development of the National Institutes of Health. They were just down the road a little from Hopkins, it was fairly easy for people to get from Hopkins over there to be interviewed, and it

was a natural marriage. And they exploited that marriage even more than, say, the Boston hospitals did. Early on, they were --

GIFFORD: I remember study sections of people with Hopkins faculty, somebody from Hopkins on every study section, and some of them have two.

JONES: Yeah. So Dr. Sabiston came from that heritage, and when he came to Duke, I think he saw that as a major challenge. He recognized that Duke did have strength in clinical work, but that it had somewhat of a deficit in this bridge of science into the basic disciplines, and then back into the clinical application. Now, having said that, I think it's important to the -- emphasize for fairness to the Duke faculty at that time -- that there was a fair amount going on for the size of the department, which was relatively small. You mentioned Dr. Grimson, and he was doing some fairly forward-thinking research. It was clinical research, to a degree, and it was actually one floor down from where we're sitting here, but it was --

GIFFORD: What was Dr. Grimson doing?

JONES: He was working with primarily dog models, and was using a lot of radiographic in points, but he was particularly interested in ulcer disease, and GI physiology. He was interested in the sympathetic nervous system, he was doing sympathectomies for vascular disease. But he was trying to be very careful, and really modeling this in animals, and doing some good physiologic work. At the same time, Dr. Sealy had some good work going on that eventually led into his electrophysiology surgery which later occurred. Dr. Bill Shingleton was doing some good research. And, of course, Dr. Hart himself had done some excellent work with the use of ultraviolet lights in the operating room, and surgical infections, and so forth. But I think --

GIFFORD: Any others?

JONES: It is -- well, there were, within the --

GIFFORD: Dr. Anlyan?

JONES: Dr. Anlyan had done some excellent work on venous problems, and in fact did some early isotope work himself, that was -- he was really the first one, in surgery here, to do isotope work, and to read some of his early work, it was really very forward-thinking.

GIFFORD: And of course, the one nobody mentions over in this building any more is Joe Beard.

JONES: That's true. Well, of course, Dr. Beard clearly was an individual that was forward-thinking in his work.

GIFFORD: Well, it seems to be his virology lab led to several of the labs that we have today. He begat Dr. Bolognesi, who begat two or three others when he switched off to AIDS.

JONES: The whole Bell building stunk from his chickens down on the first floor, but those chickens did a lot for this entire institution, you're right.

GIFFORD: OK. Before we leave this backward look, I'd like you to tell my a little bit more about Hopkins. First, one hears a lot about the Halstead tradition in surgery, and the Halstead principles. What do those phrases mean?

JONES: They mean attention to detail of technique that primarily are gentle techniques in the handling of tissue, a non-invasive orientation, for the surgeon to do as little as he -- he or she as to do to accomplish the needed task, and also a placing of surgical thought in the basic disciplines. Primarily, in those days, it was physiology. To really understand the physiological process that was underlying the surgical technique that you were applying. And that was the reason that Halstead established -- pushing in the laboratory there, gave a lot of emphasis to early laboratory work, because he understood that link between that and clinical practice. He, himself, I don't think ever spent a lot of time exploring what we would consider very basic science, but he

certainly had an appreciation for it. And that was continued with the later mentor that I remember, Dr. Blalock, who's several removed, at Hopkins, from Halstead, he's really kind of the next major figure. And of course, Dr. Blalock, himself, had done that large amount of research. And the background to his Blalock-Taussig operation clearly was the work he had done in animals, in trying to get pulmonary hypertension. So it was a physiologic question that kind of led into some thinking that was not towards the clinical goal that it eventually was directed towards, but was really a physiologic process that was understood as a end within itself, but became a bridge to a clinical solution.

GIFFORD: And that clinical solution was...?

JONES: The Blalock- Taussig operation.

GIFFORD: I see, OK. Um, now, a second point is, can you say a little bit more about the relationship between Hopkins and the NIH? At the time the NIH was founded? What was that interaction?

JONES: Well, from the perspective of a student, or a person who would want to be trained at Hopkins, it was almost understood that if you were going to stay on at Hopkins, you had to go to the NIH. That was kind of the -- that was the way that you could stay on. And by the time I was there, probably 80% of the people who finished at Hopkins had spent their two years at NIH. So it was just kind of understood that you were going to have to not only go to NIH, but also do a good job at NIH, which meant two years in a fairly basic research environment. So from my perspective, as a student, that was the relationship. Now, I'm certain that at the higher levels, where I did not exchange, with people like Dr. Blalock, that there other cross-friendships and relationships that resulted in the faculty being on --

GIFFORD: You made the point I needed. I was trying to figure out where the two years came from, and that's probably one of its roots.

JONES: I think so.

GIFFORD: Third question I'd like to ask about this is the culture of surgery at Hopkins. And particularly the leadership style of the people that you studied under. As a non-surgeon I've always been kind of interested in the rather rigorous attention that's paid to rank and deference. Can you tell me why that exists in Hopkins, in surgery, and what its benefits are?

JONES: I think the roots of that go way into German surgery, or European surgery, but a lot in German surgery, where there was the Professor, and there was a very strong Chief. And of course, it continues with Halstead, it continued with Blalock. It is a mystique of respect, some degree of awe, it is an acknowledgement of a governance style that can be best termed a benevolent dictatorship. That those who are dictated to permit because it is to their benefit. This hierarchical structure is made of people who themselves understand that at the time, they can be at the top, and will, likely, be at the top, because they are in an environment that is going to evoke so much productivity. That is so very efficient, that you don't have to have a lot of time in administrative detail, that it really permits a good person to make their way up the hierarchy. And, in fact, that was always assumed at Hopkins, and that is that if you ever got into the -- if you ever made the grade, that if you got in and accepted at the Hopkins surgical thing that if you wanted to be chairman somewhere, you could. That was a given. You may not want to, you may not quite make it, or a few people may show some flaws along the way that would cause them to come out. But it was always the mentality, that everybody there, from the first intern on to the top, was a chairman of medicine. And so it didn't bother them to kind of bow down a bit before the Chair of Medicine because they were part of that whole system, and were going to

inherit the benefits. But I think it goes deeper than that. I think there was a certain awe and respect, because these individuals at the top were truly benevolent. They looked after the people they trained as they looked after their children. And they really were involved with them. From a professional perspective, not so much personal. Sometimes, the personal lives were left alone, and that was up to you to have your own personal life. But professionally, you were looked at, and every opportunity was made available for you to advance professionally, and that was really part of the obligation of the chief. And Dr. Blalock did that beautifully, with his people. He placed them -- a lot of times, he almost placed them in their jobs without their consent, but then they kind of consented, because that was truly best for them. Much of that was the culture that really set the tone when Dr. Sabiston came, and it was just amazing to me to see Dr. Sabiston, one year here, after seeing him at Hopkins, because this rather laid-back Southern institution suddenly got a little more starched look. People didn't wear greens in the halls. They did have a demeanor that was a little bit above the general lackadaisical demeanor of their peers in the other training programs. And there was a kind of esprit de corps that began to develop, that hey, surgery can be looked up to in the institution as maybe some of the best of the institution, and we don't have to consider ourselves second to anybody. And it was kind of an excitement almost like a basketball team that had not won a national championship, and all of a sudden, they say, "We can win the national championship." And it just was that kind of change of orientation. There was a little pain involved. Some people didn't quite like that. But I think most people bought into that very quickly down here.

GIFFORD: All right, let me play with a couple of spins off of that one. First of all, Dr. Hart was, himself, one of Halstead's residents, and had been Chairman at Duke for 30 years, 33 if you count Dr. Gardner's brief interim tenure. What is it that you think -- did the first generation of

Duke surgeons follow the Hopkins regimen? They all came from there. What do you think broke that down in the Hart regime?

JONES: No, no, they really did. And I don't mean, in any way, to suggest that Dr. Hart was sloppy, or in any way --

GIFFORD: Oh, no, I wouldn't say 'sloppy', but --

JONES: -- careless. I would say that Dr. Hart reflected his times, and his times were a generation earlier than the later times. Remember, NIH was not formed until after the Second World War. I mean, there wasn't much basic science. I mean, the discovery of penicillin --

GIFFORD: I'm talking about the difference, now. Obviously, Dr. Hart commanded respect. You couldn't run the PDC without it.

JONES: Dr. Hart did not take the benevolent dictatorship model. He took the family model. And --

GIFFORD: You think this was a function of his upbringing in the South?

JONES: I think so. I think a lot of it had to do with his wife. I think a lot of it had to do with the fact that Duke was much smaller than Hopkins, and you could use that model. But I think everyone that knew Dr. Hart -- and I only knew him at a distance that would come from an intern to somebody who had served as President of the University. So he was very warm to me, and very genuine, but I was in so much awe, and there was just a natural gawk, that I can't really give you Dr. Hart's inner thinking. But those who knew him well who are still on the faculty always talked about how they felt this sustenance, how that they felt part of his family. And they talked about the Duke family. And they felt at peace with that, and they would have laid their lives down for Dr. Hart because of that loyalty. So he had the same kind of loyalty that those leaders at Hopkins had. But also enjoyed with it more warmth. And I think there was a little difference

in the time, a little difference in the size of the department -- the end result was still excellent. And I'm sure that you know that.

GIFFORD: The other spin is, who was surgery feeling second to when you came?

JONES: I think there's always the tendency in medicine to consider surgeons to be those who couldn't quite catch all the subtleties and all the nuances of disease, and had to be in the plumbing business to a degree. And so there was a little bit of this natural tension, that's always been there, between medicine and surgery. And Medicine had been good here, and I think the surgeons had been totally accepted. But they had not thought of themselves as actually kind of being the leaders, even though they were the leaders of the institution, among the younger trainees, there was not that expectation. There was the expectation that they would go to New Burn and take out gall bladders, not that they were going to be the Chairman of the Department at Harvard. And that was kind of a different mindset.

GIFFORD: And would the students in the Department of Medicine have thought about something more exalted than going to New Burn and handing out pills?

JONES: I think that there was a little bit more of that thinking in medicine than surgery.

GIFFORD: I see. And that would have been attributable, basically, to Dr. Stead?

JONES: Perhaps. Or perhaps to the people before him. I don't understand all of the reasons, but I think that there was some of that difference.

GIFFORD: I don't disagree with you, I just want to see how it looks from a surgeon's point of view.

JONES: Right.

GIFFORD: OK. Let's talk specifically about -- you followed Dr. Sabiston to Duke. Now, you are presently head of something called the surgical radioisotope laboratory. When did that start?

JONES: That really started at Hopkins, because, as I mentioned to you, I started to work in the lab with Dr. Sabiston, and at that time, he was collaborating with Henry Wagner using radionuclides to measure blood flow in the lungs. And particularly to diagnose a pulmonary embolism, a clot passing through the lungs that could be lethal. And after we had worked about two or three weeks, he called me in the office one day and said, "Bob? I have to tell you that I'm going to be leaving Hopkins, and I'm not going to be able to work with you in the laboratory any more. I've enjoyed our time together, but it's just going to have to change. And what I want I want you to do is go and work with Dr. Henry Wagner." And of course, if I'd had any choice, I'd have chosen some other surgeon, because I really wanted to work with a surgeon, because I was going to go into surgery. But in those days, you didn't challenge what you were told, you didn't - you did what you were told, and so I said, "OK." And I went over to work with Henry Wagner. Meanwhile, Dr. Sabiston, in a week or two, was packed up, and came to Duke. It wasn't long before he was here. So he and I had a fairly short time together. But it turned out Henry Wagner was one of the most dynamic people I've ever known, and he was very interested in this relatively new discipline, of using isotopes to detect it externally to mirror physiologic processes. And he and I got busy working not only with blood flow, but also ventilation in the box. But that fit in right with an interest I had in pulmonary and cardiac disease. And he and I pursued this some, and never did really publish all that much. But when I came down here as an intern, Dr. Sabiston encouraged me to continue, and I kind of worked through some clinical material on this new lung scanning technique. But then I got drafted into the Air Force. And they didn't want me until a few months later, I finished my internship in July of '66, and they kind of wanted me in September. So my wife and I borrowed money from relatives and went to Europe for a couple of months. We didn't have any choice, came back to New York expecting my orders to be here.

There were no orders, and so we were flat broke, and owed it all to our relatives, and so I called Dr. Sabiston, I said, "Dr. Sabiston, I'm sorry, but I really need a job. They're not going to take me into the Air Force until December, I've got three months, two or three months to do nothing, and I need to make some money." And he said, "Well, Bob, come on down, we'll find something for you to do." So I came in the door and he said, "I'd like you to just work in anesthesia for me." I said, "That's fine." I said, "You know, that's not very much work. That's just eight hours a day. Is there any chance you have something else you want me to do?" He said, "Well, why don't you work on your isotope work?" I said, "Well, what do you want me to do?" And he said, "I want you to really measure, quantitatively, blood flow and air flow in each region in the lung. Why don't you work on that?" And I said, "Fine." I didn't know how I was going to do it, but I walked down to one of the fellows in nuclear medicine, Jack Goodrich, and I said, "You know, I've been up there at Hopkins working with Henry Wagner, and I want to do some work down here. And we've been taking these films, and we've been using the densitometer, and then just write down by hand." I said, "Isn't there a way to computerize this sort of thing?" And he said, "Well, you know, that's interesting, because down here at Jack Tar Hotel, there's some company that says they can computerize this radiation detection over the chest." And he said, "They're supposed to be bringing an instrument down, why don't we go look at it?" And so he and I went and both -- yes, it had a little live EM tape, and we thought we could put it on a computer and so forth. And so we talked him into leaving that thing for a few weeks down here while we worked on it. And that was about October of '66. And on the second floor, we began to fool with this thing, and to make a long story short, it took us about six months to ever get our first data off -- really numeric data off the thing. And it was (inaudible) computerized, they'd never gotten any computer data, but I worked with the Duke computer center, and we eventually got it done. And

it turns out that was really the first time that anybody had computerized a dynamic radionuclide study. That is, where you take very rapid pictures of the movement isotope -- and at that time, we were still working with dogs, and working with lungs. We'd blow up, and it would be Dr. Sabiston's model, and we make a pulmonary embolism in one lung, and show that the gas could not go into that lung, nor would the blood flow go to that lung. And it was mainly to develop this technology to use in patients. But we began to notice, going through, that it went through the heart. And we could actually begin to see some beats of the heart, in the data. We knew the instrument was pretty (inaudible), even so, while I was in the military for those two years, I continued to come back and work with the company that made that equipment. And we made a whole new generation of equipment. So I came back from the military real ready to use that on patients. And while I was starting my surgical residency, we would also do this with patients.

GIFFORD: What, exactly, did you do with the patients?

JONES: Well, very quickly, we began to notice that the information available for the heart was going to be much more valuable than the lungs. The lungs could be used, but you could actually see this material move through the heart, and you could actually begin to measure how good the heart could squeeze, could function. Now before, you had to but a catheter in to do that. This was really the first good way to non-invasively image the heart. Over the 10 or 15 years it went on that we kind of toured through this process development -- which required computer analysis, which required knowing how to really process a large amount of mathematical data -- a lot of basic types of interfaces with a lot of people here at Duke. We basically developed a non-invasive way to measure heart function. And we're some of the first people to do it. There were others that worked in other laboratories that basically (inaudible). And this, now, is a fairly standard thing that's done at this institution and at other places. We've used this, over the years,

to measure function in a lot of situations, particularly during exercise. And we found that the amount of bad function during exercise is a marker of how severe your heart disease -- whether you really need surgery. And it turns out that this thing that we developed was one of the very best markers to decide who needs the clinical work that I do, which is coronary bypass surgery. And it's a beautiful illustration of the model that Dr. Sabiston had used so well, that you start with a basic problem, you're not totally sure where you're going with it. In this case, we kind of started with a lung problem. We looked at some new technology. We found, "Well, it's actually more applicable to the heart." And we actually pursued it -- with a lot of interfaces in basic disciplines in many ways, electronics, and computer science, and mathematics, and in decision analysis, and structure, and going through the Duke cardiovascular database, and into epidemiologic studies to show its use in the clinical environment. So that's a fairly long-winded answer, but I think my career, with Dr. Sabiston's mentorship, is a good illustration of how you take something that's not directly within surgery, but it's kind of tangentially related, and it can really make a full circle and come back and greatly impact surgery. Let me give you, also, another specific answer of what the radioisotope lab was. Where you're sitting, and next door, is where we did the first studies of patients. And we were, at that time, bringing 8, 10, 12 patients up here a day and doing studies. Clinicians were calling us, wanting to know the results, and try and decide whether they were going to operate on patients, or where they were going to (inaudible) and so forth. And at that point, I went to Dr. Sabiston and I said, "Look, Dr. Sabiston, this is great, but I didn't go into surgery to do all of this stuff that's clinically a little bit tangential -- important to surgery, but not mainstream." He said, "I know." And I said, "Do you want us to institutionalize this within the department? Or shall we give it to the hospital?" And at that time, medicine, when they would develop new technology, would kind of keep it within

medicine. And they would then start charging for that, and rendering a service, and keeping it within -- uh, he said, "Well, we could do whichever you prefer, but I think the best model is to let the institution take it. It really belongs in nuclear medicine. It belongs down in radiology, let's make a transition and give it down there." And I fully agreed with that. And I think that's another aspect of Dr. Sabiston that's -- at that time, was generating \$500,000 or \$1,000,000 in profit. We're talking about 198-- '77, '78, '80. And that could have staying within the Department of Surgery. We could have hired our own technicians, we could have just run it as a service, charge for the studies, and so forth. And made it independent. But he chose, and I think wisely so, to go ahead and move that back in under the general Duke hospital umbrella, and not to cash in on that early investment he had made, if you will. And I think that just, again, illustrates his view of how this basic work should be developed and then turned into something that is useful clinically. And then put aside -- and then other things should go on. So my work now does not involve at all direct interaction with these isotope tests. I've used a lot of the principles that I've learned, and right now, I'm interested in health care policy, and served as a Project Director for the National Unstable Angina Guideline. One chapter in there is on basic testing, and some of the information is on some of these tests that we helped develop, but it's a much broader scale, and that is, where do these tests interface with a much broader clinical piece of information? How are they fit with everything else? And so, again, it's, to some degree, come full circle.

GIFFORD: I brought along this green book description of what it is you're currently doing. Could you talk a little bit about the camera that was developed here? And then talk about what's going on in the laboratory now in semi-layman's language?

JONES: Right. Well, the camera, as I say, was first envisioned by another individual, and was put together by this company, and was just a matrix of individual detectors that put their information rapidly on computer tape. But it was so primitive, it was more just an idea, it wasn't really envisioned to use for dynamic studies. We were the ones to envision to use it for dynamic studies. They were going to just make pictures, static pictures. Just like still photographs versus a moving picture. And we kind of envisioned using it for the moving picture. To this degree, we needed much more information, because when you take pictures very rapidly, you have to be able to have enough information in those pictures to make the pictures interpretable. And furthermore, you have to use a lot of mathematics to turn them into measurements of heart function, blood flow, other things you can use. And so that latter part has been our contribution. It started in '66, where we showed that you needed a lot more counts, we went back to the company that made a new piece of equipment --

GIFFORD: What was the company?

JONES: It's Baird Atomic, and it was in Bedford, Massachusetts. And then we did some of the very first of these studies here with a high-count-rate camera, in the '70s, where the company would bring the camera down here, and we would be on the second floor, doing animals, doing patients, and so forth. And we have gone through several generations that, one point, when we made this new instrument, called the Centicore, I commuted once a month to Boston, and we'd spend a whole day up there with the engineers. We would talk about how you needed to design it. They would then work for the next month, and I would come back and we'd respond to what they did. They marketed this, and it did very well. At that time, the company decided to divest themselves of this unit, because they thought it was a sellable unit, and it took them a while to sell it. Eventually, it was bought by a group in Milwaukee, and has become a commercially

viable instrument. The instrument we developed is still there, although it has been made with several changes, and with the revolution of computer technology now, much of what we had originally designed has been commercialized in Mac computers, and they can just buy them through the Mac computer now, and don't have to have quite what we fully designed. But still, the concepts are there, and very much alive.

GIFFORD: Can you say a little bit about what the lines of research currently are in the laboratory?

JONES: Well, what I'm doing right now is not directly related to radionuclides, as I said. I guess it's more indirectly related. And that is, we have pretty much moved the day-to-day patient studies out of this environment. We first removed just the clinical studies out and did the research, now we've moved even the research studies out. But what we are interested in doing is placing the information from these studies, in large numbers of patients -- there have been more than 6,000 patients studied at Duke with this specific kind of approach -- and place that in context with other clinical information. And so that is more of a broad-based epidemiologic study that is based in the Duke cardiovascular database, and that thing is blended with to her types of information that has come up with our setting the Unstable Angina Guideline. And so our work, for example, here's the Unstable Angina Guideline that we serve as the Project Director, that's funded through a federal grant, that's about a \$1,000,000 grant. And it integrates all the aspects of information you would use to bring upon the coronary disease patient, one of which is the study that we have developed. But currently, putting in a request for a contract for the Acute Myocardial Infarction Guideline, or the Heart Attack Guideline. So that is the direction our work is going now. So it's probably not appropriate to call this an isotope lab any

more, even though it started as that. It's much more a lab of information processing, or the development of clinical information used in the management of patients.

GIFFORD: That brings up a question that I've wanted to ask somebody. Obviously the labs that exist here now are not all the labs that have existed since Dr. Sabiston came.

JONES: That's right.

GIFFORD: How is it determined when a laboratory ceases to exist? Or ceases to function?

JONES: Well, that's another bit of Dr. Sabiston's genius, and that is, he gives you almost full freedom to do and pursue areas that you view to be interesting. And so instead of just staying very static, you build upon your work, and it matures and changes as you go. And that's the beauty of what Dr. Sabiston's done for the people who are here, and as you talk to them, you will see that their work is all a continuum, and it can be traced and related, but it often has changed greatly in scope. And my work's a good example of that. Intellectually, all my work relates back to that very earliest thing that I did with him, but if you look specifically, I'm a long ways now from lung scanning in dogs, for example, what I do in my research.

GIFFORD: Just as an aside, you use the word 'freedom'. That word keeps coming out. Dr. Hart said, after he looked back on his career, that any success he had as a chairman depended upon there being an appropriate ratio of freedom to resources. And since there were never enough resources, the whole thing had depended upon a generous grant of freedom. Now, in his generation, he had the freedom. And he more or less told everybody else what to do, within a hierarchical structure. But he, as the Chairman, had the freedom. I'm not sure of this yet, but it look to me as though Dr. Sabiston has not had freedom in exactly that way, because he has had to be responsible in so many more directions, particularly the outside agency. But he has somehow managed to give to everybody who works for him the same gift of freedom that Hart

said was essential for his success. It's just one -- it just starts one step lower in the pyramid. Um --

JONES: And that's been Dr. Sabiston's genius. He has always been able to keep you relatively free from worrying about how you were going to pay your way, of how your career should go. As long as you were productive and doing good things, he tended take away a lot of that anxiety. Now, it's interesting -- it's almost paradoxical that almost everybody's paid their way. I think, at the most, I've had a bridging of maybe a Sec 1 secretary for three months that I couldn't fund since I've been here. And I probably have brought in maybe \$10,000,000 in grants to this department while I've been here, I mean, it would be a matter of record, but I don't know exactly what it is. But a large amount of money that I have generated, of direct, indirect costs. Some of which has gone back to the department. But it's interesting that I have never spent a lot of time being anxious about where things were going to come from. Now, I don't know that that's open - - going to be the future. But Dr. Sabiston had a way of reassuring you that as long as you worked hard and put in grant requests, and tried your best to get the kind of funding that you needed, and as long as you were working hard, that he would cover up the gaps. And for most of us, there haven't been that many gaps. Because of that reassurance. It's kind of almost paradoxical.

GIFFORD: OK. Um, let's talk about a few of the things that I picked up as I went along that had some edge to it. I did a survey. Some of the respondents to that survey indicated that the level of expectation for residents was so high that it was fairly destructive of human relationships, particularly marriages. Do you think that's an accurate statement?

JONES: Yes, I think it probably is. I think that there's no question that the focus of department has been for excellence and intensity. Parallel to the Green Beret in the military, parallel to any

other unit that has high expectations of its peers. There's a giant amount of peer pressure that has been nurtured, to a degree. Not necessarily cultivated, but certainly not defused. The quality of presentations in conference, or Dr. Sabiston's mentioning of success that any individual would have. Well, that would be good for that individual to have it mentioned, but for that individual's peers, whether they're in the house, staff, or faculty, that's almost a threat. "Well, I need to get this kind of accomplishment." And so there clearly has been engendering of a large amount of peer pressure. I don't know that you could blame every faulty marriage that's ever occurred here on that, I mean, that is kind of the fact of life on a lot of high-performance America, I don't know if it's any different in the culture of a large business, and you want to be the CEO, for example. Or any other competitive sort of thing. Show business, or athletics, or anything else that might be considered highly competitive. But there's no question that there was a spirit of competition. Now, it was never a cutthroat competition. It was genteel, on the surface. But it was there, and perhaps even the genteelness of it almost made it more threatening. It was not an overt kind of, "You're not as good as he." It was more subtle, so that everybody always felt like they needed to give just a little more.

GIFFORD: And if you weren't doing it, you couldn't protest, because it was genteel.

JONES: That's right.

GIFFORD: I see. Um, you mentioned Dr. Will Sealy. Dr. Sealy departed these premises under circumstances that have been variously reported. Do you want to offer your analysis of why that's -- what occurred?

JONES: Dr. Sealy left at the standard retirement time, and it was kind of his official time to go, and he certainly was not invited to stay. I mean, it was more a polite official distancing. But it went much deeper than that. Dr. Sealy -- to take Dr. Sabiston's side, if you will, now I'll take Dr.

Sealy's side. From Dr. Sealy's -- or rather, Dr. Sabiston's side, Dr. Sealy never came to peace with the fact that Dr. Sabiston was really, truly, the chairman of the department. Dr. Sealy kind of viewed himself as the leader of cardiac surgery here, he always felt he was the one that developed it at Duke, that he should still have a lot of options in that role, to have a lot of freedom to do what he wanted with the residents, to kind of say how things should go. He felt very much threatened by Dr. Sabiston as one of the interlopers from Hopkins. He often derided the Hopkins concept, he saw himself as, to some degree, the representative of the Hart era, and the things that were good there. And to some degree, I guess, he was. He had a lot of skills that Dr. Sabiston didn't have. He could build a lot of warm allegiances with people, tended to do so particularly with those who fell out of favor with Dr. Sabiston. He had a lot of clinical skill that was appreciated that Dr. Sabiston didn't take time for, kind of poring over X-rays, a lot of little details. You could go and be more of a peer with Dr. Sealy than you could with Dr. Sabiston, you could kind of get his wisdom about how to handle a patient, whereas Dr. Sabiston would kind of quiz you about how to handle the patient. So there was a difference of style, and differences of allegiance. It was unfortunate that the two could not see each other's strengths, and complement them. And I think it was just a situation that was not to be. If Dr. Sealy could have acquiesced slightly to Dr. Sabiston's hierarchical role, perhaps Dr. Sabiston then could have acknowledged Dr. Sealy more for his accomplishments. Despite that cat/dog arrangement, it never was overt. It was kept enough under the surface that you did not have to necessarily declare yourself in one camp or the other. For residents. And so at least that side was good. It was just a bit tragic that Dr. Sealy could not be appreciated more officially for what he did for Duke, it was a bit tragic that Dr. Sealy couldn't calm himself down and appreciate what Dr.

Sabiston was doing for Duke, too. So there was a little bit of a rift that didn't really have to be there, but was.

GIFFORD: Ivan Brown?

JONES: Ivan was quite a character. He did a lot for Duke also. Ivan was just an unbelievable slow surgeon, and was not of the modern cardiac surgery ilk. And it just amazed me as a resident that he would operate and -- that he would be allowed to operate, quite frankly. On the other hand, he was a great, great, inventive sort of guy. And he really was one of the geniuses of that era of surgery. I think it was appropriate that he left. It was always surprised me, because after, he went into private practice, and I think he's done well in private practice. But it was strange to know I -- he was not the surgical, decisive, clinical personality. He certainly wasn't the surgeon's surgeon that Glenn Young was, for example.

GIFFORD: How would you describe, beyond what you already have, Dr. Sabiston's personal leadership style?

JONES: He focused primarily on residents. He let his leadership to the faculty be pretty much by example. And my biggest --

(end of tape 1, side 1; side 2 begins)

JONES: What was that question asked?

GIFFORD: Dr. Sabiston's style of leadership, you were talking about how -- faculty --

JONES: Oh, yeah, among the faculty, I was always frustrated, as a kind of hot-headed intern, thinking I knew how things ought to be done, that Dr. Sabiston didn't sit more on the faculty. Dr. Grimson prepped his patients with bare hands, for example, which I thought was horrible, instead of having gloves on, and I thought that Dr. Sabiston should take control of that. Dr. Grimson was known for being a little rough as a surgeon, sometimes we joked about how rough

he was. And I would bring that to Dr. Sabiston's attention and think that he ought to sit on his faculty more. He used to tone me down and say, "Now, listen, Bob, some changes have to be made slowly," and that sort of thing. But I think he had, of course, a more mature view, that not everybody has to fit every mold, exactly. But he let the faculty pretty much do anything they wanted, but there was a line they could not go beyond, and that was primarily with residents. They could not abuse the residents, and they could not get the residents in to kind of camps. He was very clear that the residents should be treated as a unit, not trying to get one resident against the other, and that there should be a general group spirit. And he, at least openly, was always complimentary of the faculty. He would rarely say anything negative to any of the residents about any of the faculty. And, in fact, the only slight things he would hint with being negative were kind of in confidence, and they were not really anything derogatory. He would just validate some of my feelings of concern and say, "Yes, but look at these strengths." And go forward. I think his leadership -- that he eventually developed, matured all the way through, even to the time that I've been on the faculty. And that you don't really feel a lot of direct pressure from Dr. Sabiston, he doesn't necessarily lean on the faculty, and never has, as much as on the residents. But the pressure comes indirectly, through the peer setting within the department. And if you get too far off the line, you know that you are kind of outside the system, if you will. And so there is that pressure, but it's not a direct pressure.

GIFFORD: Dr. Sabiston maintains a distance, even from the faculty.

JONES: Yes. In fact, all these years, I don't call him 'Dave', and I probably know him as well as anybody on the faculty. He's never asked me to call him 'Dave'. If I look at him and called him 'Dave', it would not be an insult to him --

GIFFORD: But it would surprise him.

JONES: It would surprise him. And --

GIFFORD: I'm told that only Dr. Young crosses that line.

JONES: That's right, I think that's probably true. Although others who are much younger and much junior to me don't hesitate to call him 'Dave', and for them, that's appropriate.

GIFFORD: He's a grandfather rather than a father.

JONES: That's right. There's a certain respect that those of us who've trained under him that we're glad to have. In other words, that's who he is, and that's fine. We don't necessarily want him to now be our friend, if you will. That's fine. We're glad to accept him and respect him as we do in the relationship that he wanted. And he was very close to Brad, for example.

GIFFORD: Brad...?

JONES: Brad. James Bradsher. In his lab. But Brad always called him 'Dr. Sabiston'. And so the fact that you call him 'Dr. Sabiston' is not necessarily that you were distant from him.

GIFFORD: But you weren't his buddy.

JONES: No. But you were buddies in a professional sense. You were related closely, professionally. But he didn't invade your private space, and he didn't want you to invade his.

GIFFORD: All right. I have heard it said in some previous interviews that the heyday of this department sort of petered out when the residency got too big a few years ago. When the size of the residency was doubled, and it became more difficult to maintain the kind of relationships that have sustained the program over a period of two decades. Do you buy into that at all?

JONES: No. Dr. Sabiston's strength was also his flaw -- as is often the case with great men. He was such a good leader that he, in the early years, didn't have much need to delegate any sort of power. And it was much more efficient not to do so, and to just optimize people's individual performance. As we grew, and as medicine has changed, situations have become increasingly

complex, and more of them, so that one person cannot do everything. And Dr. Sabiston did not make the transition in his leadership style. He always felt it would be more efficient -- that he could make up for the lack of leadership in some areas by being as efficient himself. In other words, he always said to me when I would say, "You ought to appoint a Chief of General, or a Chief of Thoracic," he would say, "Well, the trouble I get into fighting around with that person, and trying to discuss, and so forth, will take more time than just making the right decision and going forward with it." And I think that's what's happened to the department. I think, had he been able to school some of his trusted people in training the residents, and said, "OK, now you take over the training program, and you come to me with the big decisions." If he would have appointed the Chief of General and said, "Now, you take care of all the bed allocations and so forth. Now, you and I will discuss the faculty we want to here, but here's some of the nitty-gritty you can do," and defined those roles, the same with cardiac and so forth, I think he would be much happier and I think the department would be stronger. And I said that directly to him at the time. He didn't particularly want to hear it, and I may be wrong. It may be that he was right. That had we done that, we would have fragmented, we would have had a lot more fighting and bickering than we've had, and clearly we've made a nice transition to a new chairman, and so you can't argue with success in those regards. But I think it was not so much the size, I think it was just not putting in place a structure that could deal with the growing complexity. And it's not just size.

(break in tape)

GIFFORD: Doctor, you've been very generous with your time. I usually end these interviews by asking my interviewee what question I should have asked but didn't that would have

crystallized things more completely. Do you have such a question? If you do, please ask it and answer it.

JONES: Thank you. I think we've been so thorough, and I have bared my soul to you so much, I hope you will be cautious in the way you use this information, and it's all intended to reflect positively on Duke, and on the people I've spoken about, who I respect enormously, and I think you could feel that through my answers. I think you have, really, a good view and a good perspective of, at least, my views of this place and how it's evolved. And I think we have some of the best that have come from the Hopkins system, I think it is now up to those of us who stay here to try to turn those principles into different ways of effecting the same goals. But I think, fortunately, there are a lot of good people here who still love the concept of balancing research, and education, and excellence, and trying to really make this, still, the center of American surgery as it can be, and I think the future's exciting, and I think Dr. Sabiston's given us a great base to build on, and I hope that's come through in these answers. But I think we've gone through, pretty much, all the details.

GIFFORD: Well I, for myself, I think you put your finger on the key to the future some time ago, when you mentioned the declining resources base. Dr. Hart said he succeeded because he had no resources, but had great freedom. Dr. Sabiston's situation, again to use your word, because more complex, and he lost something of the individual freedom that Dr. Hart enjoyed. But he gained corresponding resources, and was able to maintain the freedom for everybody in his pyramid. Now, we seem to be approaching a situation in which more freedom and resources are going to be somewhat curtailed, and the question becomes whether that genius that I think both men had can be sustained in tight circumstances.

JONES: It will be a challenge, but we've got a good man in Bob Anderson.

GIFFORD: Thank you, doctor.

JONES: Thank you, I enjoyed it.

(end of interview)