

**ARMED FORCES INSTITUTE OF PATHOLOGY
ORAL HISTORY PROGRAM**

SUBJECT: Dr. Lent C. Johnson
INTERVIEWER: Charles Stuart Kennedy
DATES: April 21 and June 2, 1993
[This transcript was not edited by Dr. Johnson]

Q: Dr. Johnson, could you give me a bit about yourself--when you were born, where, and then about your family background and where you grew up.

DR. JOHNSON: I was born out in Nebraska, the year of the Halley Comet, 1910. And in February, the temperature was down well below zero and there was a three-day blizzard going on. I was pronounced dead before I was born, and therefore the things were not brought out. Out in the western area, where it was out in farmland, the doctors and everybody was talked about. The good doctor was pretty frustrated, and so he'd drink at times. The other guy was not too good, but he didn't drink. So he was called, and he damn near killed my mother. And the other guy had to be brought in, in order to save her. It turned out I was alive after all, and so that's how I got started.

We lived there for maybe two and a half years.

Q: What were your parents doing then?

DR. JOHNSON: Farming. My father's family starts in New England, and each generation drops off a few states farther west. And he homesteaded out in Nebraska.

Q: In a sod hut?

DR. JOHNSON: In a sod hut... [static] and went into farming and whatever else he could do in order to get some kind of an income. Three of his brothers went out with him, but they didn't stay; they went back to Iowa. My mother was born in Sweden. At eight years of age she came to this country, Chicago, and... homesteaded out in Nebraska. They were not too far... My father was born before the Civil War, 18... So... I'm the first one. My father was... Then my mother... Nebraska. later on, went through the ...

Q: That was 1893.

DR. JOHNSON: She met my father in 1909, and they were married, and I was born. And knowing what Chicago had to offer in contrast to Nebraska, she talked about the wisdom of coming to Chicago. My father sold his property and got well screwed on it, because he was one of these individuals who was totally without guile and didn't... he sold his farm. We came to Chicago and lived in the...you know Studs Terkel?

Q: Yes.

DR. JOHNSON: One of his first books was *Division Street*. Well, we lived right along that area for a while. And then, when I was a first grader or second grader in school, I was handled by older boys who tried to set fire to my penis. We decided to move out, and we moved up to the far north side of Chicago. There we stayed, and I went through public school, and I went ... school there. Then, when it came time to go to high school, my mother was very concerned about the fact that there was a large number of girls being thrown out of Nicholson High School (which I should have gone to) because they were pregnant. So my mother said, "None of this stuff." So instead, under difficult circumstances, we moved from that area on the north side straight west from there. We lived in basements and attics... one period... we lived in an ordinary apartment and went to a school which was a religious... called North Park College. It was a seminary, a junior college, and a four-year academy. I went there for the four-year academy. I spent only three years there--took the four-year course in three years--and had the good fortune to be able to assist in the zoology course of the college... and so forth and so on. And the thing that I remember is that he brought a pigeon for me, and then he disappeared. In Mother's reconstruction, he probably had escaped from one of the schools for delinquent boys and was... disappeared as far as the police were concerned. And time after time, people who were worse off than we were, were brought into our home even though things were...

Q: What type of work was your father doing?

DR. JOHNSON: When he got to Chicago?

Q: Yes.

DR. JOHNSON: For a while there, my mother baked cookies, and my father would try to sell them. Then he got something which may have been a perforated ulcer or a mild heart attack or something like that. And for quite a while, he was not able to do anything. Subsequently, through people that we met at the church, he got a chance to work in Marshall Fields, which is a big department store, for a year or so, and then subsequently at the fair in the Boston store. He worked in those places there. When finally he was old enough so he couldn't work at any of those places, because of his age they wouldn't have him, he sold notions that a company put out. He went from house to house, trying to sell.

The real organizing aspect of the home was through my mother.

After North Park, I went to the University of Chicago for undergraduate and for medical school and for post-medical school, in part. So I was almost ten years there. We had to leave the north side and move down into the quasi-ghetto area again, when I got to the University of Chicago, because of the expenses. At that time, it was \$300 a year for tuition. The maximum income that we ever had, with my father and mother, my brother

and myself, when we were working on... was probably about \$1,000 a year. That was the maximum.

Q: So you went to the University of Chicago when, about in the early Thirties?

DR. JOHNSON: No, '27 to '31. And medical school was '31 to '35. This was quite an experience... gave all kinds of...

Q: What inspired you? Here was a family that was just hanging on. College was not in the cards for most people coming from your financial situation.

DR. JOHNSON: ... no matter what... The decision to go into medicine came as a result of reading... David Livingstone's life, and I read it at eight years of age. That was the decision.

Q: He was the explorer and medical missionary in Africa.

DR. JOHNSON: Right. This was, of course, an impossible situation financially. But my parents said, "If you buckle down and work to the best you can, we'll support you along the way." My brother eventually... He dropped out for several years while I finished my training, and then went back and got...

Q: What pushed you toward medical training?

DR. JOHNSON: Just reading...

Q: How about your brother, the same thing?

DR. JOHNSON: He tended to follow what I would do.

Q: Could you give a little feel of what it was like when you were going to the University of Chicago, what the dynamics were there? This is before Hutchinson came in.

DR. JOHNSON: That's right, Hutchinson came while I was there. So I saw the transition from...what was his name, after playing around too much he had to be gotten rid of.

Q: Well, anyway, the name that rings through the century or rings through the generation is Hutchinson.

DR. JOHNSON: When I went there, most of my friends were people who came from one or two, and sometimes just barely, generations that had come to this country, and came from various parts of the city. We were not among the ones that you'd pay attention

to. Had nothing to do with fraternities and all these parties and so forth.

Q: None of your friends, like Leopold and Loeb and those guys.

DR. JOHNSON: We wouldn't have anything to do with them. Of course, that was already over before I got there. Oh, I remember the details of that very clearly, and a lot of other things like that.

Since I wanted to go to medical school, it was clear that I had to have a lot of chemistry, a lot of physics, and so forth. Despite that, one of the things that very much frustrated me was, in four years, I couldn't take a course in every department. I wanted to... everything, and I couldn't. So I took the things I had to take, and then tried to take a few other things here and there.

The University of Chicago ran on a quarter system instead of the semester system, of three quarters, from the fall to the spring, and the summer quarter was usually used by those who wanted to come and gradually get their degree while doing something else. For instance, you might be a professor, and we had some like that, in another school, who would then come and get their degrees by summer school only. My brother and I both had to work summers, to be able to do things.

In the physics department, the teaching was done by the youngsters who were working for their degree. While Gale and McMillen and those guys were in the department, we never saw them.

Q: We're talking about the generators of the first nuclear...

DR. JOHNSON: No, this was not nuclear. Gale and McMillen were the ones who first made the speed of light...

Q: Oh, yes.

DR. JOHNSON: That was rough. And others also, who came from a much better background than myself, had a lot of trouble with physics, because they talked over our heads. ...much... we got it out of the way.

Chemistry was the major one, and I took my major in chemistry. I had plenty of trouble with chemistry, to begin with, because I'd never had any before. Finally, when you got to the last stage of chemistry, you got to physical chemistry, you know... and all the rest of it. All of a sudden I began to see the light, and all of a sudden I got an "A" in physical chemistry. And everybody couldn't figure out how come this guy who was just barely getting "C's" all of a sudden got an "A."

What it amounted to was something I have observed many times before. In teenage individuals, and particularly when they're getting a growth spurt (and I was underweight... university), they'll tend to go stupid and don't think they've got anything. And then, all of a sudden, when that growth spurt is over, all of a sudden they begin to open out again. There was some of that.

... the first year, and then dropped that, because I just couldn't keep up with it, and shifted over to gymnastics. And so I was in gymnastics for the rest of the four years there.

Now one of the things that made a big difference to me was a history course by a man by the name of Hutchinson, who had been a Marine originally. He was an excellent teacher. It was a three-quarter course, a whole year, and I couldn't take the whole year. But I did take the middle year, which was the Lincoln period, because I'd been a Lincoln buff all my life. In that course, you had stacks of books you were supposed to read every week. And I couldn't possibly keep up with that, because I was carrying lab courses. You were supposed to carry one lab course per quarter, and I carried three... quarters. So it was pretty rough going. That history thing meant a great deal to me.

Now the next thing that meant a great deal to me, and was going on at the same time, was a course in physiology unlike any course that had ever been done before. Ralph Gerrard, who was a neurophysiologist, decided he was going to teach it different from what had ever been done. There were perhaps half a dozen, maybe ten, in the course, which ran through the three quarters for the whole year. And when we got together, he said, "I don't want you to go to the library. I don't want you to read. I want you to just poke around for yourself. And we'll go to the lab and do whatever to get your attention. After all, what's in the books was not in the books originally. How did the guys that put it in the books get that way? I want you to get a feel for that." So we did that.

I helped someone draw off some blood from a rabbit, and I got curious about the blood and spent a lot of time, not really effectively, but time trying to figure out something about coagulation without reading anything. It's pretty rough to do that. Now some of the others cheated and read; I found that out later. But that's neither here nor there. In order to meet with us once in a while, he would meet in a lecture room and say, "What's life?"

What do you mean, "What's life"? We weren't supposed to read, so he'd come up with various kinds of ideas that you could move around... Everything he did, he'd flick his match and make the damn flame do what we said was only true for light. And this was extremely frustrating, but extremely...what shall I say?

Q: Challenging?

DR. JOHNSON: Challenging. It made you think, but you'd never done that before. So that went on, and I don't think I accomplished anything. But he told me, a number of years later, when I came back for a visit after medical school, I think every one of us got involved somehow or other in some kind of investigative work, and told me that I had...I'm not sure about the details, that I had isolated post... or something like that, that had not even been isolated then... He didn't want...

...after one of those... I was walking from the university, through Washington Park, which is a long walk to get the elevated to go home about two in the morning, and all of a sudden, I began to think maybe I have a brain... I had read a passage of Lincoln's... something other about how to think and so forth. And all of a sudden, I

realized that I discovered this by myself, so maybe I... Well, that's a bit about undergraduate.

Now for getting into medical school, you had to have a degree... language exam in German. Well, the German course that I took in the first year was rough. The teacher that taught it was good, but he said, "I can't teach you German until you know German." Of course, this didn't mean anything to me except what the hell are you talking about? Eventually I began to find out, many years later, what he meant. So I took the course, and I wasn't particularly good at it. And then, when it came to the fourth year and I had to get the exam, I began to look stuff over, and the exam was run in such a way that you were to give a not precise but a free translation of the stuff that they gave you to read. And much to my astonishment--total astonishment--I came in number two on that; I was afraid I'd flunked the thing.

Then the other thing was that you weren't so sure you'd get into medical school. This was Depression years, and a lot of guys had to drop out. If it hadn't been Depression, I might not have made it, I don't know. But, anyhow, if I didn't get into medical school, my alternative was forestry, having read Gifford Pinchot's work and so forth and so on. So that gets me into medical school.

This was a very, very unusual time. Rush Medical School was *the* medical school of the University of Chicago when we came there. But they started a new one on the south side, which was designed to raise doctors who would do investigations rather than just treatment. Well, most of us didn't think that was what we came to medical school for. I was enough confused about it so that I went over in the summer and took courses at Rush to find out what it was about, and found out, well, I guess this is what I really want.

Most important, I have to tell you, that ... Hectone, who was one of the great pathologists of his time, gave me books to read. He wouldn't give me advice. And the question was: Did I get the point out of the book? One of which was the fact that there was a great deal of investigation going on about how to teach in medical school, and maybe the old ways of teaching weren't the best, and there were other ways of doing it--in fact doing it, instead of getting lectures thrown at you all the time. The reason I knew him is that my mother, before she was married, had worked as something that might be called a governess, for Dr. Henry Lewis, who was an obstetrician and gynecologist and a close friend of Hectone. And when they went vacationing, she went up there, and she knew them. And so I was introduced to Dr. Lewis and Dr. Hectone, on rare occasions, when my mother wanted to know what about this guy; what's good for him? And that's how Hectone came to give me advice.

So, after reading what he'd given me, I decided to stay on the south side. But about ninety million, which Rosenwald had given, all of a sudden was worth only three million. And things were really tough in terms of plans. There had been a plan whereby Northwestern would be undergraduate, and the University of Chicago would be graduate, and they were fused. That died. There was another plan whereby Rush would be postgraduate medicine, and that Southside would be graduate medicine. And that died. But all these things were stirred up and kept us thinking about what was going on.

Now the school was unusual in that the men in the school were all very outstanding. Bensley, who made possible the isolation of insulin, was head of the anatomy department. He'd had his leg shot off when he was a youngster in Canada, and, in the hospital for a long period of time, he got hold of a microscope and would yell out the door to kids who were running by, and give them a nickel to bring something. And when he got through, he knew more about histology than was in the books. And he developed and became head of the department there. He isolated mitochondria before anybody had ever seen them under the microscope, and began to work on some of their properties, and so forth and so on.

Another one was Bartlemitz, who was one of the big embryologic investigators.

The one who probably had the most influence on me was C. J. Herrick. Herrick's brother was an outstanding neuroanatomist. When he died, John inherited and went on from there. Herrick had been here in World War I.

Q: At the AFIP.

DR. JOHNSON: Yes. It was the Army Medical Museum at that time. And he had no particular background, but the man who was head of the Museum at that time was a very, very astute and wise guy, half Indian, and he said that you're the neuropathologist, just like that. And Herrick went on. And, of course, World War I was rather a short time ago. And you drew some kind of an idea how he made such an impression... the first time he came in, he came in and washed the chalk marks off the board. We thought he was a janitor. He turned around and started to talk, in a slightly high-pitched voice, and he'd talk maybe five or ten minutes about this, then he'd talk about something else entirely for another ten or fifteen minutes, then something else completely different for a while, then in the last five or ten minutes, all of these things came together and just blew you right out of your chair with a vision that you never would get if it wasn't... That was a very important part of it.

Another very important part was physiology in medical school, and that was A. J. Carlson, who was born in Sweden and was a famous guy who was anything but tied down to what people thought that should be. He would go to parks and listen to the guys that would harangue about the world's no good and we've got to do this and that. So he was in direct touch with ordinary, everyday people who were having tough times, which, of course, most people in a university aren't. His way of teaching was not so much to tell you what it was all about, but he'd ask you what you thought, and then came that most devastating question of all: "Vat isht de effidence...?" (What's the evidence of what you said?) Oooh. And that was another very powerful, striking feature.

Those were the things that led up to clinical work, and the clinical work... start with pathology. And in pathology, we had H. G. Wells, who had written the first textbook on clinical pathology quite some time before. We had Cannon, who was one of the leaders at that time in immunologic pathology. And we had a woman there by the name of Eleanor Humphries. I should have told you that we also one in anatomy, and that was Sylvia Benson. Both of them influenced me quite a bit. Neither one of them

shone internally or externally, but both of them had the ability to keep urging you, keep asking questions, get you more excited about things, so that you saw things that other people didn't see. That was the path. department.

In the medical department, originally, Wilder didn't stay very long after they started the south side, and then he went up to the Mayo Clinic and then went to NIH. Then McLean became the one teaching... And then they brought George... He was in charge when I came in. Lewis Lighter, who was one of the really great renal disease physiologists, was in the.... ... was an excellent man when it came to chest diseases. And so forth... medicine.

And in surgery, we had something really outstanding. We had ... Femester, who had come from Rush. He'd gotten a very excellent practice out in one of those suburbs, but was... Femester was someone that we all idolized because of the kind of person that he was. He'd be going on rounds, for instance, and the student would present the case, and there would be the residents and assistant residents and so forth, and the student would make a mistake and say the local LMD, which he'd kick his resident. And the resident knew that he was supposed to do something and that he had to go out. And so he'd take us all out, then he'd take us around the hospital and show all kinds of patients who had problems that arose in the hospital. And since there was no income going and you could get consultations without paying any money, he pointed out, "Now that patient you said that the local LMD sent in, you've got to remember he couldn't do all these things that we could do. If he wanted to get a consultation, it meant it was more money for the patient. Furthermore, we're seeing the patient a lot later, and we're seeing things that we didn't see before." So he taught us to have respect for the physicians on the outside who had to work under a difference circumstance. That was one aspect.

The second aspect was that when he lectured on cases and talked about cases, he'd present the case, and we didn't know it, but many times they were not his cases, and then he'd emphasize all the things that went wrong, and take the blame for it himself even though it was not a case of his. So he was constantly hammering and hammering and hammering that things aren't so nice and easy as you think they are. You've got to be prepared for the fact that maybe you made a mistake, and what are you going to do about it, and so forth. This was a really great man.

Drag Stet was a great physiologist, and there were a number of others. And, of course, Percival Bailey, who had worked with Cushing in Boston, he was the neurosurgeon and was someone who made a great deal of effort.

So those are the teachers.

Now there's something else that is important. We had more pathology in medical school than any school in the country. We had pathology all through our third year and our fourth year, and we had extra pathology. And when we were doing surgery, we had to go up and see all the specimens. We had to describe them and tuck them in, and we had to look at the sections and see what we thought about it before the official decision was made. When we were working in pathology earlier, a number of us took a special course where we went out to the ... House and did autopsies. We had to cut them in; we had to cut our own sections and so forth. So we had much more pathology than most

schools had. And this left me with a good understanding of how structure, when it's analyzed properly, can give you insight and understanding to what's going on.

Then the question was what to do next? Should I go to..., which was the great place where everybody would go. Again, Hectone didn't tell me what to do; he gave me something to read. And I got through reading it and I began to realize that, as he implied, it was better to stay where I was, because this was an unusual medical education, and get the actual experience of working with the patients, and responsibility (the schools, you worked with them, but you didn't have responsibility), before you go somewhere else. Okay, fine, so I applied for an internship in medicine. And I didn't get it. And the man who was in charge of the applications and deciding who were going to be interns wanted to know how come I didn't apply. Didn't apply? I applied.

I forgot to tell you that, between my third and fourth year, I was called back, in the summer, and asked to take over the internship of the chief of medicine, whose intern had gotten sick. So I had that summer and the fall, I had six months being an intern before I ever finished medical school.

But I was outspoken when I thought things were wrong. And so, being outspoken, my application for internship in medicine didn't ever get to the committee. I didn't know until a few years ago how it happened.

But what did happen was that I was then offered an internship, without applying for it, in surgery...

I had thought that getting the medical one and then going elsewhere for surgery was the smart thing. It turns out that what happened was much better than what I wanted, because surgery was superb in the Midwest, and medicine was superb in the East. So after I had my surgical training, I went east and got on the Harvard service at Boston City Hospital and realized that the medicine was ahead of... respect. So it wound up that I was better off having had that.

I had two periods, also, in which I did research. One was the six months that I had, because I didn't get into medicine and had to wait until the surgery one started, under Huggins, who won the Nobel Prize and was a liar and a crook, and I really learned how much things that look wonderful on the outside may be just plain crap underneath. He got mad one day because he was getting a cat, and the cat scratched him and bothered him all night. He came in... kind of stuff... I'm going to... but, for your interest, he came in and got all the cats, turned them loose, and went after them with an ax. That's the kind of guy he was.

Then... I got a letter...

Q: Basically, you had been looking at... Pennsylvania, and then the ... came along.

DR. JOHNSON: ... the war came along...

Q: Was he the director at that point?

DR. JOHNSON: ...

Helwig was picked out, was obviously outstanding, to be the chief pathologist for the Pacific, working under DeCoursey, who would be the administrator. Tracy Mallory, from Massachusetts General Hospital, Harvard, was the key pathologist for the European world. Then a few others of us were given other special jobs. I was given the special job of going to the Fifth Service Command, where we had ten or fifteen hospitals in addition to Billings General Hospital, where I was to do the pathology for all of those, and send the selected material in here.

Now Ash did something very unusual. He set up little AFIPs, little ones, scattered around the world, which would filter out the important things to come back to Washington and not have the place loaded with everything. And I was selected for this one in Indianapolis. And, as I said, Hal... Europe and Asia. And got a great deal out of it.

Then, after two years there, they were all of a sudden having to open up a lot of new hospitals, to go overseas to Europe. And all of a sudden, I was assigned to go to Camp Alice, which was a staging area. And I was out learning how to shoot a gun, which I never had anything to do with before, when a Jeep came roaring up and grabbed me and took me and said, "You're not going overseas," which turned out, again, to be a break, because the general hospital I would have been with never got much to do. And I was sent to the Mayo General Hospital, at Galesburg, Illinois, which had essentially a university staff, but ... university to come in to uniform there. And there we did a lot of excellent work. In both places they did a lot of work.

And then, at the end of the war...

Q: That would be 1945.

DR. JOHNSON: Actually '46. I got asked to come back here as a civilian. For the first time, they were going to have civilian staff. I was asked to come back, and I was very honored about that. But I had gotten married at the prime part of the war, and I had four and a half months leave that I'd never used, and so I took the time with my wife to make a long trip, and finally came here and became part of the civilian staff. So from '42 to '92, that's fifty years that I've been tied to this place.

Q: What was the impression that you had of the way Colonel Ash ran things and of the man? Obviously, you were a relatively junior person at the AFIP.

DR. JOHNSON: ...

...generated a very strong sense of filial responsibility to him. Everybody that worked for him thought very highly of him. He was kind, thoughtful. He could nurse a drink through a long period of time and look like he was getting drunk, but he wasn't; he knew what was going on, for the most part. But that was the big thing to do in those days. When I first came here, my uniform was put together wrong, and when I came and reported to the adjutant, he told me, "Jesus, God, what's the matter?" I had to take off a lot of unnecessary stuff that was on there. And then he took me out here to Walter Reed to find a place to stay, and he said, "Why don't you walk around, I've got to tend to some

business." As I was walking around, all of a sudden reveille came along, which I didn't know what to do, so I had to hide in the bushes until that was over, because nobody would walk around; I couldn't find anybody to imitate.

The reason that I'm telling you this is that the most exciting thing was, in those days, the Army Medical Museum was a part of the surgeon general's office, and so you had times in which you were the surgeon general of the United States Army, at night on duty, and you had to be in the surgeon general's office. Of course, they had all kinds of recorders and things... and you couldn't do anything wrong. But I'm the only one here who's ever been the surgeon general of the United States Army.

Q: You came here to work in 1946, wasn't it?

DR. JOHNSON: I was working with it all the time.

Q: But when you were back here in '46, what was the working environment, as you saw it?

DR. JOHNSON: The working environment consisted of attitudes, primarily, and materials, which were really excellent, I have no complaints. I'm sure everybody doesn't agree, but...

Q: How were you getting cases in those days?

DR. JOHNSON: After a bit of reorganization (you're bringing things to mind that I hadn't thought about), the cases came in and were sent to the individual who was specializing in that area. Then you indicated what you thought at the beginning. We would have a conference down in the basement, with a very big fancy microscope which had about ten or twelve eyelets on it. Colonel Ash would be there, and you put your thing under this scope and then everybody could see, at the same time, what you were seeing. And Colonel Ash, I guess he could move it too and decide what you all agreed on. And for a while this business about a general agreement played a role, and eventually that faded out and we were responsible ourselves.

But I have to tell you something, in between, about that. Ash ran the department out of his hip pocket, without paying attention to a lot of fancy regulations and so forth. When he was over in the Philippines, he told me that he had to be the officer in charge of the enlisted men, and he didn't know what the hell to do, so he'd sort of mumble gruffly, and his first sergeant would give the orders, and the guys would do it. So he looked like he was doing fine.

He also had an unfortunate marriage, and his son was killed horseback riding here, and then his daughter turned against him. He had a pretty rough time.

He was a superb organist, an excellent pianist, a well-educated guy. He had been born in Philadelphia. His father was an outstanding newspaper editor. His mother was born in a log cabin in Davenport, Iowa, at the time that that was just getting settled. I

think she was the first white child born there. And there was quite an age disparity. There were several other children before him. And... wasn't so sure he was going to live, and so it was rather tough on him. His brother, who was an engineer, and another brother, who was a teacher... professor there at..., they helped get him started. He was outstanding, excellent, outstanding. He was a Quaker, a Quaker who abrogated his Quaker anti-war message. He and Carol were born the same year. It was close to the Civil War, and so he was impressed with a lot of things that you learned from the Civil War.

Q: In 1946, he left and Brigadier General Raymond Dart came in. I'm told this was a real watershed. Ash and Dart were two quite different personalities, weren't they?

DR. JOHNSON: Yeah, yeah.

Q: How were you seeing that, again, from your perspective?

DR. JOHNSON: Well, first I have to tell you that Ash didn't leave. They created the registries, and he became the head of the registries, while Dart became the head of the Institute. That's number one. Number two, Dart went very much more by regulations, and following the regulations very closely, contrary to what Ash did. As a matter of fact, I was not too conscious of this because I was busy with my own work, and vaguely began to realize it, and then found out, but there was difficulty because Ash hadn't had all of his things in nice pieces of paper the way Dart thought it should be. This was part of the difficulty. And then came a very painful occasion.

Well, I have to go back. Dart was the administrator, but didn't have much to do with the reports that went out. Hans Smetana, who had come from Colombia, was the guy who was in charge, and all the reports went through him. He would initial them, and if he initialed them upright, that meant he agreed; if he initialed it upside down, it meant he didn't agree. But he didn't tell you, unless something was very badly off; he left you free to do it your own way.

Then Dart began to pick on Ash, and Ash began to indicate he wasn't very happy about this. And we had a meeting in which Smetana and a few of us on his staff were to be with Colonel Ash. And always... Loyalty was absolutely the most important thing in the world.

For instance, Vernier ran into trouble with this, and his way of solving it (I didn't know about this; he told me later) was that when Dart wanted to have his loyalty to him, and forget about Ash, he said, "I can't do this. I will be loyal to all the things you want, but I'm not going to turn against Ash." And he refused to do it.

So that gives you a little background. And at this meeting, the question came up why Ash did this or why Ash did that. And as I listened, being very immature and not much aware of some of this, I asked him point blank why he did what he did at the registry (and I've forgotten what it was). And that disturbed him a great deal.

Q: You asked Dart.

DR. JOHNSON: No, no, Ash. Dart wasn't there. Ash. And later on, he asked me why I did this, because it appeared to be turning against him, see, this sorority thing that I wasn't aware of. And when I was in his home or when I came, he asked me about it. I explained it, and it was perfectly okay once he understood what I was doing. I wasn't turning against him, but I was trying to figure out what the heck was going on.

Q: Did you have the feeling at the time that there were two different regimes going at the same time or something?

DR. JOHNSON: Sort of.

Q: One was more...

DR. JOHNSON: One was the boss.

Q: Sort of the boss and fairly free ranging. The other was more organized. This is a very typical thing that happens.

DR. JOHNSON: No, I would reverse it. The boss was the guy who was very well organized, and the free-ranging one bothered him.

Dart did a lot of things. On one occasion, he told Mrs. Ash, "I'm going to have to hurt Colonel Ash." Because of something or other. He was going to make him retire and not be around here any more. That was it. And that upset her, as I learned later on, and upset Ash as well. But Ash took it in stride and became... hospital up in Bethesda, and continued active there for a long period of time.

I might make an interim remark. I've been interested to see how guys who've been director here do when they become civilians. Because when they're directors here, it looks like they're big stuff, and it would be nice to have them do this or that and something else. And I would say that, by and large, none of them could make it. When they don't have a uniform to protect themselves, it's not so good. If you've got a uniform on, then the guy has to do what you say whether it's right or wrong. But when you haven't got a uniform on, by God, you've got to measure up with having the stuff to do it.

So, by and large, let's see now...

Q: We're looking now at the listing of those who...

DR. JOHNSON: This would apply to Dart and to DeCoursey and Smith; to some extent to Blumberg; to some extent to Silliphant. In other words, all of them had trouble when they became civilians. Ash did a very good job. Townsend did a pretty good job as well. But some of them were in a lot of trouble; they just couldn't be out of uniform and really direct things with their head instead of with their uniform.

Q: You came here to set up quarters in 1946. You'd already had this... What were you doing? Were people both doing very specified jobs, areas, or were... You mentioned these groups getting together and looking, so I take it that...

DR. JOHNSON: This was daily, at the very beginning, in order to decide what was the appropriate diagnosis to send out. So to somebody who didn't have experience in the field, we'll just send out a ... diagnosis. That continued for a period of time, and then it got to the point where we would have meetings once and maybe a review twice a week, in which you would present your case on the screen, and people would comment about it and so forth and so on. And it was quite some time before it got to the point where there was no cross check. All of us were primarily seeing a lot of material coming in...

Q: You weren't just in one area, osteopathic.

DR. JOHNSON: Not at the beginning, but within a fairly short time, each one of us came in a special area. Helwig had skin and GI; and... had central nervous system, and so forth and so on.

Q: And you had...

DR. JOHNSON: I had skeleton and inflammatory vascular, at first.

Q: This was the postwar era, and we just had something like 13,000 men and women in the military forces, many of them getting out and all. Were you seeing a lot of cases related to the military as opposed to the civilian at that time?

DR. JOHNSON: At the beginning, it was primarily military. Primarily military.

Now I have to give you another piece of background information. This place got started because when the Civil War came along, the only training in surgery you got was by operating on war wounds. There was no training otherwise then. And it was very quickly apparent to both the surgeon general for the North and for the South, both of them, that you had to do something about this. Now Hammond got into trouble soon, so he got court-martialed out, after a while, and many things he wanted to get involved only this place. Whereas his running mate, whose name I've forgotten right now myself, got most of the things he felt were needed, and did a very good job. Then, in order to teach the surgeons what they should do and not do, the idea was to accumulate material and then teach it to them as to how it should be done. Well, it was more accumulating than teaching going on, because Stanton wouldn't let them get a school going and he wouldn't let them get a pharmacy going for the new...

Q: Stanton was the secretary of war.

DR. JOHNSON: Yes. As I said, the idea was to know what was wise to do. In other words, just going through the Crimean War, both of them knew that the Crimean War indicated a lot of things didn't work the way they should.

I should add one other thing, that Hammond had done excellent research work while he was in the service, then he went out of the service and became head of anatomy and surgery at the University of Maryland. So when the war came along, the surgeon general was half drunk most of the time, and the Sanitary Commission put the pressure on to get him out, and brought him in to become the surgeon general, over the heads of a lot of other guys, because once he'd gone out, he was down at the bottom again. And a very fortunate thing that they did.

Hammond used to have the stuff brought here so it could be prepared in order to teach. And some odds and ends, a great deal of regular stuff was done in chicken houses, and you know... and throwing them out. Then he sent men out to get the stuff. In fact, he asked the surgeons to send it in. Well, most of them wouldn't; the few that they liked, they'd keep themselves. So he had to send men from here out to get it and dig them up and bring them in here.

Q: You're talking about specimens.

DR. JOHNSON: At times, you'd have a big barrelful of specimens, and it was preserved by adding contraband liquor. The only trouble was, the enlisted men would tap the liquor. So it had a bouquet like no wine you've ever seen, and it would arrive with not much... These were some of the kinds of things that would happen.

They put the stuff together and got the medical and surgical history of the Civil War, which led doctors all over the world to realize something brand new had been achieved, like nothing that had been before, and that from a war, we'd gotten information which we could use in peacetime. Virchow, who was the great god of pathology at the time, spoke very highly of it.

I should tell you something else, that all the pathologists who were here during the Civil War were from Philadelphia and were the product of Samuel Gross, who was a great surgeon and a great pathologist who wrote the first big textbook of pathology in this country, and who was very much respected by Virchow as well.

Q: Virchow was whom?

DR. JOHNSON: Virchow was the great German pathologist whom everybody looks to as the great god of pathology, from Germany.

Q: Now to '46 and shortly thereafter. Essentially, it was still concentrated on the problems of young men and a few young women involved in the war, wasn't it?

DR. JOHNSON: Well, there's an in-between thing that I must tell you about, however. After that was done, then the microscope began to be important. The microscope had

been somewhat during the war. Toward the end of it, they'd seen germs. They'd studied hospital gangrene, which was a horrible thing and so forth. Then comes the discovery of germs. And the Pathology Museum almost died, because all the effort goes into germs, all the effort goes into finding out what's causing Yellow Fever.

And just by the way, Walter Reed didn't believe the business about the mosquitoes at all. And the only reason it was worked out is because of the guy that came from Hopkins, who knew something about it, and made himself injected and died from it. And all of a sudden, Walter Reed got recognized, and then he gets all the credit for it. But he didn't believe it to begin with. And he was here in Washington... worked out well. But that's the military way.

Then, after that, comes World War I. And with World War I, stuff is shipped here from Europe and we ran into brand new things: we had shrapnel; we had gas gangrene and so forth. And Callender, who had come into the service about 1911 or something like that, saw that something had to be done with this, and he tried to work this out. And in addition, recognized we had to know how the hell missiles worked. So down in the basement in the old building he got a firing range, and he got animals in there, and did the first work demonstrating how missiles produced damage in... That all began down there.

Then, once that was underway, and getting microscopy underway, he said, "I want all of the pathology in various hospitals, after the end of the war, to send their occasional tumors and all interesting cases here." Well, it was the same thing as it was in the Civil War--nobody wanted to do it, and were very reluctant to do it, because they were afraid that if they sent it in and the guys here wouldn't agree, then they'd look like jerks, and they couldn't stand it. But that took a long period of time before that got going, from about 1920, oh, perhaps through 1930, before it got going very well.

Meantime, between that time and World War II, the place was good only because of Callender and Ash. There were other guys here, but either one or the other was at Walter Reed and looking over the shoulders and keeping things going well downtown.

Then Ash begins to notice something. (I should give you a background.) Pathology in general is done by professors in the medical schools who do the autopsies and tell you what's happened... tell you that you shouldn't do this and shouldn't do that. They never bothered to look at surgical pathology. As a matter of fact, surgical pathology was not allowed in the Path. Department at Hopkins when World War II got underway. That's how bad it was. And the surgeons had to do their own stuff.

Now Ash recognized that this was not good, and so he started the business of surgical pathology in collecting material. Actually, it was begun with Codman up at Boston, who collected bone tumors. And he had a little room about this big...

Q: We're talking about a very small room.

DR. JOHNSON: ...pocket and so forth. Started the first registry, the Registry of Bone Tumors, which I have all of that material in here. And Ash began to realize that this was essential not just for getting an archive, but for treating patients. So he became an expert in several special surgical fields and was recognized as one of the outstanding surgical

pathologists, as a pathologist, not as a surgeon. So this was the next thing that came along.

And so, when World War II came along, all of these principals had been built up which could now be utilized, and became important when he set up subsidiary centers, like the one I was at, where you could get material and send it in and learn more about what went on when they got blast injuries underwater and all kinds of stuff like that. So that part is essential, to know what happened when '46 comes along.

Then '46 comes along. A lot of stuff had been coming in during the war. And a great many places now where they'd had material were closing down, and they sent all their stuff here to be either thrown out or made into the records. There was a lot of stuff then coming in here, military stuff primarily, for quite a while.

And since the registries had been started, the next one that got a lot of attention was the Eye, which Ash played a role in, and Callender had, too. Since the registries had been started, they began to send in stuff from civilian sources to the registries. And the Eye was the main one, to begin with, as far as this place was concerned. And then gradually it began to enlarge, and again, Ash, as the head of the Registry but no longer head of the Institute, encouraged that. So all of that unfolded.

Q: Well, now, were you involved? Was there a Bone Registry in '46, by that time, or not?

DR. JOHNSON: No, no. See, the Codman Bone Sarcoma Registry was in Boston.

I'll have to tell you something else now. I had had a lot of training in skeleton pathology with Femester. And Femester was an unusual man who had worked with Erdheim in Vienna.

Now Erdheim was a tall guy with a high-pitched voice, who was Jewish and therefore wasn't any good as far as Vienna was concerned, and was relegated to taking care of the alms house, where he had to do his own autopsies, write his own protocols, cut his own sections, and do everything himself. He had no technicians and no secretary or anything. And he did them very thoroughly. And he was wont to say, when people wanted to do dozens of cases, "Do one case thoroughly, and to hell with having fifty-five cases. You'll learn more from one done completely than you will from fifty-five done casually." And, of course, this was in response to Vienna, where they did hundreds and hundreds of autopsies. He'd been squeezed out, and so he was making this now.

Femester worked with him, and Femester learned how to make those sections. He made big sections, like these that you see here.

Q: You're showing me something which is about six inches or more.

DR. JOHNSON: Well, that was all started by Femester in this country, and I learned it with Femester. So when I came here and the Bone was shifted over to me, I wanted to do that. And everybody thought there was really no excuse for doing that. Just an ordinary little piece of tissue was adequate and you don't need that. And I finally was able to get it

through and, furthermore, shifted it from as it was originally done--which was in celloidin, which would take many months before you'd get your section, because it was so slow--shifted it to a very rapid method of paraffin. And I could take a piece of spine, that long, and you hand it to me on Monday, I could hand you the section on Friday. Well, that was something nobody had ever seen before. And much of what I have done that has changed things has been concerned with the fact that you've got to understand things, you've got to have a context. You can get a context from this, but you can't get a context from a little piece.

Now to underscore that even more, we've just recently had letters coming in on somebody who wants to look at some of the original Bone Sarcoma Registry cases, in which they had little tiny fragments of material, and they were called this or called that. Well, I went over some of that quite a while ago and found out that many of them were not sarcomas as they were thought to be. They had an x-ray and a little fragment of tissue, and the fragment had become so perforated that only a reaction it isn't a tumor, and it's not a very good one.

We're going back to that now, back to that business of getting little needle biopsies and making decisions which are going to lead to a lot of trouble in the future. It's already underway. So, again, we start with little bits; we're going back to little bits. And I played a major role in taking the ordinary one, the usual thing, when I started, was to have a piece of tissue about that big.

Q: Smaller than a postage stamp.

DR. JOHNSON: And then I insisted on making these big ones. So I developed that whole system when I came here.

Q: The legs of the AFIP, which at that time was the Army Museum, were teaching, research, and consultation. Where did the Museum fit in, in the forties and early fifties?

DR. JOHNSON: Most people ignored it, I'm sorry to say. I didn't, so I became sort of a pain in the neck down there, because I kept insisting you can't understand this if you don't understand the whole thing. And in these several large papers I've written about reorganizing this place, I've indicated that the Museum is where you've got the gross anatomy, and you've got to have the gross anatomy in order to understand the microbe. And that the EM work that's being done, we don't save it. The guys can walk... themselves. We should take the EM stuff...

Q: EM is what?

DR. JOHNSON: Electron microscopy. We should have the gross; we should have the usual microbe, which is this; and we should have the EM. And nobody's paid much attention to it. So things are not really as tightly organized as they should be.

Furthermore, we had several people hired in the Museum who didn't give a damn

about it, but about ten years ago, we had a guy who was just incapable--he couldn't pass his Boards--incapable and threw away a lot of valuable stuff that he didn't understand. And we're still having trouble with things that are missing we should be able to get.

Q: You looked upon the Museum as being essentially a repository for research, for furthering your knowledge.

DR. JOHNSON: Research didn't count for much then. See, pathology was primarily concerned in turning out diagnoses. Period. The only kind of research most people did: well, let's get fifty cases of this disease and see how those fifty patients got along, whether they were cured or whether they got worse. That is clinical research; that's not real research.

Q: What was the attitude when you came here, in the forties, towards the teaching, the research, and...

DR. JOHNSON: We didn't talk that language.

Q: It was consultation, was it?

DR. JOHNSON: That's right. That's right. And making diagnoses. That's all.

Let me make that clear. The criteria on which you make a diagnosis are concerned with what's wrong with the patient. And it's presumed, once you know what's wrong the patient, you know what to do. Unfortunately, that's not necessarily true.

I'd emphasize something else entirely, in addition to that. The important thing is not the diagnosis, but how you got to it. The essence of that is that I've got this diagnostic thing. And contrary to the way all the teaching was going on, you don't get that from one way. You can get that from a dozen different ways. And what you want to know is which pathway led to this. And that means a lot of work that has nothing to do with diagnostic criteria. It means getting a lot of context work that will tell you something about the background and what was going on before this developed and how it developed.

And now you know that this came from that pathway, not this. And that makes all the difference in therapy. Now this is a change, a big change in attitude.

Q: Again, back in the days of the brick Museum when you were here, how did the major medical universities treat the AFIP?

DR. JOHNSON: They didn't think much of it. And then another thing that Ash did was to bring in the heads of the departments of maybe a dozen major medical schools, for about a week or two weeks here, and by and large, they were astonished, totally astonished at what they ran into. Because this business about figuring out the relationships and things was part of the stuff that Ash talked about. This was in the early part of World War II, not after the war. And they'd come in and find that youngsters

which ran a department knew more about diseases that they had written about than they did. It was a stunning thing.

The other thing was that they got all excited about seeing gross specimens, because they didn't do gross anymore, they threw them all away. And the result was a fair number of gross specimens were taken to their schools to use for teaching. Well, they wound up in closets and most of them got destroyed. Add to that now, the same thing was done for England. In England, the great Hunter Museum was very badly damaged, and Ash gave a lot of gross material to them, after World War II, to get restarted again. So this is the kind of thing that was going on with gross specimens, more to have them to teach, because all of a sudden these professors found out that, by God, what I see under the scope makes more sense when I've got the whole specimen.

Q: After Dart and DeCoursey and Silliphant, was there a change in emphasis from the consultation business?

DR. JOHNSON: The change in emphasis came mostly from the civilians, not from the leaders. It came from Smetana, who had a superb research background, as it had come originally from Ash. Aside from Ash and Callender, there's not a single one there who was trained in and skilled at research. Not a single one.

Q: We're looking at a list of the former directors. Did you find your role was changing as time went on? Gosh, we're talking about fifty years now.

DR. JOHNSON: Not really. Not really. Because I just kept quiet and did things that I thought should be done quietly, got them done and then didn't get involved in big arguments, except when things would come along where something was all screwed up. I'll give you an example of an all-screwed-up thing. Hansen...

Q: This was Colonel Hansen.

DR. JOHNSON: ...received a document from one of his men that indicated that we were wasting a lot of money and not doing things properly. It came to my attention, and I went and got the information for him that showed how the money was being spent, and he wasn't a damn fool, wasn't... In other words, when you're a director, if you don't keep your fingers on everything yourself, the guys under you can make themselves look good and screw the director.

It was on the occasion of that, that I wrote one of the documents that I wrote about what had to be done in the Institute. I don't think there are more than two... documents down in the Historical Section, big ones that I've written about the Institute. And each one of them is done differently, because circumstances and demands have changed.

Now the idea of three kinds of activity--research, education, and consultation--gets its major boost from Blumberg. Blumberg decided to get a logo for the AFIP and all that sort of stuff. And when they wanted to get a logo done, he came up with those three

things, which weren't particularly original; dozens of other places had it as well.

Q: Ash is one of the names; Dart to a certain extent; but then Brigadier General Joe Blumberg, as far as activists, how did he impress you?

DR. JOHNSON: Well, I have to do it by contrast. See, the difficulty is that once you've been a director here, there's nothing else for you except to leave the service. So one of the things you try to do is to parlay your job as a director here into something that will give you a job afterward. Blumberg's job afterward was to go around as an inspector of the pathology departments of various hospitals and universities. He was very concerned about the Institute and its amounting to something, but he was also concerned about himself, because he went from here (nobody else did) to running R&D, which was in conflict with the Institute and the roles... between them.

Townsend went to a university and got along fine there. Townsend also did a lot to get adequate incomes for people here, because we were not paid worth a hoot. He did a lot for the Institute.

So Townsend and Blumberg are two guys who have done a lot for the Institute, apart from Ash.

Q: They saw it as an organization rather than just, you might say, the medical side.

DR. JOHNSON: Yes, as an organization, that's right.

Q: Colonel Morrissey of the Air Force was director here from '71 to '73. When he left, he sort of left a blast, saying a number of things. Many of these things are very common, I must say, in any organization. I come from a different organization, but I understand how it is. And one of them was that each department was sort of run like a little dukedom or something. In some, they were really very responsive to requests for consultation; in other ones, tremendous backlogs. How did you find this, from your perspective?

DR. JOHNSON: Let's go back. Before he came here, I had letters from Texas, where he was, saying that this guy doesn't know pathology worth a damn, and he's not worth a damn when it comes to running the department, but we need to have your help (on the stuff that I worked with) without his knowing about it. That's how bad it was before he came here.

When he came here, he thought he was going to get his star.

Q: You're talking about becoming a general.

DR. JOHNSON: When he didn't get his star, he didn't give a damn about the place. That's number two.

Number three, he was not a competent pathologist, and he was not a competent administrator; he didn't know how to run things.

Number four, that report that he turned in was illegal. He turned in something that he was not supposed to turn in, in the way he turned it in. Because of that report, a group of us were brought together to say what should be done about that. And one of the big documents that's down in History is the answer to that report, including the statements that he asked for information, he was given information, he says he never got it, and he was lying. I didn't say lying, but I indicated it.

Q: Your response must have been around 1973, because he left early, and he made his report in '73, so, for somebody looking within the files, it would be around 1973.

DR. JOHNSON: Yes, this was within weeks.

Q: Doctor, there must have been times when you were all together and looking at this, and, as in any institution, particularly when you have conflicting personalities and a certain amount of autonomous effort, as you had in the different divisions, how about response to consultation? Did you see it, looking at this, that some bureaus were a lot faster in getting things out?

DR. JOHNSON: Oh, of necessity, this had to be. For instance, if you get a patient's skin, there isn't much you can do except say what you see and put a label on it. When you've got a bone, you've got to give decalcification, you've got to get x-rays, you've got to do a lot of extra work before you can give an opinion. So, Skin versus Bone is going to have a very striking difference in the rate at which reports go out, in terms of how soon after it gets here. And a fair number of those things that we got from Bone was that they sent us gross specimens. We had to do a lot of work before we could get a report. Well, I said we could make one of these in five days. Most of the time, it takes a hell of a lot longer than that.

Q: We're talking about a six-inch...

DR. JOHNSON: It was the large slide. So there was a substantial difference in the speed that those reports could go out, because of what had to be done versus what didn't have to be done on different types of tissue.

Q: Did you get at all involved in the heart-wrenching work of moving from the brick building to here?

DR. JOHNSON: Oh, yes, I was involved. Fortunately, I had a couple of excellent people working with me who did a wonderful job of getting it all moved in here and straightened out.

Q: This was at the director's level, pretty much, the decision how to go about this and all that.

DR. JOHNSON: Crudely. Crudely. The details. As a matter of fact, a lot of my stuff was brought out in our own private automobiles, so that we could keep track of... But there were no big regulations at all, that you had to go this way or go that way, just get it out of here, at least as far as I..

Q: Did it make any difference to you, as a practical measure for your work, where you were located?

DR. JOHNSON: Yes. It was essential that I be right next to the laboratory, because we had to work with our lab all the time. It was back and forth, back and forth, and so our setup was right with the laboratory, which was across the hall.

Q: You were mentioning you had some very good assistants. How about the people who came in for a relatively short term to work with you, did you see any changes in them? You've been here for quite a long time. The type, the caliber?

DR. JOHNSON: It varied. There wasn't a particular trend at all, like all one way. We had some excellent people, and then we had occasions some that weren't very much good. And the ones that weren't very much good, what you primarily did was to see that they didn't get reports out.

Another thing, while I ran the department, Dr. Sweeten was in OR, when I ran the department, no report went out that I hadn't seen. I wouldn't allow any reports to go out that I hadn't seen, just like Ash did when he was running the place. Therefore, things that would go out in reports that were not adequate, I could prevent.

Q: Up until the mid-Sixties, we had one program or another, like the Berry Program, but essentially doctors had to serve a certain amount of time in the military--any doctor who was getting any support. Did you find this was a good source of getting people?

DR. JOHNSON: The trouble is, I can't look back and say who were Berry Planners and who weren't. In fact, I wasn't particularly concerned with it. We must have had some Berry Planners, but it didn't have a special meaning.

Q: Well, I suppose, behind it, there was a certain impetus. People had to put in their time in the military, and the AFIP was a competitive and sought-after assignment, wasn't it?

DR. JOHNSON: NIH was the sought-after assignment. If you were the professor and you had your special guys, you got them to NIH so that they wouldn't have to leave and go out and fight all through the war. Or, even better yet, you got them in uniform and you kept them where they were. This was the great thing to do.

Q: How did you find the role of your particular specialty as far as the authority that the AFIP had in this, as opposed to universities and NIH and other places?. Did you see a change in whether people were referred to you or referred to some other place?

DR. JOHNSON: First, scant material came here from universities. Universities figured they knew it all and they didn't need to be bothered.

Q: This goes right back to the beginning.

DR. JOHNSON: Yes. And second, as a result of that, most of the material we got came from isolated local hospitals, with things that had happened there that didn't happen in universities, which could now be collected and be the basis for information that you couldn't get at a university. In other words, having stuff come from a lot of little places all over the country gave experiences that you would never get at a university, because they wouldn't allow certain things to happen. So that was one aspect of it. Now what else did you want to know?

Q: I was just wondering, did you see any change in the desire of hospitals to send things to the AFIP? Did they, over time, begin to send things more to research hospitals closer to them?

DR. JOHNSON: Well, I can't answer for what went on outside of here. But NIH would come and ask me what the hell it was all about, and we had to straighten out problems for them. Occasionally, a university would do it. For instance, when Ted Kennedy's son developed his sarcoma, within a half-hour of the time the x-ray was taken, it was in my hands, asking me what to do about it. And I was there at the surgery, doing frozen sections as well. And then I was at his home, deciding about what post-operative therapy should go on. So I would get involved in things like that.

But I would say, by and large, I can characterize what you asked by a quotation. One of the men who was here during the war and then went to be head of a department in a medical school had heard someone say that the experts at the AFIP said thus and so. He said, "What the hell do you mean, experts? Since we left, there aren't any experts." So that epitomizes it.

Q: Did you work on a registry? Is there a registry that comes out of this office?

DR. JOHNSON: A registry is a public relations device, really. That's all. So that you can say there are so many indications of this disease or that disease in our registry. That's all it is.

Q: But I would think (and, again, I'm speaking as a real layman in this thing) that if you had so many specimens of various types of bone cancer or what have you, that just by its very nature and longevity, the AFIP would have so many of these things of maybe a

peculiar type that just wouldn't occur at, say, a teaching hospital or something like that, that this would be information that just isn't available because of the depth of the number of cases.

DR. JOHNSON: Bone is a little bit different from most others, for this reason: the total number of bone sarcomas in the country as a whole is about three thousand a year, and if you divide up all of the bone sarcomas so that each doctor gets one to see, in a whole lifetime of practice, he'll see three. And you're not going to get anywhere that way. Therefore, the Sarcoma Registry was started up in Boston, by Codman, in order to do something about this. And he damned near got run out of town on a rail, and tarred and feathered because he did it, because they just didn't like that sort of stuff.

Q: Why not?

DR. JOHNSON: I'll make a quick interpretation. They weren't so sure of themselves, and if they got an opinion from them that didn't agree, then they would...

Q: It would embarrass them.

DR. JOHNSON: Right.

Q: From your point of view, the basic document, you might say, is at Boston.

DR. JOHNSON: No, it's here now. ...started back in...

Q: Do you publish it?

DR. JOHNSON: You mean the results of all this stuff?

Q: Yes. Or is there something that comes out so that...

DR. JOHNSON: The stuff that comes out is presented in meetings and in discussions and in the teaching aspect, where we have various courses. Written stuff, no. That is planned, but the reason it's no is that everybody... no need to get involved in how you name them. Where I'm concerned really (what I told you about earlier), what are the mechanisms that lead to it? And that I've worked on, that kind of preliminary publication that was presented at the New York Academy of Medicine many years ago. We had another publication, the *Jaffe...*, a few years ago, and we never were able to get our hands on the tumors in here.

Q: We're talking about on the face.

DR. JOHNSON: ... Finally, we got those, and now we're getting to the point where I

am ready to say this is the basis... tumors. This is not... complex organ. And I can get one kind of tumor at one spot; I'll get a different one at another spot; I'll get a different one at another spot. Because the kind of tumor you can get depends upon the kind of normal activities going on in those areas, and those areas are different. Therefore, the tumors, which are diagnosed merely on what you see under the microscope, there is no real understanding of what goes on in neoplasms. And I centered my big efforts on getting this aspect, which requires the large slides and the x-rays.

Q: You've been looking at bones for fifty years or more. Lifestyles, the environment, everything has undergone major changes, with chemicals, the way we do things, everything. Have you seen any particular trends in what basically causes it? Again, I'm talking as a layman asking this question.

DR. JOHNSON: We're not concerned a damned bit about cause, because we don't know. You've heard about chaos and complexity? That all of the things that have been done in science in the past have essentially been done by getting a straight line and putting your data on it. If you have to use a logarithm to make a crooked line straight, that's fine. But now it's apparent that this is a bunch of nonsense, that things don't work that way, and if things get chaotic, they get a pattern here, and then they get... here, change a different pattern. Under those circumstances, the idea of cause has no meaning. Absolutely none. Anything you want to name, you've got a dozen causes. And anything you want to name, it's got a dozen different consequences. And therefore cause as the explanation which should have been taught as... the great goal was to find the cause of things, then you can deal with it. That's not true. That's not true.

Q: Again, I'm way over my head, but it's obvious that smoking's not a good thing to do. This was the cause of a lot of things, which was not really understood. There may be subsidiary things, but if you can come up with a gross cause--knock off the cigarettes and you're going to be better off. Is there anything within your field that you can say, you know, cut out doing high jumps or something like that?

DR. JOHNSON: No. One thing that did come up, and this goes way back to the 1860s and '70s and '80s, is that we didn't have x-rays, and if a patient got tumors, you couldn't treat them very much. And when you did treat them, the most that you could do was to amputate them. And this was a period when a lot of people weren't on good nourishment. It became apparent, when we began to compare stuff, that they didn't die near as quickly of their tumors when we didn't treat them. They die faster now when we treat them.

As a matter of fact, when I was up at the Children's Hospital (this is a paraphrase and therefore is not valid), but if you had a child that fell off a swing, and the mother's got cancerphobia, she's worried the baby's going to get cancer, she runs to the hospital and says, "I want to be sure my baby hasn't got a cancer," so they get her an x-ray, and there's nothing there. But this time, there's something there. So you rush them up to the operating room, and you put on tourniquets, and you take it off. And the baby will die a

hell of a lot faster than if it was a Jehovah Witness and it won't leave the brain child dead.

And this is what nobody paid any attention to. Absolutely none.

Therapy itself can lead to difficulty. So we've got a period in which we had no therapy, then we've got a period in which you've got a lot of therapy, and now we've got something new. We've got them picking up so damned early that a lot of them are getting credit for the therapy that we do so that they live longer, when in fact they're picked so damned early that it isn't the therapy, it's that you got them early.

These things are the kind of things that turned up when we began to investigate the bone tumors, not causes it.

As far as causes is concerned, there's only one, and that is radioactive material... to the bone, radioactive radium or stuff like that. And, of course, that isn't ordinarily run into.

We do, however, since you brought that up... Right now I'm in the midst of a first-class mess, and this is that some pathologists went to Congress quite a number of years ago and said that fluoridating the water is causing 40,000 more cancers per year than we would ever have. This was pure nonsense to begin with, but Congress says to NIH, "You better find out about this." So NIH undertakes a work with rats, and comes to conclusions. Then, down in Cincinnati, another group gets involved, and up at the University of Wisconsin, another group gets involved. And I'd been seeing all this material. And they're all claiming that fluoride will cause cancer. That's absolutely wrong. If it does anything, it'll stop it. Because, again, we've got this business that I told you about, I've got something going on here, and if I can get something that preceded, that causes it. But in point of fact, what you do is lose bone, and then the repair that comes because you lose bone leads to repair, and the repair is thought to be malignant, when in point of fact it isn't. So I have to go around the world. We go to India. We've got places there where the fluoride is so bad in the water, the children, ten years old, have got extensive bone disease. Nobody ever gets bone cancers. Nobody ever gets... But they don't worry about that; the animals are more important than the humans. And I've got a first-class fight on my hands. I went to the National Science Foundation, and they wouldn't pay much attention to this because the animals are the only thing that counts.

Q: I don't want to keep you tied down too long, but you were saying that you wanted to talk about how you think the AFIP could be more responsive.

DR. JOHNSON: That'll have to be a separate business.

Q: Okay. Well, why don't we stop at this point. What do you think?

DR. JOHNSON: Okay. Whatever you want.

Q: Well, no, are there any other areas that I haven't covered that you...

DR. JOHNSON: Oh, dozens.

Q: Dozens? Well, again, I'm fishing, as you understand. We're talking about...

DR. JOHNSON: We've already indicated this guy was totally incompetent.

Q: Colonel Morrissey, yes.

DR. JOHNSON: And at this point, something new got underway.

Q: We're talking about Colonel William Cowan, who came in in 1980 as the director, and then Robert McMeekin...

DR. JOHNSON: Now he was lazy... taken care of, and, you remember, they just sat and sat and sat and sat and sat on his desk. And finally the surgeon general got upset enough about it so he said, "Why don't you come down here to my office once a week."... we got something brand new come in. We got an officer... a man is appointed as medical overseer for the whole Department of Defense. And the current one used to be head of the Walter Reed Hospital here. All right. When that gets underway, he simultaneously gets these guys, neither of whom has any adequate experience.

Q: We're talking about Dr. McMeekin of the Army, and Karnei of the Navy.

DR. JOHNSON: Karnei had never been head of a laboratory when he came here to the AFIP, had no background for it, made all kinds of silly rules. For instance, he promulgated, on three occasions, that we should have to sign in and sign out when we went to the toilet.

And this guy was primarily concerned...

Q: McMeekin.

DR. JOHNSON: ...with being able to stay here not four years but ten or twelve. And so he disobeyed a direct order of the Department of Defense and got thrown out before his time was over.

We've got here a completely different set of circumstances than what goes on at the AFIP. The current one who's in charge now is weak and, as a matter of fact, has always been weak. He was the head of neuropathology for a while.

Q: So your feeling is that really from about 1980 on, the directors, and hence the leadership of the AFIP, have not been very strong.

DR. JOHNSON: That's right, in terms of what we thought was the major mission. See, they got a different idea about the mission. Their idea about the mission was primarily

that it should be for their personal benefit. For instance, when he retired, he had a retirement ceremony like you have when you're the admiral of a fleet. And he repeatedly disobeyed orders of the surgeon general about people that we had here that had to be called in.

Q: What type of orders were these?

DR. JOHNSON: For instance, we had a man... that he decided was no damn good... advance in grade. And the surgeon was... advance him... Finally, the surgeon general... I don't know, but he did get the advance that he was entitled to for the length of time he'd been in service. And he thought he was so good that he didn't have to pay attention to the surgeon general.

Q: It's an interesting thing. Have you seen a change in how the various divisions work?

DR. JOHNSON: We've had some of our best people leave because... don't rock the boat...

Q: Are we talking about doctors?

DR. JOHNSON: Doctors. Now we had one of the really great pathologists, recognized all over the world, given advanced degrees at various universities, in soft tissue. And we had a woman who came to work with him whose father was a general. And she thought, because her father was a general, she could do anything she pleased. And it got to the point where she would go down, of course, because... these guys, Karnei thought that that was very important, and he thought she was the best pathologist in the Institute. Plenty of us have got cases where she didn't quite know what she was talking about. Well, she eventually told Karnei that Enzinger, the great pathologist, was going off his nut. Unbelievable. Then he had to go around and ask other people is that true? Then she thought she was going to be able to do all kinds of things here, quite different from what other people did. And Enzinger just said to hell with it, and he left. She thought she should be the head of the Department of Pathology at Hopkins. They wouldn't have her. She's now head of, I think, surgical pathology, I don't think all pathology, at the University of Michigan. She joins everything and gets to be well known so that... That's her life, her business.

So we've had that kind of problems.

We had another one who was here for a long time, and his wife was here originally, and eventually the wife business got so bad that she had to be eased out, and then the marriage broke up.

On the other hand, we've had some topnotch ones. We've got one in here who is just outstanding--Vietnamese with a French training. And the French training is much more logical than the American training. She's excellent.

Q: Sort of a final question on this, then we can come back again. What about the change over the time as far as technical support? I'm thinking of things like electron microscopes and all. Has that made a big change in how we deal with it?

DR. JOHNSON: The biggest change has been made...let me go back now. I said earlier that the Army Medical Museum just barely survived the big enthusiasm about germs when that came along. And if it hadn't been for a civilian who kept it going, it just would not have had anything to do with pathology. That was Dr. Lamb. We're doing the same thing all over again now with the modern genetic business and molecular biology, much of which has nothing to do with what this Institute is concerned with. But because it's hot stuff, the directors are trying to get people to do this, and tear off places and bring these people in. It's a very serious problem as to whether this place can survive much longer...

Q: Well, why don't we stop at this point.

DR. JOHNSON: Fine.

Q: Today is June 2, 1993. This is the second interview with Dr. Lent Johnson. Dr. Johnson, we talked in sort of general terms last time about your time in the AFIP. You said you wanted to discuss its earlier history and all, so why don't we start from here?

DR. JOHNSON: The history of the AFIP that's been written is very deficient in many respects. As a matter of fact, Colonel Ash, who had a great deal to do with this Institute, was quite disappointed in it.

Q: This was done in 1962, and it was for the hundredth anniversary of this. It was by...

DR. JOHNSON: The history of the Institute is intimately tied up with the history of medicine, and that's intimately tied up with the general history of the country and the world.

And to go back and just get a running start on it, we had twelve hundred so-called doctors during the Revolution, only a hundred of whom were trained. And the death rate ran about sixteen to one in terms of disease versus injury, and yet there were about ten thousand soldiers each year that died.

When we got to the Civil War, we talk a great deal about the North and about Hammond, and everybody forgets to talk about Moore, who was equally important for the South.

Q: Moore was...

DR. JOHNSON: Surgeon general for the South, and Hammond for the North. There were ninety-eight doctors when the war began, and we wound up with twelve thousand of them. Hammond and Moore both realized that drastic changes were necessary.

That had been demonstrated beautifully in the Crimean War, where the death rate was just horrible. And it didn't begin to change until Florence Nightingale got in and began to insist upon some things that the doctors and the commanding officers weren't paying any attention to.

The big change that came with the Civil War as far as America was concerned was the presence of anaesthesia for amputations. Now amputations originally were done very early, then they were done along in the mid-course, and eventually they got to be done quite late. And when they did them late, they could save a good many limbs that they gave up before.

This was all tied up with discovering, and the discovery depended upon some of the material, and the explanation clearly depended on the material, that was accumulated here at the AFIP.

If you had an individual who had a wound and a broken bone with it (and that was the combination that was particularly bad), you delayed amputation until things began to quiet down. Then you found out that you could do a lot better if you put the limb in a splint instead of just letting it lie there. And finally, having got a splint, the next thing you did was to hang it in such a way that you would get dependent drainage of the pus. As you did this, you didn't have near as many amputations, and your amputations could be done later on those that didn't recover adequately. Despite all this, we ran, for the North and South, between forty-six and fifty-six percent people who died with wounds that involved their bones.

The nursing for the Civil War was primarily by families and friends. A corps to get men off the battlefield had to be developed at Bull Run; they were still lying out in the rain a week later. At Antietam, which was the worst one, they were all cleared in twenty-four hours from the battleground.

Now the work done during the Civil War at the AFIP involved particularly getting gross specimens, cleaning them up, seeing the relationships and what had happened, and relating to the story and how long they'd had trouble. Microscopic work was also done, and germs were also investigated. The result was a massive amount of new data, which was applied to civilian medicine as well.

There was no surgery to speak of except in wartime, and in fact the old term for a surgeon is primarily an arrow remover. So a great many people rushed to every battle that was coming on; a great many doctors rushed to get some experience. And rushing for experience, they'd rush off as soon as they got through cutting. And we had to create a reserve corps that if you wanted to come and cut, by God, you stayed at least two weeks to take care of the patients.

All of this brings up, then, what primarily has been the mission of the AFIP, which in turn depends on what the mission of pathology is.

The primary mission of pathology in general is the structure, period, the structure of disease, not the physiofunction, not the chemistry, and not the germs. And the importance of the structure is illustrated by the fact that it's still true with the most fancy study that's going on. Last night, for instance, Watson (of Watson and Crick) talked about genes and genomes. And it's the structure of the genome that determines it, not

the... of the genome. So structure continues to be a key all the way along the line.

Now the mission of the AFIP was to deal with problems that they didn't know why things developed the way they did, why they died quickly, why they got blood poisoning, and then how did it unfold (and that's the difficult part) from normal to abnormal. Abnormal, when it's..., can be recognized. The early stages, you don't know you've got anything wrong. And there's a whole series of transformations that are necessary in between to understand what's going on. Furthermore, when you have something new to deal with, you can get more information from looking at the structure of it per unit of time that you put into it than from any other approach. And this was true during World War II when the Japanese balloons were coming over. We didn't know what they had, but we prepared to find out what would be the structural results of the balloons. Well, none of them gave any, so we didn't have to worry.

Q: I might, for the record, put in that these were fire balloons, basically paper balloons, that were sent to take advantage of the air currents. Excuse me, doctor, for the layman, which I am, could you explain what you mean by structure?

DR. JOHNSON: Structure, in the body, for instance, means, first of all, what kind of a person you are--big, heavy, well muscled; or skinny, elongated; or dwarfed; or a lot of fat; or no fat; or no muscle. That's the first aspect. The second aspect is the dissections, in which you find out that there are various kinds of organs, various kinds of tissues, various kinds of bones, and what their relationships to each other are. The next step is that you've got these organs, and you've got tissues of the same kind in every organ, and that has to be dealt with. Then the next thing is the cells that make up these tissues, and the cells that make up the organs. And all of these are precise things that can be dissected out and identified and dealt with as they change in the presence of disease, in contradistinction to chemistry (Is the blood sugar up or is the blood sugar down?) and physiology (Does the individual have good digestion going on or does he have indigestion?). Those are things that come and go. Structural changes, while they may start, may not progress all the way, they may regress. You have to have a whole series of steps from the normal structure to the abnormal structure, which is represented by disease.

Now the overall result of this whole approach is that you've got a disease which is in effect and, by God, I want to know the cause. So the big thing was to establish the cause of disease, whether it was due to syphilitic germs, or whether it was due to lousy food, or whatever you wanted to talk about was the king.

The trouble is that there is now clearly established no necessary relationship between cause and effect. So we're in trouble on that original viewpoint.

Then the next thing for the pathologist was to find out about these steps. And if you've got the steps clarified, then that was called pathogenesis: How did you generate a disease from the normal?

Well, that doesn't work very well, because pathogenesis does not have a single pathway. For any particular disease you'd want to name, there are dozens of causes and dozens of consequences. There is no such thing as a one-to-one relationship between a

cause and a disease and the consequences of that disease, what the patient's going to suffer from. Therefore, the problem is which particular pathway led to that disease, and which particular outcomes are going to follow from it. These are the things that involve modern pathology in great detail.

Originally, we were concerned only with those things that we saw in the gross or the microscopic that would give us a diagnosis. That involves a small part of what's present on the specimen, either in the gross specimen or in the microscopic section. There are a great many fragments that we pay no attention to. Diagnostic criteria depend upon the fragments that have been labeled as important, and that's primarily a matter of dogma.

In order to understand the mechanism, the pathways of disease, you have to pay attention to a large amount of subtle information that is there in order to understand the disease. And that subtle information is not concerned with the diagnostic criteria only. You have to go at it from the gross standpoint, the microscopic standpoint, from the x-rays of the patient, from the surgical report, and from the complete clinical story of the patient of all the difficulties that the patient has suffered from. And when these are put together, then you begin to understand the disease, and you get a particular pathway that leads to it and a particular series of consequences that are going to result from it.

This is illustrated by the ideal of the autopsies. The autopsies don't amount to much nowadays, and we don't actually experience autopsies very well. The original idea before we got to the present state was that the autopsy should set out to explain the history of the patient, the laboratory findings, the course in the hospital, the cause for death, and account for everything that happened to the patient. This separates the men from the boys, and there aren't very many men.

Today we have most of our diseases going out the front door instead of the back door, via the morgue, so we have a very poor experience with the pathology of most diseases. We concentrate now on high blood pressure and strokes and carcinoma as the training part of what diseases man can be subjected to. And it may seem okay, because we're getting rid of them. But in a few moments, I'll indicate that that's not okay.

My final exam in pathology, which was in the Thirties, consisted of a series of microscopic slides from which the most obvious ones were removed. And from those slides, I was supposed to indicate the age, the sex, the color, the kind of person it was, the laboratory findings, the course in the hospital, and the history and so forth, and predict backward from the slides to those. And that is a training device that is not used hardly anywhere anymore.

All right, now to get to the story, the application of all this to the AFIP.

We had surges of activity--the Civil War, World War I, and World War II. Then we had periods in between when we didn't get very much done.

In the Civil War, we had a whole bunch of trauma to deal with that we hadn't dealt with before, and a whole bunch of gut diseases, with more people dying from disease than dying from trauma. Once this was worked out, then all of a sudden germs took over and we spent all of our time trying to work out what germs did, and to hell with the structure, for the most part.

In World War I, we began to get new kinds of trauma, with shrapnel. We began to get gas. We got new diseases such as the flu. We got trench foot and so forth. All of these we knew nothing about and had to work those out. In the course of doing that, chemistry became very important. And again we went into an interregnum slump which led to not advancing very well.

Then came World War II, and we got a whole bunch of new kinds of trauma. We got arctic diseases and we got tropical diseases and we got many things--immersion foot and blast injuries--that we never had to deal with before, where you can kill without actually showing anything on the patient in terms of a penetration.

Well, we've got the same thing to face for the future. In the future, we know that we're going to have radiation and germ and chemical warfare possibilities which will give us new things that we've never experienced before. We try to get clues about this from occasional accidents that occur when a radiation plant gets into trouble, as happened in Russia, but we've had them happen here as well. But no big effort has been made to collect that material and prepare. This is a failure in preparation.

So the history of the AFIP is concerned with general history, medical history, and with the changing biological research that goes on. And what is seen that goes on here is dependent upon those things.

For instance, in World War I, because we had a lot of experience with germs and a fair amount of experience with chemistry coming on, our clinical laboratories of pathology were doing very well, but our anatomic pathology stank out loud. Wilson from the Mayo Clinic, and Ewing from New York, both pointed out that the anatomic pathologic was very poor. As a matter of fact, the only decent material we had on influenza came from civilian sources and not from military, because we had them dying by the dozens, but we had no good data.

Q: We're talking about the great influenza epidemic of 1918, 1919, which killed probably as many people as World War I did.

DR. JOHNSON: More.

Then, with World War II, we got a great deal more chemistry and we got a lot of new selective pathology coming along. And with World War II, we brought in, as we had in the Civil War and in World War I, civilians to become part of the organization. We dispensed with them when it was over, previously. Colonel Ash brought in some twenty pathology professors of top medical schools in this country, and they were astonished at what they learned here. And they learned it from young men who were working here at the Institute who had to put things together.

One feature about it, for instance, that no university had was that everything on a given patient bore the same number. So that when a piece of an ear came in, you looked and found out that you'd had five other specimens from the patient over the previous ten years, and you got them together, and you began to connect things with each other, which was never done before. In general, at most of our universities, each specimen was separate, and you didn't even know the patient had had previous material. So you related

the old data to the new as well. Because of this, you had surgical specimens which gave the steps in the disease, not the end of the disease which you saw in the autopsy material.

The young men that Colonel Ash brought in were very carefully selected, and many of them wound up as professors in various universities.

Now to go back and relate this to the action of the Institute and to some of its results. We got the six-volume history of the Civil War from the AFIP, which led Virchow, the world's greatest pathologist at the time, to say, "This is something brand new, that we will use the tragedy of war to learn how to take care of patients in non-wartime as well."

Now this involved not only getting to know what it was, but why it was. And this also involved long-term follow up. So you had the specimens, and you had follow up with the patients that lived, to see how they did, whether certain procedures were better than others.

Then, with World War I, we got our special diseases that I've already indicated, like gas and flu and shrapnel. But following World War I, we had a number of problems. We didn't get near as many specimens as we did in the Civil War, primarily because the anatomic pathology was poor. And we began to get shrapnel to deal with, and nobody knew how that worked. And so, in the basement of the old building, the first work in wound ballistics was undertaken by Callender, to find out how does a wound mash or damage tissue, in short and long range and large and different... So that the whole business of wound ballistics began here at the AFIP.

Q: Callender was the director of the AFIP.

DR. JOHNSON: He was one of the directors.

Meantime, it was clear that the registries, which were first started in Boston, by collecting dozens of cases of any particular type, of the eye, or the bone (as a matter of fact, the first one was the bone, but the first one which was brought here was the eye), would give you information that you couldn't get from seeing individual cases. And Amory Codman of Boston pointed out that until you see a lot of cases, with all the variations, you don't understand any case. So the registries got started. and this meant that you had a large archive of material which you could go over and begin to get understanding of disease you couldn't get any other way.

Now during peacetime, the amount of pathology that would come to the pathologists in the military was very scant; you had a healthy group, and you weren't going to get much. And yet they should be able to deal with it when it comes, so Callender arranged for the AFIP to serve as the archive for the registries, and he brought the eye one here. And as a result of his work on the eye one, we began to get understanding of a whole bunch of diseases, cancers of the eye, that we had no understanding of before.

Then in order to do that for the military material, orders went out that you send all the tumors and all interesting cases and all autopsies and so forth from the Army path. labs to the Army Medical Museum, which is what the AFIP was called. This didn't work

very well; it took a good many years to get it to work regularly.

It was beginning to work pretty well, when World War II came along, and then we began to have a lot of trouble in that we had all kinds of new details we hadn't run into before. And this led to some specialization: one man began to look at all the brain material; one man began to look at all the skin, and so forth and so on. So subspecialization, which was going on in civilian... but had never played a part in pathology, began there.

We got a whole bunch of new entities, as I've already indicated. And we began to put out a series of atlases. As a matter of fact, some of them had started before World War II, by the subspecialization of pathology, which Ash introduced, because pathologists did not pay much attention to the subject of pathology. As a matter of fact, during World War II itself, surgical specimens were not permitted or accepted at the Pathology Department at Hopkins. The surgeons had to do it by themselves.

Now a whole bunch of new entities began to appear, post-World War II. As we began to get subspecialization, you began to look at lesions in detail of a particular area, and you became aware of a lot of normality and transitions you weren't aware of before. So some things that were thought to be horrible turned out not to be, and other things that we didn't know anything about. A whole bunch of new entities had come to light. And then we began to get more and more atlases coming out as well.

We finally got around to trying to do something in American pathology comparable to European pathology. European pathology was epitomized by the so-called Hanke Lubarsch multi-volume pathology handbook, which ran to many, many, many volumes. Well, we thought we should do that over again in America. No, that won't work; that's too expensive and we haven't got the setup for doing it, so let's instead do a tumor registry. So the tumor fascicles got started as a result of pressure or encouragement from the outside. The tumor fascicles were all published here, all coordinated here, but did not originate by a staff here until later on.

The other thing that was needed after World War II was follow up, again. The follow up was to be arranged by getting all material from the Veterans' Administration hospitals on patients who had died or had further trouble, who had had difficulty during World War II. And this failed; it never worked worth a hoot.

So a large number of changes were going on. We shifted from ten-to-one disease versus trauma to the occasional disease for all the trauma, and disease just about disappeared as a cause of death during war.

Then we began to do something, in addition. In Korea and in Vietnam, we figured out how to handle it so that we could get the injured patient into medical care within minutes, and not within hours or days. And the result of this was a fantastic increase in survival rate as a result of injury. But we got virtually nothing from Korea and virtually nothing from Vietnam in the line of specimens. They didn't think it was important, the specimens that existed. Everybody thought that, well, you've got plenty of examples of that, you don't need another, and there are other things are more important. (Well, we're going to get to that in a few minutes.)

The result of all this is that you're going to have an archive, and you can go back

to that archive and restudy it. You can restudy the old ones in terms of the new ones, because they all have one number.

To give you a couple of examples. For instance, Grover Cleveland was supposed to have had a cancer of the jaw, which was operated on and he was cured. Well, I've seen those slides--he didn't have a cancer. It was supposed to be a bone tumor. It was not malignant. That's the kind of thing that's needed. The slides have now disappeared and we can't prove it, so nobody will believe it. But I saw them, and that is the field that I have done my work in.

Q: That's very interesting. That secret operation he had is an interesting historical item. Where did you see the slides?

DR. JOHNSON: Right here at the Institute. They were here.

Q: And then what happened?

DR. JOHNSON: And then someone dug them out and asked me to look at them. I said, "No, there's no malignancy there." And then they disappeared, because it had all been written up that there had, and the Institute had... that the cause is a guess.

Another example was the Bone Sarcoma Registry, which was the first one and was started in Boston by Codman. Orthopedic surgeons from all around the country contributed to that registry. There are very few bone tumors, benign or malignant, in general, and so you have to accumulate to get somewhere. Well, we had that original Bone Sarcoma Registry all here, and as we began to go over it, many of them were not sarcomas. That's the kind of thing that this place should be doing and is concerned with.

At the Department of Defense, there's a lot of talk about the fact that we should limit ourselves to those things that are important to defense. Well, one of the complaints that comes up is that we're doing so much cancer pathology now. And as I've indicated, high blood pressure and stroke and cancer are the big things that people talk about now, the things that occur as you get older. And why should we be spending our time doing a lot of cancer? For a very simple reason: in cancer, you get the early and the most precise details about what happens in individual cells, in fine detail. And that fine detail can be then applied to a whole lot of other things as well. So the people who are yelling that we're wasting money and time doing neoplastic stuff are wrong, because there's where you get details you can't get in the ones that are infections and traumas, such as occur with trauma infection. And immunity has become fairly important recently.

Now as the Institute got started after World War II, it was brought together by Billings, in which you would have the library of the surgeon general and the museum of the surgeon general in the same building. So you had the literature of disease and the specimens of disease. The literature of disease was chaotic, and he created the wherewithal that made sense out of it--the Index Medicus, which made it possible to find out about the most recent work and the oldest work on any subject you wanted to work up, and then you could compare it with what you saw in the specimens. And what was in

the literature didn't always agree with the specimens. So one gets taken away by the friends of the library, who don't like the way it's being run and don't like the museum, and they get it taken over, simply and completely. And while we could work very closely with them, we have to work like a medical student now; we can't work closely with them as we used to before.

Q: You're talking about the separation of the medical library. It's over at the National Institutes of Health.

DR. JOHNSON: It's over by NIH, the National Medical Library.

Q: It's a big library, but there's a major separation from that.

DR. JOHNSON: Right, right.

Q: It's five miles away or something like that.

DR. JOHNSON: Then came the idea that we couldn't keep having separate services. So we got tri-service. This was the first tri-service organization that looked wonderful, and I was proud to be a part of it. To run it, we have a board of governors, which involves the surgeons general of the Army, Navy, and Air Force. And then to support it, we've got a scientific advisory group, which involves some of the outstanding pathologists in the country. Then we also have consultants that come in. Well, the consultants began to drop out after a while when we found out that, by and large, the consultants were professors from medical schools where the pathologists were primarily concerned with teaching second- and third-year students the basics of pathology. One person commented that a course in pathology was primarily an introduction to the language of disease. That's an overstatement, but it has a lot to say about it. So our consultants frequently came here and were not familiar with what we were doing in the field that they were talking about. So that sort of died out.

As our scientific advisory board and our board of governors got to working, it began to be clear, and was so stated by one director to me, that the purpose of the scientific advisory board is to be maneuvered into supporting what you want to do. You feed them information that you want, in the order that you want, so they say that's a good thing to do.

Q: I have to say that this is true of every advisory board in the world, practically.

DR. JOHNSON: Right, right. Well, Rich, of Hopkins, was so disgusted with this that he refused to be pulled out and wouldn't have anything more to do with the Institute.

We talked about the fact that we really needed to get experimental studies here, and we set up a separate institute to experiment. We did not set it up so that the pathologists who were doing the regular work could check it experimentally. Well, to

begin with, you don't have time to do both.

Then with the board of governors, they don't know anything about it. All they do is get told what the directors want. So that you get both of them, to some extent, manipulated. You're more responsive to the board of governors in terms of what the military organization demands.

Then we've got the problem when we got two surgeons general. Abrams didn't like the guy who became the next surgeon general, and his pet didn't get it, so we created a second setup whereby all the hospitals of patient care were under a man that he selected, and the surgeon general was left without direct control of that. He had the R&D and Walter Reed and the AFIP and so forth to deal with.

Q: This was about when?

DR. JOHNSON: This was during the latter part of Vietnam, when General Abrams was in charge.

Then we got a separate charter. So all the time it's been a branch of the surgeon general's office, with the Army surgeon general being the manager, and the other two advisory. We got a separate charter as a result of Ted Kennedy's boy who got a sarcoma, and he was appreciative of all the things that were done. One of the men at the Institute took his sabbatical and worked for a year with Kennedy and told him about this. And Kennedy got a separate charter for the Institute.

Q: This was in 1976, I think.

DR. JOHNSON: Something like that. In addition, the registry money which came in could be kept here. Anything that came in went into the general fund, and we couldn't do a damn thing about it no matter what we did before. So we began to get a separate charter, and that leads to still further changes.

Now all along, we get somewhere only because we've got civilian support. And the military nowadays forgets that. We got the Old Red Brick building by virtue of some of the outstanding civilian surgeons who said to get it. We got this one by outstanding ones. It didn't come from the need that was expressed by the military itself.

The AMA was so appreciative of what was done that was recognized worldwide as a result of the Civil War that it created and gave to the Institute a statue of Gross, who was the man that trained most of the men that were at the AFIP during the Civil War, and he wrote the first big textbook of pathology, which even Virchow said taught him a great deal.

So this business about interrelationship between civilian and military is important, but it is ignored as much as possible. And as a matter of fact, I have overheard the commanding general of Walter Reed, and I have been directly told by an AFIP director who forgot that I was not in the military at the time, talking about the goddamn civilians. That's the general attitude. The main reason is that if somebody in uniform doesn't do what you want him to do, you just ship him out, but somebody who is civilian and you

can't ship him out, he's a pain in the neck.

We also have other aspects of interrelations. Craig, who was a director, worked with First, I believe it was, and Strohm, of the Army, worked with Stit, of the Navy (but we don't recognize that here particularly), in developing the whole field of tropical medicine, which became terribly important during World War II. As far as bone was concerned, Femester, who trained me, trained with Erdheim in Vienna. Smetana was trained in Vienna, and he was head of the Pathology Department here for a while. Puchan and..., both of them Europeans, worked very closely with me. So we had a lot of civilian background in this country, as well as in Europe.

Now for some general principles.

Academic pathology in this country is primarily concerned with research. This means experimental, because experimental you can get a lot of papers fairly rapidly. It takes years to write one good anatomic paper. And you can't get much of a bibliography that way. The result is that clinical pathology has advanced rapidly. (We talked about that, from the chemical standpoint, before.) Anatomic pathology dragged and became less effective, as I indicated, when the professors came here during World War II and were astonished to see what could be done. As a matter of fact, they were so astonished that they walked off with some of our gross specimens. Then they never used them; they disappeared. Anatomic pathology went downhill.

Now in the universities, by virtue of this approach, you take an individual person with an individual disease, or an autopsy, and you do very intensive work on that particular patient. So, intensity is the basis on which you get new data.

The registries were completely different. The registries went from one to about thirty of them, here at the AFIP. They all were primarily sent material not from universities or medical schools, but from private practitioners outside.

So here is a big pool of material that is lost, some of it much more valuable than what the university gets. Because in the university, they're doing the best kind of medicine, and a lot of sloppy medicine is being done elsewhere, and you can get to see what slop will do.

So, as a result of the registry material that came here from nonacademics, we could get a volume of material, where we could see the range in variation of data.

That is now becoming important under what is known as fuzzy logic. Now intensity is strict logic, and fuzzy logic is you do it sort of maybe this way, maybe that way, and all of a sudden, all the maybes begin to make sense. That's becoming so important that books have been published within the last year about that.

That's what the registries could do here. Each registry was related to a specific form of clinical disease, and each registry was supported by the clinicians in that specialty. So we were beginning to get the vast support for the clinicians which Ash had originally worked out himself, and now all the registries began to take advantage of that. And, of course, with the registries, we're going to get different stages of disease, so we can go back and begin to understand mechanisms, as I indicated before, that we did not understand previously.

Now some things about the organization of this place. The organization is an

historical accident. The gross specimens dominated the Civil War, and that got to be called, since it was originally an Army medical museum, that is now called the Museum. Then in World War I, we got movies and photographs, and that became the Illustration Division. We had Pathology all along, and then we got the registries. So you've got the Museum, the Illustration Service, the Pathology Department, and the registries kept as if they were separate things, which is totally irrational, an historical accident.

A general and basic reorganization of this place is needed, in which you reorganize Gross and Radiology, because the Radiology depends upon the Gross, but we don't fit the two together. And the material that was originally in the Museum is needed, because we don't see it any more; those things have gone.

We also added dental material, which the doctors don't pay any attention to and think is no good. But in my field, dental is a very important part of understanding how the bone goes along. We don't even have teeth coming in here anymore, because all we know about teeth is when we do oral pathology. And I've got more dental material than the, presumably, Dental and Oral Department has, because they're not interested in teeth.

Then we began to get a lot of anomalies coming in as we began to do babies, and Maude Abbott played an important role, from Hopkins, in that. And we began to get comparative anatomy. And by looking at these things in different aspects, we could begin to understand particular pathways related to particular diseases and what their consequences would be.

Now the Museum, as I said, should be concerned with the specimens and x-rays. And then we have the microscopic, and I'll come back to that a bit later. And then we've got the submicroscopic, which gets to the semi-chemistry and the electron microscopy.

Now as this has developed, we've had a whole series of directors. They were all physicians until General Callender came along. None of them were trained as pathologists, and all autopsies were just done by a doctor who was assigned to the laboratory. Callender was the first trained pathologist. And then we had, from then on, pathologists involved. Many of the directors were here just filling time and really accomplished nothing. Then we got the tri-service coming in and...

Q: This was around 1948ish or something like that.

DR. JOHNSON: Something like that, yes. And with the tri-service, you've got the Navy coming in. And Stewart, who was long head of pathology at NIH, pointed out that they recognized that when the Navy got in, we would have trouble, because the Navy would get things its own way and was not always cooperative.

Q: There's that saying: "There's the right way, the wrong way, and the Navy way."

DR. JOHNSON: Yes, and each said that about the other. The Navy has got to look good; it has to represent this country in various parts of the world related to its battleships. It looks wonderful, but when you try to work things out in detail underneath, you have a hell of a job, at least as far as pathology is concerned. We had two Navy men

who didn't really fit in with the way the place had run before.

Then we got an Air Force man who was so bad that they had to get rid of him after a couple of years, and who couldn't be trusted to make diagnoses in Texas. We have letters in our file in which there were... opinion... on these obvious things, we just don't trust this guy. He thought he was good enough so that when he got here he was going to get a star. And when he found out he wasn't listed for a star, he didn't give a damn about things. And he wrote, against regulations, a devastating report on the Institute.

Q: Yes, I have that.

DR. JOHNSON: And I... to answer that report that I wrote, which was a special answer. They wanted an answer within a couple or three days, and it took longer than that. It's down in the Museum files if you want to look at it.

Then things began to change. We got an Air Force man who was lazy and didn't get around to doing things, and it got so bad that the surgeon general decided, by God, I want you to come down here at regular intervals and see what's going on. Then he put an Army man in, a Navy man in, and now they've got an Air Force man in, who have never had command function, never even run a laboratory, just did things and moved up. In some respects, I suspect that they were sent here to get rid of them. We used to have a lot of that trouble during World War II and right after. The general thing is when the surgeon general had someone he didn't know what to do with, send them to the AFIP. So we didn't get the best kind of help.

Now of the various ones who've been here, the ones who had worldwide notoriety for what they accomplished were Hammond, who started the place, Billings, Callender, and Ash.

And then, at the time of Hansen, at the hundredth anniversary, something that had never been done before, a special issue of the Virchow archive, dedicated to and written entirely by members of the staff, was put out. This was something brand new, in recognition of what had been accomplished.

But after that, we have no proper experience for people for running an independent organization which has now got its own charter, and have had no command function and no command experience. And we've got all kinds of trouble as a result of it.

We pay no attention to civilians; we don't give a damn about them. As a matter of fact, Lovichka, at the Smithsonian, got his whole start on material that was turned over to him that had been collected from all over the country, unrelated to war. Matthews, who was one of the great anthropologists and who wrote up the details of the night chant of the Navaho and so forth, was here. Nobody even knows about it. Woodward, who invented photomicrography and staining, when nobody else was doing it, as well as doing microscopy, is never recognized or mentioned. Ryan, who held the place together while the guys that were doing all the germ work, like Carroll and Reed and Russell, held the place together, otherwise it would have just gone down, and nobody gives a hoot about him. In general, as I've said, we don't care much about the civilians.

Now for some particular details of Karnei, because he's been the source of a lot of difficulties. He didn't care much about the civilians. He refused to follow orders from the surgeon general; when the surgeon general said this man should get a bonus, he refused to do it. He did this twice, and the surgeon general had to give him hell and make him give that guy a bonus. He didn't like the guy, so to hell with him. He put in, as a matter of fact, this is a Navy principal, put in a whole bunch of people between himself and the pathologists. And these guys are perfect because he can say, "That's what they told me." And he can say, "I didn't know," when the pathologists were upset about things. They brought in two individuals who became very powerful: Micozzi for the Museum, and King for the registries. King was picked out of the University of Chicago because he was so damned bad as a dean. Micozzi was in trouble with other places, and he came in. Each of them was trying to make theirs separate.

There is the thought that the Museum would become independent and would be downtown. Koop was the surgeon general for the NIH group as part of a big organization to do this. And this is possible because the surgeon general's investigation said you have no excuse for having all this stuff in a museum. Most of the stuff was artifacts and had nothing to do with specimens. And they came here to investigate, and, boy, they were close. Somebody told them, you go talk to Johnson. And I said, "Those specimens are the only thing we've got for gross material. We've got to keep them." And so they said, "Fine. Keep the gross and get the other stuff out." But the way they're doing it is they're taking some of all our material in Bone, which is microslides. The Museum was taking over, because Karnei was mad as hell at the director of Orthopedic Pathology, who wouldn't write a nasty letter to get me fired. He came here with the idea of firing another pathologist, by the name of Ping, and myself. He hated us. Well, Ping died during the course of the regulation. But his lawyer said that he'd never seen so much monkey business as went on in this administration.

Therefore, we've got to push. And King goes down to Congress with me to try and get the registries separated. And there's been talk of moving it to Baltimore.

And we charge a fee now for anything that anybody sends in. This was in order to cut down on a lot of stuff that we couldn't handle. So we got the fee, and it drops down. Then they said, gee, we don't have the money. So we're going to have the reports go out in twenty-four to forty-eight hours in order to get more people to send stuff here. And it turns out the reports aren't getting out late from here primarily because of here, but primarily because there's up to ten days between when the pathologist got the material and the time he mails it. And then there's the mailing time, which isn't too bad. And so now they want to speed it up, and they were going to speed up.... take a diversion again and indicate it.

Over there I've got material on cases that have been sent in from Stanford University. I've spent two weeks on it and I still haven't got it figured out. But I'm gradually getting it figured out. The material that came here, originally came here because people couldn't understand it. If it can't be understood, then it takes more than a glance and a quick look. And yet the front doesn't respect this at all.

Another thing that's related to the Museum is that it's assumed that diseases are

constant, that we're always going to have diseases. Well, that's clearly not true. That's why we've only got cancer and high blood pressure and strokes in most of our autopsy services today. Well, they're gone, we don't have to worry about it. Oh, yeah? You've got other ones that are coming along. When you begin to get rid of one group of infections, others take over that before could not survive. So we've got new ones coming in. The old ones come back the minute things get bad. We're getting tuberculosis coming back, and syphilis coming back, in a population that knows nothing and pathologists have never seen the damn stuff. That's why the idea that it's constant is wrong.

I proposed that the Museum should be primarily concerned with the history of disease, not of doctors and not of medicine, but how diseases come and go with different kinds of ecological circumstances. That was knocked out.

Then we've got big plans for the future. We're going to have telepathology. Somebody's going to press a button in Texas, and you're going to be alerted to it, and they're going to put the stuff on a screen, by communication, and ask you what the diagnosis is. Well, this is going to be used in local areas where you've got four pathologists with good pathologists, and they look for fairly obvious things. But it can't be used for the kind of problems that come here.

First of all, you will see only the fields that he can show you. You can't look at the whole slide under a microscope. So they said, well, we can get around that. We'll fix it so that, from here, I can move that slide under the microscope. I can move it by..., but I can't feel it. And the feeling that I have is what integrates it in my head, the different things I see in the different areas.

Next, I'll only have the slides that he says are worth looking at. I won't even know he's got other slides.

And third, I don't get a good history; I don't get all the data I need to put it together.

So telepathology is for the birds.

You want to deal with complex material, which this Institute should do, not duplicating what others do. We should be doing what others can't do, and not be duplicating anyone.

The next thing is we've got the computer coming in. The computer is actually occupying so much time keeping records that we're cutting down on the output of work. A massive amount of time goes into just sitting and poking around with that computer.

Then we've got the DNA coming in. The DNA so that we can identify corpses in wartime by DNA instead of by your dog tags, if you don't have them, or by the dentifrices and broken bones if you've got records of it and so forth.

I think the challenge has to be eventually raised that the cost of doing this, of bringing people back and getting them to the right families, is so high that I don't think we've got a justification for it. But the amount of money that's needed for the computers and for the DNA is so vast that much of the money that belongs to the support of pathology has diverted into that area. And the DNA is only just for the future. As I've already indicated, genomes aren't going to give you the answer to a lot of things that people think it's going to do.

Then we've got individual departments being given individual budgets. And they had to get their budgets by teaching courses. Well, we started out teaching courses for guys that needed it. Now, we teach courses in order to get money--only for getting money, and that's all. And then it goes into the registries, and the front office takes a chunk out. The registry takes big chunks out for food and stuff like last night.

Q: You're talking about the Ash Lecture last night and a big dinner and all that.

DR. JOHNSON: Yes, that's right. And the result is that the budget that's assigned to this department is all used up and we get very little of it. It's misused.

Q: Administrative expenses, as usual.

DR. JOHNSON: Most of which are unnecessary. We could get rid of a lot of people in the administration, and run this place much better than it's being run now.

The other thing is that pathology was always represented by a head of the department, ever since World War II. That head of a department was a civilian.

The first one was Hans Smetana, who was from Hopkins and a brilliantly trained individual who was aware of what needed to be done. He was sent to Asia to come back and report on our laboratory there. He reported on the laboratory as you would a civilian one, and he damned near got killed for it. His report was destroyed and only the parts of it were used that made it look good. That's the basic military way of doing things.

Then we had Grady who was there, and Grady had been in long enough to realize that didn't work. So he, as someone used to say, whenever he went into a department, he'd tell his beads and just keep his mouth shut.

Then, for a long period of time, we had a superb individual, Elson Helwig, who recognized that the primary function of such an individual was to serve as a buffer between the administration and the working pathologists. As a buffer, he came to each pathologist, talked to them individually, and then made his own summary and talked to the administration. And when the administration had ideas, he told us such parts of it that were essential and were valid. That way, we kept out of trouble. When it came time to retire, he suggested that Mostofi take over because he was the best man in that he was a close friend of the surgeon general, who was GU man, which was Mostofi's field.

Mostofi, when there's a bit of pressure, Mostofi comes first. And then he did something you're never supposed to do, which Smetana did--"If you don't do it my way, I'll resign." "So, fine, we'll accept your resignation."

Now, there's no civilian who plays any role. Everything is done by the military itself, and done through these subdirectors who have only one thing to do, and that one thing is to get along with the director.

So those are the things.

Now why am I talking this way? Why have I got to talk this way? I'll give you the background.

I trained at the University of Chicago and graduated. I was there ten years. I

trained in medicine at Harvard. And I had my training, after medicine, in surgery. Then I went into pathology. And I had people like Femester and Hectone, who I forgot to mention was the individual who made the registries coming here possible by being on the National Research Council, and was one of the great pathologists that we had. Each time, I thought I'd go back into the field I was in. I eventually decided to stay in pathology. And I had a broad experience in the... of patients. Most pathologists today have never taken care of a patient and they don't know what the patient aspect of it is. I also have AOA and Sigma Psi, which are honorary, you can't join them, you are invited for outstanding work in research or outstanding work in medical school.

As I indicated, we should have a history of disease. We don't have it. At the hundredth anniversary, I said that the primary thing we should have is the fact that when we get dense populations, we get a whole new bunch of diseases by cramming people together, just having to live together aside from usual. And that was dumped. I was the director of paleopathology here, and trained both Ph.D. and Master degrees, getting their degree under my direction here in paleopathology. I did the original forensic pathology, which is mostly bone, for the FBI, before they got other people here. One of the big things that I did was making large sections and demonstrating that the disease you get in the bone depends on what part of the bone, not the kind of gene. It's field pathology, which goes above the large organ and microscope, and you have to have big sections like this to compare...

Q: He's showing me a section of about six by eight, a glass slide.

DR. JOHNSON: So field pathology, which was never developed adequately. And there is no constitutional pathology in this country. Both of those are essential.

Q: Constitutional pathology is what?

DR. JOHNSON: It means the pathology of how the whole individual fits together, not just how individual parts...

Q: Sort of holistic or whatever...

DR. JOHNSON: Well, no. Like... Certain types of people got tuberculosis, certain types of people got apoplexy, and so forth, not everybody.

Then I created a bone lab, which didn't exist here. We figured out how to make those large slides and make them rapidly and do them with electrolytic decalcification. If we needed to, we could go from a gross specimen to the slide in five days, which nobody else could do. We worked out a whole bunch of specialized chemical studies in which we could work out the chemistry of what was going on. We figured out how to make undecalcified sections, and from the undecalcified sections, we could make micro x-rays, which would be read only under the microscope. Every specimen was x-rayed before it was prepared, so we could correlate regular x-rays and clinical x-rays. X-ray of the

specimen, micro x-rays, and histology, and put it all together. And we worked out methods for using ultraviolet light, infrared light, and polarized light forms. We studied them long before it got into the literature. We worked out the whole details of the development of bone and how it develops in different parts of the body, from embryo on... We worked out a good deal of comparative, because what you see in one animal emphasizes; what we see in an animal brings it to light. Then I brought in material from Harvard, from Hopkins, from the University of Chicago, and from Zurich and... These are university collections which ordinarily don't get here.

I developed the X-ray Registry and was offered to run it. I said, "No, I can't do too many things." But that all is the result of what I did.

And I worked out the method of teaching x-rays. You show the x-ray, and then you interact the x-ray to these specimens, to show how...

Q: You're talking about the large slides.

DR. JOHNSON: How this part of the x-ray fits that, and that part of the x-ray fits that, and there isn't just a single diagnostic term that accounts for it.

So those are some of the things. And then, as I said, there are several reports in the archives that I made about what could and should be done at the Institute. This is the background against which the understanding of the Institute's mission is, and that that mission is not merely in terms of the immediate. It has to lay a lot of groundwork before you can use it, come wartimes or come disasters of various kinds, including ecological disasters. And this should develop part of the mission which nobody else can do.

As the Institute has gotten involved with germs and chemicals, and now with DNA and so forth, those things eventually get shipped out to specialties for those, and the thing that is essential remains the structure of disease.

Q: Well, thank you very much, doctor. I really appreciate this.