

Dr. John Sherman Interview

Office of NIH History
Oral History Program

Transcript Date: June 21, 2005

Prepared by:
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Dr. John Sherman Interview

Buhm Soon Park: This is an oral history interview with Dr. John Sherman. Today is June 1st, 2005 and interview is conducted at Dr. Alan Schechter's home in Bethesda, Maryland. I am Dr. Buhm Soon Park of NIH History Office. Thank you very much Dr. Sherman for giving me this wonderful opportunity to talk about the history of NIH. Could I start now?

John Sherman: Surely.

BSP: I understand, Dr. Sherman, you spent 21 years at the NIH from 1953 to 1974, first as a bench scientist, intramural scientist, and then as an extramural administrator. You also served as a Deputy Director of the NIH from 1968 to 1974 and at one time Acting Director of NIH. I would like to focus our conversation today on the period when you were a Deputy Director, a very important time in the history of NIH. There were lots of changes and conflicts and tensions, but like any other interview I'm going to start with your background. Could you tell me briefly about your family and educational background? How you came to NIH and why you decided to come to NIH as, I understand a commissioned officer and could you say a bit of your pre-NIH history?

JS: Sure. My CV shows I was born in a small town in upstate New York in 1919. My father had a pharmacy in this town and I somewhat gravitated to working in that and thinking of it in the future and then World War II came along of course and I happened [laugh] standing near the front of the line when the draft occurred. So I was drafted in February of 1942 – '41, before Pearl Harbor. I have a wonderful; to me anyway, anecdote about that time and my future life in the Army, and that determined a lot of other things as well. A traffic policeman watched me grow up because he had been on the corner where my father's drugstore had been located. He was very fond of me and when he found out that I was about to be drafted in February of '41 he said, "Johnny –," the only person who ever called me Johnny – he said, "I understand you're going to be drafted into the Army. Would you mind listening to a few words of advice from an old World War I veteran?" It was all new to me so I said, "Fred, I'd be willing to take advice from anybody," what I was about to face being a big unknown. So he said, "Now

remember, never tell anybody in the Army that you worked in your father's drug store or you will be buried in bedpans all of your time in the Army," Then he said, "Never serve in the Signal Corps because they climb trees and poles and people shoot at them, and stay out of the infantry because they walk in the mud all the time and they get shot at, too." He said, "Stay in – get in the artillery because they ride."

Well a few days after that, together with 20 other young fellows from you my hometown, all draftees, had been sent to Albany New York for induction in the Army. We stayed there all of one night and then we were trained to camp World War II days on Long Islands called Camp Upton. At the time the Army had no provisions whatsoever for really organizing the processes of inducting a person and deciding whether they wanted to put him in the Army and then training him. So we were all just thrown together there and I remember one time we were told to check a bulletin board to find out what assignment we were to get. The first one I had was air force, but then I was one down on the number and wasn't chosen. Two days later my name did appear. I was going to be assigned to the 4th Infantry Division in Fort Benning, Georgia. It was filling up its ranks because it had just assisted in the mobilization of the 8th Infantry Division in [unintelligible] like the 4th had previously. We took five days on a troop train to get from New York City area to Fort Benning near Columbus, Georgia. We arrived on a cold wet Saturday night. We were ushered into a big almost circus-like tent with very poor lighting from lanterns and so forth, and out of the 550 troops, I understand, on that train there must have been formed at least a dozen lines for individuals to join as told. When they arrived they reached the front of the line there was a sergeant at the desk and a roster there and you were then assigned the unit you would be in – as well as what branch of the Army.

I realized when I was standing in the middle of this line, which moved rather slowly, and it was an eerie environment with the darkness and these lanterns and so forth, that there was someone standing by my side. I looked up and here, resplendent in full dress uniform and very tall individual, was what turned out to be the division – 4th Division chief of staff who had come on a Saturday, even with the cold night, over to this tent to see how the

assignments were coming along. He looked at me and you know I thought he was probably just descended from heaven or something with all the gold [medals] that I'd never seen before and he said to me, "Sherman –," well first of all, "What's your name?" I said, "Sherman." He said – he did a – "What do you want to be in the Army?" I remember old Fred's admonition and I said, "Sir, I'd like to be in the artillery." He asked why and I said oh I'd taken extra courses in high school and so forth and he says, "Well, okay," and walked away.

Well, when I got up to the front of the line and faced the sergeant and I was assigned to Company F of the 8th Infantry where I was, according to Fred, not supposed to go, but there wasn't anything I could do about. Well two weeks later my infantry platoon was out on an exercise and I heard my name called by a corporal in a command car. They had just driven up and they said, "You Sherman?" And I said, "Yes." He said, "Come with me. Pick up all your gear. You're being transferred to the 20th Field Artillery." It was a minor miracle. So started out in the field artillery, which had been my ultimate choice of branch had I any choice at all, and almost immediately, we were sent to Louisiana for maneuvers and then back to Camp Butner for re-training and refurbishing of our equipment. Not long after that one of the fellows in our motor pool had done just an excellent job of taking care of our vehicles during that pretty nasty maneuver and begged first sergeant to give them a pass, which under the command of another commander, wasn't supposed to be done. But the first sergeant was a humane individual and he gave it to him, and the fellow lived 100 miles or so south of the camp, and he hitched a ride with a fellow who turned out to be drunk and had an accident and he was killed.

So on what became Pearl Harbor Day, I and three other fellows were ordered to form up a color guard for his funeral, the first time I'd been in anything like this; he had very little experience because he was a young man. His parents lived in a little house out on stilts out in the country somewhere. On our way back we had a our radio tuned to a private station and heard about Pearl Harbor, got back to Fort Benning after dark, and we were afraid for our lives because trucks were running all over the place and people were shooting anytime they saw a shadow. Well the first sergeant had taken a liking to me and he wanted to know if I wanted to go to

Officer Candidate School at Fort Sill. I said, "Yeah," I found that pretty good. So I was assigned to class 16 at Fort Sill, went through the three month officer training course, graduated from there, I think, the 1st of June 1942 and was assigned to the 4th Infantry Division – or the 89th Infantry Division that was then being activated at Camp Carson [?] and I did fairly well even without a college education. I was first lieutenant, and then the powers that be in the War Department decided that they needed to try out an arrangement where a regular triangular infantry division with 15,000 men and heavy equipment--we had 105 of their Howitzers, and all of that equipment was taken away from us and we were given jeeps and also a big mortar in place of our Howitzer-- which was a terrible disgrace for artillery. We maneuvered in terrible weather, the worst ice storm they ever had in Louisiana, and nobody bothered to read the map where they had put us for our basic camp and we ended up in the swamp. Literally the vehicles were down to their axles in mud and we were supposed to be fighting there. Well we finally got up from there and ended up on two trains and went on to California. In the meantime, I was able to persuade my battalion commander to let me off the train so that I could get back to Denver to get married. We maneuvered on what was called 180th military reservation near the site of the Hearst Ranch in San Simion on the coast of California. [Audio Break] – because as I said we had jeeps, at least we could get around in the mud out there. It was 171st Division that maneuvered against us, and their organic transportation was hundreds and hundreds of mules. I didn't care for mules. Well the country was so rugged and without our heavy transportation and weaponry it took us a week before we actually made contact even though we were just two or three miles apart [unintelligible]. Well the War Department decided after all that the situation wasn't going to work, so they put us back on troop trains and we came all the way east to Butner up to North Carolina to near Durham in Raleigh, and we were completely reequipped with our old equipment and retrained as a regular infantry division. Well that meant you were a year late in going overseas [inaudible] and we went over in January 1945. Finally, went into lines in Germany in March – late March of 1946.

There were two experiences that were notable; one we made in combat, an assault crossing, at the Rhine River at the Lorelei Rock where the Rhine

narrowed to a very sharp cannon and we lost a lot of men – folks were getting picked off like pickles in a barrel with the Germans firing at them from the other side, but we made it, and then the most horrible experience of all, one of our combat teams, we had three, was attached to the 4th Armored Division and we overran the first of the concentration camps. That's one where if you've been to the Holocaust Museum you see the picture of Eisenhower, Bradley and Patton and all the rest of the bigwigs looking at the pits where the prisoners were cremated, and that was a real shocker. None of us have ever gotten over that. And then we went on into Germany where we went further east and were told to stop because [unintelligible] was with Stalin and [unintelligible] river would be the boundary, and then we went back for another interesting experience and not –[break in audio] – infantry division. We ran a series of what they called the cigarette camps. These were staging camps for troops that were coming back from Germany and either going on to Japan or coming back to the States for a transfer. Our division had the responsibility of managing these camps and the biggest one held, I think, somewhere around 65,000 troops and we provided all the feeding and the housekeeping [break in audio] artillery headquartered and I came back just before Christmas '45 on the advance party division. [Audio break] – immediately decided when I got out what I intended to do was go to pharmacy school, which was my father's suggestion and I graduated from Albany and decided that I needed graduate work to do anything useful unless I wanted to work in a drugstore for the rest of my life.

I graduated with a PhD in science from Yale in 1953.

BSP: What was your major?

JS: In Pharmacology.

BSP: Pharmacology?

JS: Yup.

BSP: So you studied a lot of biochemistry, physiology and...

JS: Yes and a lot in pharmacology and cell

BSP: The standard –

- JS: Good basic courses – I had an excellent mentor, and one day we were told a recruiter was coming up from a laboratory at NIH and he was a bouncy little fellow who'd been really actively involved in the anti-malarial campaign in World War II. So he interviewed two of us and selected me to take the job, and I didn't have my thesis completed and he said, "Come on down anyway and get to work." So I came down here in late 1953 and I was in the laboratory in the section on chemotherapy, laboratory of tropical diseases in what was then the National — Institute, later became Institute of Allergy and Infectious Diseases. I worked there in that lab for three to four years and then the powers that be decided that malaria was no longer an American disease threat. And so they decided they didn't need that laboratory and they broke the laboratory up. We all went six different ways.
- BSP: Right. Well, before getting into that important subject let me just pause on the moment that you came to NIH. You said that there was a recruiter that came to Yale and picked you up even before you finished your thesis. Did you explore other career opportunities or just you are so attracted to NIH? Was NIH that good for you to choose at the time?
- JS: Well, actually I suppose as I remember there were people from other institutions but, you know, at that time the research level at American universities and medical schools was pretty low. There wasn't much of anything going. There wasn't any money to support it, and so I was most attracted to the program at Yale. I knew the institution, that was an attraction. They didn't – at first they didn't give me a scholarship, which I'd been offered in another place, but it was associated with tobacco money and I didn't want that.
- BSP: As a World War II veteran you just became a Public Service Commissioned Officer?
- JS: Bob Coatney was a section chief. He was there at the time, and after I came aboard at NIH, my paperwork had been completed and he said, "John, one thing you ought to decide right away do you want to be in the Commissioned Corps or do you want to go into civil service? Now let me warn about one thing." He said, "If you come into the Commission Corps you can count your military time, as far as pay purposes are concerned, and that will make the difference between \$7,000 odd dollars in the Commissioned Corps and \$5,000 as a civil servant. Well I had been a member of the Commissioned Corps in the Army, but that \$2,000 in salary

made a big difference in the attraction because I had by that time a wife and a baby daughter. So that was one of the easier decisions I had to make.

BSP: In terms of your daily work it doesn't make any difference? No?

JS: No. The laboratory of tropical diseases, and as a matter fact the whole Microbiological Institute, was largely at that time quite in contrast to the newer institutes with old-timers from the Public Health Service; they had done – before the war they worked on malaria and other tropical diseases for the Army. So I just started naturally in that area.

BSP: Who decided that malaria would no longer be a problem that NIH should deal with?

JS: Well that was a big mystery. Whether it was made – I don't think it was made at the NIH level. I think it was made further up the line.

BSP: The Public Health Service?

JS: Yeah.

BSP: The Surgeon General –

JS: Or even the department I'm not sure, but anyway the lab was dissolved and most people stayed at NIH in different institutes. I couldn't find an attractive place for a pharmacologist so I ended up in the extramural program in the Arthritis Institute but retained my rank and my pay in that position. So it was a period of some confusion. A year later after they dissolved that lab, simian malaria was discovered in the South East area of Asia, and they tried quickly to reconstitute the lab, but it wasn't until a number of years later that they recruited a good lab in malaria.

JS: On the simple conclusion that malaria is no longer an American problem.

BSP: Could you describe about the situation that the National Microbiological Institute or NIAID faced in mid-'50s when the emphasis was increasingly on the chronic diseases and infectious disease, the diseases of the past and, you know, and so could you comment on the general sentiment –

JS: Well it was –

BSP: NIAID?

JS: I was at that time in the intramural program, and therefore [reviewing?] grants from many of the institutes. So essentially the intramural program was still concentrated on various water-borne diseases and most of them were indeed tropical and most of them with no seemingly direct connection to the status of American health, a very short-sighted attitude of course but it prevailed.

BSP: Was it a big – was it a big change from bench scientist to the extramural program officer for you?

JS: Well the last days of the Biological Institute laboratory of tropical diseases, even though it continued without the malaria program, was still sort of a backwater of NIH attention anyway, and the administration in the grants program was just beginning to grow and I was attracted very much by the man who recruited me; I had several nice offers. He was head of the extramural program at that time and arthritis and metabolic diseases and what it was then was fascinating. He and I became very close friends.

BSP: Do you remember the name?

JS: Ralph Knutti.

BSP: Ralph Knu –

JS: K-N-U-T-T-I. He later became Director of the Heart Institute before he retired, and Ralph was – he was an excellent musician. He was interested in a wide variety of topics and he knew his business. He had been Chief of Pediatric at Children's Hospital in Los Angeles and he'd been recruited to come to NIH. He had a pretty good shot there for research and training, especially in arthritis, metabolic diseases, diabetes...It greatly expanded. So I turned down the other offers and went with him [unintelligible]. [unintelligible] my pay and I was very happy there. You know the program was beginning to expand with increased appropriations. And while I missed the lab, I had plenty of interesting work to do building up that extramural program.

BSP: Right could you say a little bit more about how you build – how you built the extramural program? What kind of daily work you did and how to do your business at the office when it is rapidly growing?

JS: Well, in the first place, there was a lot of organization to do because it was obvious at that time the NIH was going to grow and some of the processes in play crossed the entire spectrum of extramural program and were just inadequate to the task. So a little bit of the time was spent on organizing a system to keep track of the grants and their content and their direction and roles in the general scheme and strategy for the institute. We had a staff, as I recall, of two professionals, Ralph Knutti and myself, a secretary and we had two women [unintelligible] for the research grants component, and the other did fellowships and research training grants because they were quite different, and then we gradually over time, as the program grew, the staff grew too when I left it. I remember they were looking for a physician to work on rotavirus, a research training program and we recruited one fellow from the Yale faculty on the recommendation that his mentor there said that he thought he'd do better at research and in preference to working in the patient area. Well I stayed with Ralph Knutti –became chief of the extramural programs, and then I was given an offer to go to the Neurology Institute as Associate Director for Extramural. Well we just didn't hit it off; it was a different atmosphere than I had been used to in the Arthritis Institute.

BSP: What's the difference? Could you describe a little bit?

JS: Well, in the first place, I think probably the field of neurology was moving faster than in arthritis and diabetes and some of the more chronic diseases, and that was a plus, but their minuses were the director at that time, Dick Masland who is now dead was administrator; he didn't particularly care about the details of where things were going and I guess maybe I was I was more concerned with everything that was going on. There were some excellent people in the Neurology Institute. I remember I was having a business trip to Italy and Sweden and got a call from Joe Ingram who was a legendary doctor at NIH at the time. I don't know if you've run across him in that context. Joe was the Head of the Rheumatology group at NIAID, but he was also regarded as the family physician for the NIH staff, a wonderful person, and Joe called me saying – I was in Italy at the time but he said, "John, you got to come back [laugh]." He said, "Ralph is going to the Heart Institute as Director and we need you, we need you, we need you." Well, actually I did submit my resignation at the Neurology

Institute as Associate Director taking Ralph's place in the Arthritis Institute.

BSP: Did a research background in pharmacology helping you to manage extramural business?

JS: Yes, in large part because most treatments for most chronic diseases, setting aside cancer, which was always different from all the others, was bereft of really efficacious treatments. The opportunity to grant applications for science, and then secondly there we enlarged the opportunity to, in some instances, establish, in other instances, expand the research training programs of young clinicians in those fields as interns...So there's always something new and challenging in which I think were, and I may be unfair to the Neurology Institute in this, I'm not certain, but it seemed to me that they were leaning towards pursuing more rigorously

BSP Can you briefly describe how the extramural program was populated at the time? I understand that the grant proposals were sent to the division, grant research grants and fellowships, and you were the institutions, this particular institution, institute's extramural program officer, but at the same time you worked closely with the Division of Research Grants and then it when it went through the peer review process and all the grant proposals were sent to the advisory council meeting. In that process what kind of leverage you, as an Associate Director or the Grant Officer – what kind of leverage did you have in molding the program?

JS: Well at the start most of it was limited to my own institute. It was complemented by the fact that we used to have, and I guess there still is, an executive – well anyway it was a meeting of all the lay officers from the other institutes that met once a month to talk about common problems. That was originally chaired by Ernest Allen who was the – not the first, but an early Director of the Division of Research Grants. It was primarily designed to debate and assure the adoption of common rules of operation or anything that pertained to the organization in being the NIH intramural program by themselves. I was in that job at NIAID for five years I think. I was Associate Director of Extramural Programs. Well that gave me a fair amount of leverage on the entire extramural operation, because we again formulated and we ran a pretty darn good extramural program with no scandals, no problems. The usual list of rules and so forth common to so many government agencies we didn't have. We had sort of a handbook

that we made up ourselves that didn't pay attention to any government rules.

BSP: Oh I see.

JS: Finally somebody caught up with us, that was when I was asked to come over and be Deputy Director.

BSP: Could you give an example of the rules that you devised? If you remember.

JS: Well, we cleaned up the rules for the review process itself, on the receipt of applications; deadlines were established for grants between one institute and another. That was particularly important at the time when our appropriations were growing, but not very fast; there were numerous instances of excellent applications in one institute where there just was not enough money to fund them. We tried to arrange an inter-institute transfer of promising applications, and we would arrange the use of common terminology in their application, anything to clarify, both for the NIH but also for the benefit of the grants community, so that they understood the process that much better. This was all before the days of electronic submission and so forth.

BSP: The initiative to change rules or revise the rules came from inside or came from extramural communities or the grant and so forth?

JS: Well mostly from amongst researchers. A large number of these people, all we did was to have them attend study section meetings we had, so we – each of us at the institute became quite familiar with the work of particular individuals in that institute's area of research.

BSP: Did you go to the advisory council meetings? And one thing that I'd like to know in particular was that in most advisory councils there were laymen, laypeople –

JS: In all.

BSP: Yeah, and I understand about half of them were laypeople and could you say something about how they gave their inputs and how the experts, the scientists and physicians reacted to that, and what was going on in the council minutes.

JS: Right. The – I think the most useful role of the – what did we call it? ECME, the extramural –

BSP: Just a moment. This is an oral history interview with Dr. John Sherman in Palmson Park [spelled phonetically]; this is the second tape. We moved to Dr. Sherman's house in Rockville, Maryland because in the previous tape we found that some of the conversations were recorded too faint, so we're going to repeat some of the subjects that we covered and then we moved onto Dr. Sherman's time in the 1960's and 1970's.

Okay, Dr. Sherman, could you briefly talk about your decision to come to NIH from Yale University and to NIAID, at the time National Microbiology Institute. What affected your decision?

JS: Well to start with, at the time I received my PhD from Yale in 1953, the opportunities available to young people coming out with graduate degrees in the medical sciences from any university or graduate program were considerable fewer than what is taken to be the norm ever since. The NIH grants program had not nearly achieved its rate of growth at that time, and the NIH intramural program also was not yet growing to the extent that it should.

But I was interviewed in New Haven by a member of the NIH staff from the old Microbiological Institute, the original title of the National Institute of Allergy and Infectious Diseases, who was looking for a pharmacologist to work in their program on antimalarial drugs. There had been some effectiveness from the drugs used during World War II, but not nearly such as could control epidemics of malaria in malaria-infested countries, particularly in the tropical sectors of the world.

The interviewer was named Dr. G. Robert Coatney, who had been active in antimalarial research during World War II, had come to Bethesda as part of the section on chemotherapy, which he headed up, and that, in turn, was a component of the Laboratory of Tropical Diseases, which studied a whole variety of insect-borne diseases, water-borne and air-borne, endemic to tropical areas. Of the two people he interviewed he selected me. Although my dissertation wasn't quite finished, he was ready to recruit me on the spot. So, in effect, I came to Bethesda prior to December 1953 and achieved my degree in December of '53.

Rob right away gave me, or faced me with one conundrum, which was relatively easily solved in that he said, "John, you have a choice facing you. You can either join the Commissioned Corps of the Public Health Service," about which I knew nothing, "Or you could come in as a civil servant with a pay level of about \$7200 - \$7500 a year." That because of my military service in the Commissioned Corps, even though it would amount to a lesser grade than my majority in the Army, would get me the grand salary of about \$7200 a year because of my Army service, and so naturally I took the later all other things considered equal.

I did finish my dissertation at the laboratory in Bethesda. I graduated with a PhD in December '53, and assumed the position at laboratory. I worked there until sometime in 1956, when some higher level of government involved, with considerable influence in the health arena, decided that malaria, now that the war was over, was no longer an American threat and, as a consequence, the lab was dispersed, completely broken up, and our personnel went widely within NIH, but also as far away as Hamilton, Montana, the PHS lab there, and one went out to the University of Rhode Island to head up the department of, I think, chemistry in that institution.

BSP: Do you remember any relationship between CDC, or Center for Disease Control, and the NIH?

JS: I remember it because Coatney, for example, had been at CDC one time but I had no direct connection with any activity or personnel at Atlanta.

BSP: So some of the staff members, scientists at the NIH were sent to the CDC center in Atlanta or other places?

JS: I don't remember. I know there was one who joined the faculty at the University of Indiana. There was one who did go to CDC carrying with him one of the most valuable laboratory agents that we had in the lab. That was a flock of chickens, which had been raised in the laboratory building for a number of years, but who, by virtue of their genetic make-up, were excellent experimental units for testing new types of antimalarial drugs. And the investigator, Joe Greenberg possessed a laboratory tool which was probably more valuable in tracing of the genetics of the malarial parasite than he was at testing drugs to be used against the disease. And as I said, one went to Rhode Island and I think a couple of the technicians, or maybe three, stayed there in that building.

I was offered a job by two or three members of the extramural program at NIH, one in Cancer, one in the Division of Research Grants which was responsible for collecting and distributing all of the grant applications that were coming in from individuals in colleges, universities, medical schools, medical centers. I settled on a job in the extramural program of the then National Institute of Arthritis and Metabolic Diseases, which appealed to me on the basis of its heavy involvement in chronic disease, arthritis, diabetes, various other metabolic diseases.

BSP: Could you comment on more about changing emphasis from infectious diseases to chronic diseases that were happening at the NIH?

JS: Well, as I said before, the NIH in all its respects, was just beginning to expand. When I joined the staff the clinical center had just been dedicated and was receiving its first patients, I think that was late in 1953, so there was room for a big expansion there in clinical investigation, which hadn't happened before. There had been no appreciable money for competitive research grants, that was just beginning, and there were some fellowships for research training, but relatively little in training grants for research available through the NIH at that time. So everything was expanding and the opportunity seemed almost endless at the time. But those things in my experience tended to expand much faster than the availability of the structure, the personnel and the process, to assume the full capability of operating such a rapidly growth in funding.

BSP: Were there many intramural bench scientists who recruited as extramural program officers like you throughout the NIH, were there many?

JS: Well, for a time because of the shortage of talent external to the NIH, the so-called study sections, which were the initial review groups for the research grants, were often manned by at least two or three intramural scientists. That was eliminated as soon as more outsiders of expertise came along because it could be viewed, although I know of no indication that it was, of a conflict of interest between the intramural and the extramural program. At the same time James Shannon, the legendary Director at NIH, succeeded his predecessor.

BSP: 1955?

JS: Yeah, anyway I'll think of his name.

BSP: Sebrell?

JS: Henry Sebrell.

BSP: Yes.

JS: Shannon came out of the pharmaceutical industry and was recruited to head up the Heart Institute's intramural program when he was brought down from Squibb, and immediately it was obvious that he had a plan and he had the capability and knowledge to put into action as planned; he was a towering figure, both physically and mentally. He had recruited some extraordinary fine scientists, Earl and Thressa Statdman, Bob Berliner. Then he had also sort of stashed away in medical schools promising young investigators whom he would bring to the NIH when the Clinical Center was open, Don Fredrickson and Jim Wyngaarden, both of whom eventually became Directors at NIH. Bob Bowman in instrumentation, Julie Axelrod, a number of first-rate scientists; one or two won the Nobel Prize.

BSP: Right. And James Shannon first came as an Associate Director for Heart Institute.

JS: Right.

BSP: Position commonly called the Scientific Director.

JS: Yes.

BSP: And later on he became Associate Director for NIH in charge of all the intramural programs and then became NIH Director in 1955.

JS: Right, right.

BSP: And that point coincides with your becoming an extramural program officer.

JS: Just about, it was a year earlier that I left the lab.

BSP: Yeah, and do you remember any of Shannon's input upon shaping the extramural program, or just extramural program just growing out of its own momentum or its own system?

JS: Well Shannon induced a number of, I think, far looking axioms for the running of NIH. One was that there be a relatively literal intercourse between, at least at that time, between the intramural and extramural scientists. He wanted the whole operation to be above board with no hint of any possibility of conflict of interest or anything of that sort. He also recruited an extraordinary group of first-rate

scientists, some of whom are still here, many of whom unfortunately are dead. Some left, and many of those who left were what we all called the yellow berets during the Vietnam War.

BSP: Right.

JS: They had served their two years military obligation in uniform at the NIH and had discharged their responsibility solely at NIH and went out into the intramural – or extramural program, usually as members of faculties of major medical schools. Shannon had another farsighted view, which is still invaluable, and that is that he wanted to find a way legally to match the dispersal of extramural funds, that is research grants, graduate training grants, fellowships, and later with construction grants for institutions, with a broad view of building the infrastructure of these institutions; they had to be non-profit organizations at least at first, and they had to be institutions with not only promise, but the ability to spend that money wisely for infrastructure purposes. And that attitude continued throughout his career. A sort of an offshoot of that was a program that was called grants for general research and training, which we always used as intuitional grants; they were sizable grants, block grants as it were, given to the institution as a whole, but with the express idea of building up the quality of the infrastructure, including the faculty, the general purpose and operation of the institution and then that combined with the instruction program when it came along to provide the buildings.

So there were exciting times there where you could see Shannon's vision being realized. He tried one unsuccessful experience, and it was shot down by some of the more influential members of some of the advisory councils. He thought that among the highest tier, in terms of quality and diversity of academic institutions, it should be possible were they given some free money as it were, that is sort of a block grant with no or relatively no restriction on their employment, that they could be advanced to a super level of quality and activity, and then he would identify a second layer based on their grant performance and provide some grants to them in the same fashion. Well since he only had money – he talked about this for quite some time but he only had money for, I think it was at the beginning at least no more than 10 grants and later it expanded a bit beyond that, but it faltered on the greed and the jealousy of the have-nots and so some of his selections created a lot of discourse and dissent, but in his advisory – the academic members of his advisory council, and after I think about three years of that he got that program.

Then he had another program, however, that in a modified fashion did survive, and that was to have the institutions put forward for grant of money promising younger individuals who, if they were successful in competing for one of these limited grants, would be awarded money to make sort of a superstar of the individuals thus selected, and those again were purposely scattered around institutions throughout the country. As I say that was modified in terms of eligibility and so forth later on, but I doubt if it's running now but it was, for a time, and opportunity for a rising young scientist to become better known in the field and to support better work.

BSP: Well I understand – I agree that James Shannon was an extraordinary administrator of the research programs with charisma and with intelligence and a bit of influence in academia and also in the political arena, and within NIH – an extraordinary director I understand. However, he was supported by his staff members. Even though he was such a great person without his supporting cast, staff members; he would not really push for what his ideas or he would not generate many great ideas. I was trying to go the level – go the level beneath an NIH Director who really generated the ideas. One who really supported the NIH Director, through – continually throughout the whole years, and one of the persons that I found very impressive and also important in that role is the Chief of the Office of Research Planning, Charles Kidd came up – the name came up quite often and also there were other names. So could you comment on his immediate staff members and their roles, and if you can remember from this picture, for example, David Price...

JS: Yeah. David was an old time Public Health Service Officer and became eventually Deputy Director before he became Deputy Surgeon General of the Public Health Service. I think I would differ a bit with your conclusion. Shannon was a man of ideas, and of grand ideas, some of which like all grand ideas good or bad, but his idea people were primarily Joe Murtaugh spelled and Chuck (Charles) Kidd.

BSP: Joe Murtaugh is a deputy to Kidd?

JS: No.

BSP: What position was he do you remember?

JS: Joe may have been his deputy. I'm not sure. When I –

BSP: He left around '62 or so –

JS: Yeah, he took a took a leave of absence – took a bit of absence and wrote a book on financing you’ve probably seen that.

BSP: Right that was in ‘57/’56.

JS: Yup.

BSP: And this man was – Murtaugh, was working with Kidd for a while but Kidd just disappeared in the early ‘60s, and then Murtaugh became the idea man as you said, and then at some point Tom Kennedy – Tom Kennedy came to that position. And so, in terms of idea man that generated ideas could you comment on Kidd, Murtaugh, Kennedy and because I found that a lot of articles written under the name of Shannon, or speeches made by Shannon, actually drafted by these people?

JS: Well, and one of his chief speech writers isn’t even pictured here, a fellow by the name of George Payne.

BSP: George Payne.

JS: George was an extraordinary fellow. He had been a newspaper man in Iowa somewhere and George was a man who – excuse me – always kept to his own council, even to the point he had three responsibilities. One was to write the director’s statements before Congress, second was to write the minutes of the institute directors, which met once a week and the third, it was a combination of sort of general handy man, and he used to infuriate Shannon, but he was so good that Shannon overlooked that. For example, a standing joke around there was when Shannon was to make a speech before the – before some body of the Congress, a committee or something like that, at least the day before if not sometimes the morning of the day, there’d be a frantic search all over Building 1 for George Payne. He used to come to work at 11 o’clock in the morning. The only person in the civil – the whole government that I knew who worked on his own schedule. He would be up until 2 o’clock in the morning or something of that sort, but George was able to put in the words, particularly where scientific phrases or words were involved, a layman’s understandable paraphrase of that. Joe –

BSP: He’s not in this picture?

JS: No, George Payne is not. He was was a second or third layer in the director's staff. [Unintelligible]...Mississippi River for something to do before he found a job, and his first job was, what was then the Bureau of the Budget, and in the hospital division of the Public Health Service he used to run a series of at least 10 or 12 hospitals with his own staff located primarily, as a consequence of the old responsibility for seamen. Then he decided he would leave the OMB, maybe it was because of the fact that he could never sell his idea eliminating the Marine hospitals, the public hospital, the hospital for the Public Health Service. They really were an appendage in those old days, so finally he left OMB, or BOB, and then came to NIH. And he had a very quick mind and did a lot of fleshing out of Shannon's proposals.

BSP: Oh I see, so the way that the idea came out is kind of a discussion between Shannon and his staff members, and Shannon, I understand he wasn't particularly articulate.

JS: He was not.

BSP: He was not, according to Earnest Allen Jr., in his memoir, he said that Shannon was an imposing figure but he wasn't really the best speaker.

JS: Oh, he was a terrible speaker.

BSP: Right. So when he's, you know, thrown out some ideas, as you said, Kidd would flesh out into beautiful words that the most ordinary people could understand.

JS: And even then he'd sometimes mess 'em up. Something came up in OD staff and there was a lot of grumbling about it, understand the propositions. And a group of us were standing outside his office chatting about this, the pros and cons of his proposal, which obviously he's going to continue to push regardless of what we thought, and one of the group stopped the meeting immediately and we all dispersed and he said, "Listen you guys, around here Shannon's the honest to Christ boss." And nobody ever forgot that. He scared a lot of people. He never scared me because it was always a good relationship.

BSP: So in 1964, you directly succeeded David Price or there was a man between –

JS: No. I can't remember when Dave – he retired. Well first of all, he went to be Deputy Surgeon General, and then he retired and he went over to Hopkins School of Public Health.

BSP: Mm-hmm.

JS: His successor was a fellow by the name of Stuart Sessoms, an MD.

BSP: Right.

JS: And Stu was a wonderful foil for Shannon because he would do things that Shannon would try like the devil to find an excuse not to do himself, and so Stu had the responsibility of giving out the bad news, although Shannon was capable of doing it. Another period, well Bo Mider headed up what used to be the Laboratory of – oh what was the title, Chief of Laboratory –

BSP: Director in Charge of Laboratories?

JS: Yeah, and Clinics, I think that was it. It wasn't Deputy Director of Science, but it was essentially that position. But this fellow here, Jack Masur.

BSP: Oh yes.

JS: Jack was again a remarkable person. He had, I can't remember the sequence, but at one time he had been head of the Public Health Service Marine Hospital Division. Then he'd been in civilian life, although it might have been the other way around, at Montefiore Hospital in New York, and he was the only one – he was the only Public Health's officer ever elected to be President of the American Hospital Association, and a fine, fine person. I have a little anecdote to kick off the – detract from that. But I can't remember the year. Jack had taken a drop in rank and pay to come to the NIH as head of the Clinical Center, and Jack and I were always very close friends. Shannon called the two of us in his office one day and in typical fashion, without any introduction or flowery language or anything, he said, said to the two of us, "Jack and John, I have a star to award," you know a rank. And he said, "I've decided to give it to John because he needs the money most." Well I was a lot older but because he had some senior [unintelligible] me. And less than an hour later, I was in my office in Building 1 and a package came addressed to me. Out of curiosity I opened it up and here were the two stars.

Ernest Allen, together with originally Dave Price, and Dave's predecessor, CJ Van Slyke as Deputy Director of NIH, were responsible for getting the grant program off the ground, and it was a first-rate operation, but with the understanding, we weren't fixed in stone. But I remember after the Nixon administration came in and they went over us with a narrow toothed comb

wanting to find faults, and they realized we didn't have a manual of regulations or anything.

BSP: Right.

JS: So they said, "You better make one." I said – I'm a Deputy Director at the time, or maybe Deputy Associate Director for Extramural Programs, I said to Ernest, "Gosh, we've run this grant program successfully for a number of years and never realized we had to have a manual regulation." And Bo Mider of course ran the intramural program, a superb scientist, a wonderful friend. When he retired from the Library of Medicine after having gone from NIH over to the Library because of a heart condition. He had asked me to give his farewell speech at the banquet. And I said to Marty Cummings, the Director, "Man, Marty. I can't do justice to what Bo deserves." And Marty said, "You've got to do it; Bo wants it." Well I got the wonderful idea of asking my secretary to call about fifteen people and just say what's the first thing that comes to mind when the name Bo Mider appears. And I got the most wonderful series of commentaries, most of them different, and stitched them together with little pieces and it worked wonderfully well.

BSP: That's great.

JS: He had a myriad of nicknames, Bo-Bo, No-Bo, Dr. No, I don't know how many others we identified, but just a remarkable person. So the whole staff you know, Cliff Johnson, he was our head of communications at the time.

BSP: Oh, I see.

JS: We really didn't worry much about that. Dick Seggal, head of administration just died a couple of weeks ago.

BSP: Oh.

JS: He had training in the Bureau of the Budget.

BSP: Could you comment on the relationship between Division of Research Grants and each individual institutes, extramural program, how it worked in terms of handling the grants. It's a bit complicated because it's changing all the time, right?

JS: Mm-hmm. Well, and we were experimenting all the time then, but there were some things that were constants. For example, we developed a combined

application form, we developed a schedule by which all of them would be reviewed regardless of what institute was involved. We insisted at first that all the applications be reviewed by the non-partial research grant study centers, and then we had these weekly meetings with all of the associate directors from the individual institutes together, and we'd hash out problems where somebody had brought something up and sometimes it was a sharp diversity of opinion between one institute and another. And we usually, by a process of negotiation, were able to reach a consensus and make it go. In later years, as the training grants grew and more particularly as the so-called center grants came on the scene and grew with the big monies that were at stake, there was a division that never really was resolved between the Division of Research Grants and the Extramural Program Directors, to let the Institute set up their own review committees for those center grants, and that caused a lot of great satisfaction in the individual institutes with a great deal of criticism otherwise.

BSP: I see, as opposed to study sections.

JS: Yeah, right. I don't think that any serious damage was done by it, but it does split the comity of the whole operation and it just took more people, so a larger investment was had.

BSP: Well I understand that since NIH was primarily structured around the categories of diseases, you know, heart, cancer, arthritis, and so on, but the scientific activities are primarily organized around disciplines, physics, chemistry, biochemistry, molecular biology.

JS: Right.

BSP: And so there was some areas of overlapping and areas of repetition, and at the same time areas that were not really covered, and so there was some complaints and contentions about NIH operations. I understand that Dr. Shannon was in defense of the current categorical structure because it has a lot of strengths and advantages in bringing more money from the public. And I'm curious how working at the extramural program, how you tried to resolve some of the conflicts, some of the problems that arose out of categorical, so-called categorical structure at the NIH.

JS: Yeah, sure. Well first of all, when the grants program first started, there was always a general catchall study section, and if you didn't know where to put an application as far as its categorical relationship, you tossed it into this catchall operation. And at that time DRG actually had a program, a grants program of its

own, it had some of its own money and they could make awards. That privilege was taken from them, oh I'd say in late '50s, early '60s. Well, then the next thing that happened after that, in part driven by that challenge and in part by the fact that the financial situation of the institutes was very uneven, one institute that had a fair amount of grant money left over and another institute that was begging for money to award even a few grants, that was particularly true then with Mental Health; they didn't get much money and they had a lot of applications. So what we did was to form an arrangement where those types were again sent back to the Division of Research Grants. An attempt was made, first of all, to put them into what might be sort of an uncomfortable location, uncomfortable in their relationship to the main body of that particular study section, or they would reassign it to another institute or, in rare cases, the two institutes would divvy up the cost, share the cost.

BSP: Mmm-hmm.

JS: So there were a number of things of that sort. I frankly don't know what they do now. It still might be a problem, but they had done valiant attempts to add study sections, reduce study sections.

BSP: Who decided to create new study sections, who decided?

JS: Well it's usually done between the Division of Research Grant, I can't remember the present name of it. The Chief of that and his staff – his or her staff.

BSP: Oh I see.

JS: And the Director's staff. Rarely at that level is there any interference from Human Public Health Service.

BSP: I see.

JS: One of the things was that Ernest Allen, when the Nixon administration came in, was elevated to certain general staff at the Public Health Service level, and he was able to, he was a great conciliator. And so he took care of a lot of issues that that chart came up even though he didn't have any money, strictly his personal ability to do it. In fact he had such experience in the Division of Research Grants itself.

BSP: So DRG was primarily in charge of running the study sections and having peer review and then the peer reviewed proposals were sent over to the Advisory Council meetings at NIAMD and at NINDB, and also as I, also as the Director for

NIH you probably participated in lots of Advisory Council meetings, and could you say what you [unintelligible], how that [unintelligible] discussion at the table?

JS: Well there was always a fair amount of – I wouldn't say overt antagonism, but more just a strong willed competition between the institute directors on their grants program and my job, in the council meeting arena, was usually to take the director's message. Particularly when he knew they weren't going to like what he was going to say and then, as a consequence, I didn't participate in any of the discussions on individual grants. That might be done by the associate director of that institute but not by the associate director of NIH for that purpose.

At the study section level the study sections usually lasted two, two-and-a-half days. These were in operation three times a year. It was almost expected that there would be a staff person from the institute related to a particular application who would be there as an observer and presumably could answer questions from a council member, wherein the executive secretary of the study section wasn't that informed sometimes about the background of a particular proposal. And likewise, when it came to council meetings, the associate director of the institute usually sat in on the – I don't know of an instance when he didn't sit in – on a council meeting to outline any general policies of NIH or the institute that were being under consideration for modification or change in some fashion. The councils in that context were quite passive in nature. Once in a while they would revolt, but it was usually on an important issue and for a pretty good reason. And the council's were made up mostly of professional people but they did have names that would appear out of the blue from the line.

BSP: Blue?

JS: You know, not certain where they came from.

BSP: Oh, out of the blue.

JS: The bigger institutes particularly, Heart, Cancer and Mental Health were most likely to have a Frank Sinatra put on or some Hollywood star.

BSP: Oh really.

JS: You never knew when that was going to happen and there wasn't much you could do to stop it, but it was balanced by the fact that there was always a majority of non – of professional types.

BSP: So how – how – the issue was resolved when the laypeople expressed some enthusiasm or expressed some urgencies that we have to conquer this particular kind of disease and the experts have no answer at that point, and how the different perspectives were resolved? Do you remember?

JS: Well it varied considerably. Some of them were for very astute business men and they weren't about to suffer a put down on something that they proposed or questioned. On the other hand, you'd get a bunch of flattering women sometimes – not a bunch, but one or two – that's all you needed. I remember a wonderful woman who was put on the Arthritis council. Her name was Florence Mahoney. She was a protégé – she'd be insulted by that, but she was a confidant of Mary Lasker, Florence's interest were more in the area of mental health and in child health, and the first time I – the first council Florence was appointed to was the arthritis council. I was relatively new on the staff and I always used to brief the council meeting on the day before the meeting started to let them know how the whole system operated. And so I went down to the front door of Building 4 where we were going to meet, and I waited and waited and waited and the only person that came in was this old gray haired woman – my wife doesn't think she was gray haired, but anyway – she was an older woman and she carried with her a thermos bottle.

BSP: Lunchbox?

JS: Rectangle type of bag. Well nobody else had come along and I finally decided maybe this was Florence Mahoney whom I was supposed to meet. Sure enough it was. She'd driven up in a beat up old car. Florence would come into a meeting and you could always expect one major non sequitur from her.

BSP: One major...?

JS: Non sequitur, where she'd throw out something that had absolutely no relevance to the discussion that was underway. And next to her on this particular occasion were seated two big fellows, Carl Moore, who was the very well known and respected and a fine chairman of medicine at Washington University, St Louis. On the other side of her was a fellow by the name of Charlie Reagan who was a leading rheumatologist on the staff at – I can't remember if it was Cornell or College of Physicians and Surgeons at Columbia. Well they had – one or the other of them had smoked a cigar on the previous day in the meeting and she took violent exception to that. So that afternoon after lunch they both lit up the biggest cigars you could imagine and sat there, you know, in utter innocence and Florence

got up and started opening all the windows in the room, but she certainly deserves credit for finally getting the legislation for the National Institute of Aging passed.

BSP: And you know Florence Mahoney and other laypeople were engaged in deciding, making a final decision on the grant proposals.

JS: The whole council.

BSP: The whole council and in your impression laypeople's input really changed a lot of the ratings –

JS: I wouldn't say a lot.

BSP: Could you...?

JS: I would say some.

BSP: Some, say 10% or less than 10%.

JS: I wouldn't say more than 10%.

BSP: So when the case was reversed or changed usually the laypeople – how do the laypeople argue for specific proposals?

JS: Well many of them, you know, were on there not as pure political appointees but rather as representatives, as it were, or promoters of individual diseases or organizations related to a given disease and so they had some good working knowledge, some of them did – Florence certainly did – of what she was talking about. And so even though she wasn't a professional she commanded attention of other members of the council.

BSP: Well thank you I'm going to stop for this tape.

JS: All right.

BSP: This is the third tape of the oral history interview with Dr. John Sherman. We are still at Dr. Sherman's house in Rockville, Maryland. Dr. Sherman, we covered lots of subjects and in the last tape we talked about Florence Mahoney and the role of laypeople in the advisory committee. Do you have direct interaction with Mary Lasker, you know, another very well known layperson?

JS: Yes, but nearly as much as with Mrs. Mahoney. There was a casual conversation here and there. I remember when Bob Petersdorf became the President of the Association of the American Medical Colleges, by that time I was there.

BSP: What year, do you remember?

JS: That would have been '78/'79 somewhere in there. I left NIH in '74, but I set up an appointment with Bob who was formally Director of Internal Medicine at the University of Washington Seattle, and then he'd become Dean, I think, of the Medical School of San Diego. Bob had no experience whatsoever in the political world and you had to have a watchful at least, if not special talent, to deal with Mary Lasker and Bob was a – he could sometimes be either disdainful of others in high places or he could shoot from the hip, and so I decided I'd better set up an appointment with Lasker, which I did, and we went up there and it was an experience in itself, you know, with her full window-to-window carpeting and white fur and all that thing [laugh], but I didn't have that – at time when I did have was when I was Deputy Director, but they were intermittent and, as far as I was concerned, inconsequential.

BSP: One historian who examined the role of Mary Lasker and other laypeople in shaping NIH policies said that their relationship with Shannon and the NIH directorship, sometimes very helpful and sometimes very mixed and sometimes not very helpful, and could you comment on the how Shannon dealt with Mary Lasker and Florence Mahoney and other people, as far as you remember?

JS: Well, in the first place, when the relationship first started it was all quite cordial, particularly between Mary and Jim, because they had the same ends and the same thoughts as to how to accomplish those ends. Later on the relationship, especially with Mary Lasker, became very strained because Mary had different ideas as to how to run the place. For example, the two of them had a big fight, verbally, over who should have the authority to award contracts in addition to grants and so forth. This was particularly true in the Cancer Institute where they were running many of their cancer chemotherapy programs and development of chemotherapy agents under contract, and that was not an unreasonable way to get it done, and Mary thought that just because the councils reviewed contracts they ought to have the authority to grants, they ought to have the authority to review contracts and Shannon really drew the line there, because it's in the law that contracts from the federal government have to be decided by federal employees.

BSP: So who reviewed the contract?

JS: They were done by panels.

JS: Panels within the NCI?

JS: Yeah.

BSP: I see mostly internal group of extramural –

JS: Yeah, they had a separate group of contracting officers –

BSP: Oh I see.

JS: Just as they do in DoD and other places, and Mary didn't like that and she took that first of all to John Gardner. Gardner rightly backed Shannon, and then she went to Lister Hill. Her purpose in going to Lister Hill was to get a law passed that would require the NIH councils to review contracts as well as grants and fellowships. Shannon, boy, he really rose up in righteous wrath and he went to Hill, with whom he had a superb relationship, and explained the thing to him and that was resolved in favor of Shannon and NIH. Then it wasn't long after that I think Mary had persuaded Lister Hill to do the first overall thorough review the NIH, and Shannon and Jerry Wiesner, who at the time was head of the office in the White House – I can't remember which.

BSP: Scientific Advisory Council, President's Scientific –

JS: Yeah something like that. Jerry was sympathetic to Shannon that it not be a politically inspired whitewash or a criticism of NIH, and Shannon and Wiesner convinced Senator Hill. Who was already to put this other approach – Mary's approach – into law, or at least into legislation, agreed with the position that Wiesner, Shannon and Gardner took to have a first review of NIH but not to get into all of this argument about contracts. Well out of that came the famous Wooldridge Committee; I don't know if you've heard that.

BSP: Yeah I'm familiar with that.

JS: And that was a remarkable group of people and they – I remember their most prominent and enlightened recommendation was as far as the American people were concerned they were getting the best money for their dollar that they could imagine and went on to praise practically everything. The one thing they couldn't – they disagreed on with Shannon, and he and Weisner just couldn't get it out, was this, the establishment of this outside committee, which would be in effect

away from but above the councils and would look at NIH as a whole, and that was one distinguished group of people.

BSP: So the Advisory Committee to the Director was recommended by the Wooldridge Committee in 1965.

JS: This was the committee that was set-up –

BSP: Yeah set-up.

JS: In response to that.

BSP: And James Shannon was opposed to that initially?

JS: He had those councils that gave him enough trouble. He didn't want this other group, but he was able to handpick the members in consultation with Wiesner and with the Secretary so it came out all right. I don't know even know whether it still exists. Marjorie Wilson and Tom Kennedy were the staff. Bob Marston later became director of NIH, Bob Berliner who was sort of representing the intramural program and he was by that time, I think, his title had changed to Deputy Director for Science. I was on it. Maurice Hickey. I can't remember who was. Henry Riecken was a really well known sociologist from the University of Pennsylvania. Bruce Woodhall [spelled phonetically] was a Chairman of Surgery at one of the southeastern schools. McElroy was head of the science foundation. Wiesner, Irving London was head of a program; he'd been chairman of medicine up in the northeast somewhere and he became head of the combined program between Harvard and MIT on medical biomechanics, biomedicine I guess is the best way to put it. Phil Cohen was long time Chairman of Biochemistry at Chicago I believe and –

BSP: So this group is composed of mostly the professional scientists?

JS: Yeah, yeah.

BSP: There's no laymen?

JS: No.

BSP: No businessmen? Mostly academician and NIH staff.

JS: Yup.

BSP: I see. So what did they do as opposed to the council –

JS: Well, the one thing that they did do that bothered Shannon was to say that they ought to meet at least – I can't remember whether it was once every six months or something like that. It wasn't that onerous a task, but a lot of reports had to be prepared and so forth and so on, and the other thing was to give good cover to the NIH to the thorough examination that had been and it was a worthwhile operation.

BSP: Do you remember any particular recommendations made by this group of people in terms of policy and the planning?

JS: No, not specifically.

BSP: So basically some ideas and –

JS: I think they were sort of reactive to what Shannon might propose.

BSP: Oh, I see. So as a sounding board for approval for disapproving some of the ideas proposed by the Shannon staff?

JS: Yeah, each institute does now have an advisory committee, which is kind of separate from the council; it has no lay-members on it. So it's sort of an outgrowth of this.

BSP: I see and before that you know there in each institution – each institute has its own council. The Surgeon General has also the general council called the National Health Advisory Council and so I figured up to this point NIH Director has no – his own council so I thought that Shannon would welcome the arrival, the creation of his own council to present his own ideas and get some support and, you know, because they're all professional experts. That it is a more effective way of communicating with the academic community.

JS: Well he did to some extent but it was out usually on an individual basis and essentially he used his own staff because he had confidence in them and he'd selected all of them, and he didn't just see the need for another group of outsiders to come waste their time and his time.

BSP: Could you going back to Wooldridge Report? Do you remember anything that the Woodbridge Report changed other than this creation of this committee?

JS: That was the primary recommendation. I think it may have been – do you have access to a copy.

BSP: Yeah, I read it –

JS: I can –

BSP: I read that report –

JS: I have one here if you want it.

BSP: And also I read preliminary responses made by NIH. Jim Shannon generally said “that this is a good recommendation but on specific points I would disagree”, and one of the points that he disagreed was about the kind of research programs that the intramural laboratories had to conduct because the Wooldridge Report recommended that NIH laboratories should be devoted to long term risky projects. Other university type research can be done – could be done by university scientists. So giving it away to – those kinds of research to the university scientists whereas the NIH intramural program should focus on the so-called federal government type of research, and Shannon once said that well, if the university scientists do not have interest in a specific research then it isn’t really worth it. He wanted – my impression was that he wanted to keep the intramural program more like a university, even though there may be some overlapping areas, it doesn’t really matter to maintain the high quality and maintain the excellence of research. And so that encouraged the intramural scientist to compete with a scientist outside in terms of the quality of their products. So that then was some point of disagreement between the Wooldridge Report and Shannon.

JS: Mm-hmm. Yeah, I had forgotten that one, but I remember it now that you mention it, but on the whole it was a glowing report. It was an elaborate study, as you probably saw from the book; they had panels all over the place.

BSP: Oh yeah. Before Wooldridge Report, there was a Fountain Committee Report.

JS: Yeah.

BSP: And so do you remember it?

JS: I sure do, I testified the first day of that.

BSP: Oh, really?

JS: I was trying to remember what the primary issue was.

BSP: The management of the grants?

JS: Well, it started out with the basis on the fact that the grant was at – one of the institutes had made a grant to a for-profit organization somewhere out in Connecticut and had given them indirect costs on it, and Fountain didn't think that was right. And Fountain had a staff assistant by the name of Delphis Goldberg, I'll never forget him as long as I'm alive. And he jumped on that saying, "How come you can say that the government will never make a mistake in doing something, and why is it you're now opening up profit making companies to indirect costs as well as direct costs." And that went on for a while, and Fountain's committee staff digging more and more into the granting process, and it became not only a pain in the neck because they were on us all the time about getting finished up or asking all sorts of questions or providing all sorts of data, and I was trying to think what prompted the questioning the day I was a witness. And I'll be darned if I can remember, but that had the potential of doing great harm at NIH. And again, Shannon has his reasons for thinking that this is an exceptional case that merited the indirect cost, and he told his boss. So that was an unhappy period.

BSP: Well, in terms of unhappy periods, when President Johnson –

JS: Went out to the NIH.

BSP: Yeah, went out to NIH, called all the NIH people to White House, the Institute Directors to White House in 1966, and pressuring them to produce more tangible results and being committed to more targeted research, and I understand that NIH Directors came out with a shock. And then the relationship going on between the administration at the NIH, and the next year Johnson came to NIH campus to console some of the concerns expressed by the scientist, and did you go to the White House?

JS: Yeah.

BSP: Do you, could you describe the tensions?

JS: Well in the first place, the whole thing had been started by Mary Lasker.

BSP: Right.

JS: And she had insisted that there was no movement of findings from the laboratory shelf to application, and so she got a hold of President Johnson and insisted that he pressure the department and NIH to turn their program towards application of new ideas rather than just the development of additional new ideas. And so there was first, as you say, the White House conference where the admonition was administered. After that was this visit to NIH and there's this famous picture of the President and the Surgeon General, Shannon and Phil Lee, the Assistant Secretary for Health. Behind the four of them, Shannon is walking up and he's the only one that wasn't in step, and the famous caption of the picture was, "Shannon always marches to a different drum." But it didn't – there was some companion legislation passed the Birch Bayh – the Bayh-Dole Act to investigate this question of inventions developed by a federal laboratory lying on the shelf and not being put to application.

BSP: That's 1980? Bayh-Dole Act.

JS: Yeah, about that. By that time I'd moved on to AAMC, but I remember testifying about it. The schools hated it and I remember I introduced my testimony by saying – I was the last one on the witness list, all the rest of them had been academic types and scientists and so forth. I said, "I guess I feel like a skunk at a wedding party." I was opposing the legislation that they had. I guess it's probably made some difference. There was more rapid movement of more research findings in industry.

BSP: Right. Well, as part of applying current knowledge in biomedicine to the cure and prevention of diseases, in the late 1960's there were efforts to create, establish regional medical centers?

JS: Regional Medical – it was the Regional Medical Program.

BSP: Regional Medical Programs.

JS: With the Regional Medical Center.

BSP: Right, right.

JS: As a matter of fact, Bob Marston, who was recruited to head up that program, manage it, and then later become Director of NIH after Shannon left.

BSP: Well, I just want to discuss the transition from Shannon – Shannon’s era to Marston’s era, and you’ve just mentioned that he was selected as NIH Director primarily because of the Regional Medical Program, or other things?

JS: I think it’s fair to say it was primary because of the showing he’d made in that. The big difficulty was that Wilbur Cohen was the Secretary of HEW at the time, and he proposed to shut down these Regional Medical Centers and Shannon objected to that, and in one of his rare indiscretions Shannon said, “I’ll quit before I’ll do that.” Well to somebody like Wilbur Cohen and anybody else in that position, you would do that once, but that’s all.

And so it was obviously time, as far as Wilbur Cohen was concerned, to replace Shannon and we went for a few months there. I think with an acting director and then Bob was appointed. But Shannon changed his mind; he would have like to have stayed for another year or so, but it wasn’t his choice.

BSP: And by that time, if I’m not incorrect, NIH was separated from Public Health Service and becoming directly under the Assistant Secretary for HEW, is that, do you remember anything related to that arrangement?

JS: I don’t remember that we were actually separated. We may have been, but I don’t recall it.

BSP: I mean, not – maybe I’m wrong, but you know, before that NIH was part of one of the divisions of Public Health Service.

JS: Right, right.

BSP: So the NIH Director has to report directly to the Surgeon General, and around that time it was decided to bypass the Surgeon General and directly report to the Assistant Secretary, all NIH. I have to check the organizational chart, but there were some change in the relationship between Surgeon General –

JS: Well that I remember. There was a shake up there because it was about, I think it was about that time, that they eliminated division hospitals. They switched around the other two majors of Public Health Service, what was that, Bureau of State Services and Bureau of Medical Services, and I can’t remember exactly what did happen but as far as day-to-day operation of NIH, it had no particular impact. We lost the Institute of Mental Health for a while.

BSP: Right.

JS: And then that came back a year or so later.

BSP: What is the reason to decide to take away the Mental Health Institute.

JS: Well the mental health community didn't think they were getting enough attention under NIH's leadership.

BSP: Is that true?

JS: Well it – I wouldn't say it was true. They had a couple of very able directors at the Institute. Bob Felix was sort of a legendary Director, and he and Ken Endicott as head of the Cancer Institute were constant thorns in Shannon's side; they always wanted more than he could give them. And of course the Cancer Institute finally got partway out, but I don't remember the Mental Health Institute in contrast to the National Institute of Mental Health leaving the NIH, but it may have at that time. But I suppose it doesn't make much sense for the Institute to leave and not to have the function removed.

BSP: Around that time, you know, Environmental Health Sciences Institute was also created. Do you remember any of the, you know, basically how the new institute was created, who pushed it, was it an idea suggested by the Shannon's staff member of just from outside?

JS: I think it came from outside.

BSP: I see.

JS: There really hadn't, he hadn't been paid that much attention to environmental issues per se before that, and I'm quite certain that it was the clamoring of the public and the Congress of the environmental health forces that moved that out and established it. The general mood of NIH was not to establish additional institutes. I remember one time going down to testify on Florence Mahoney's patent legislation that took forever to get through, and every time we'd come up – every time it would come up we'd go down and testify against it. It was always on the ground of the fact that we don't need another institute. It was getting so big and wide, you know, that for administrative purposes, it was pretty hard to control.

BSP: Mm-hmm.

JS: Even Harold Varmus, you know, thought that it ought to be consolidated; others did too. Well I remember Ted Kennedy was chairing a committee meeting that day and he said to me, “Well, what would you do, Sherman, if you awakened some morning and find out that we’d passed legislation that established the National Institute on Aging?” And I said, “Straight forward, well, we’d make the best effort and best institute we possibly could.”

I had a minor at the time, Deputy Assistant Secretary for Health, who was a dentist at or something like that. I thought he was going to fall over in a dead faint because the strategy was to just say, “No, we don’t need it. We can do it with what we have.” And I knew we fought the thing long enough that sooner or later it was going to pass and then we could probably handle it, and we couldn’t do it well in the present circumstance.

BSP: Mm-hmm.

JS: But I don’t think Kennedy was shocked, but John Zapp, the minor from the department, slumped down in his chair; it wasn’t the answer he was supposed to give.

BSP: We talked about James Shannon pretty much, and after Robert Marston, the director Robert Stone was a different kind of director, I understand. Could you describe how he was received by NIH community and by the general scientific and medical communities, and your personal impression or assessment, because you very closely worked with?

JS: Mm-hmm. Well, in the first place nobody knew him. In the second place, as you said, he didn’t have any background in biomedical research except the little experience with the Atomic Bomb Commission in Italy – or in Japan. And he was a pretty stiff character who’d come out of the Sloan School at Harvard. Stone was fired right after the first year, 1973.

BSP: Mm-hmm.

JS: And I had a call that day from Elliot Richardson and Elliot said, “John – ”

BSP: Who is Elliot?

JS: He was Secretary of HEW. He said, “John you know we – ” no he didn’t say “we.” He said, “Robert Stone has been fired and we’re making you acting director of NIH.” Well, I always admired and got along very well with Elliot and

also Frank Carlucci, his deputy, who'd been a holdover from some time before that, and we were then introduced to Bob Stone and he didn't know exactly he was supposed to do other than we've heard, and it's pure rumor, that he was given orders by the Assistant Secretary of Health which was Charlie Edwards that one of his tasks in addition to "straightening out" NIH was to get rid of three fellows out there Kennedy and Berliner –

BSP: Kennedy?

JS: Berliner and Sherman, and all the evidence suggested that it's probably true. Well, we were told by Weinberger, who by that time was the Secretary, and by Charlie Edwards for whom I have very little respect, he was sort of a dilettante as far as I was concerned, that they would have a new director appointed in short order. Well came one month, and then two months and nothing happened, and Bob Berliner came to me one day and he said, "John, I just can't wait any longer; I have these offers." The most attractive one is to be the successor to Julius Comroe, who is a very famous biomedical investigator at Scripps Institute in San Francisco – not the only oceanographic but, you know – and the other is to go back to Yale from which he graduated and be a dean. And he said, "I've got to – these people are pressuring me. I've got to make a decision."

So I said to Bob, "Okay let's go down and talk to Charlie," meaning Charlie Edwards and we met in Charlie's office and there was a fellow by the name of Harry Simmons; he had just taken on a deputy, sort of a yes man, and I right away said, "Charlie we've been told that we're going to get a new director out there, but it's gotten pretty serious and Bob has these two jobs and wants to accept one or the other if we're not going to get a director satisfactory to the staff," and he just sort of nodded his head and I said, "You know if people start leaving out there it's going to be a hard on a pretty substantial institution." And he tossed off very lightly, "Oh John, I don't think it'll hurt NIH to suffer a while." I said to Bob, "Come on Bob, let's get out of here, we're not doing any good." And about a month after that was when Bob Stone was appointed director. Well, I didn't get on with him from the start we had a – we were on speaking terms when I was working, but I wasn't happy.

I remember coming home several times to my wife complaining that this wasn't for me, and there was general unrest and there were all sorts of writings in the press about it including one letter to the editor from I think 500 scientists at the NIH protesting my being let go. I still have a copy of it. So in March – I know it was in the annual meeting of the AAMC in the fall 1974 – '73 the president of the AAMC and Charlie Sprague from the University of Texas Southwestern in

Dallas, and Dan Tosteson who was dean at Harvard at the time and they were the two senior officers at the AAMC, and they called me up and they said, “We want you to shore up our front office and accept the job of vice president.” I’d known both of them for a long time, and interestingly enough one of the other deans had come up to me the day before and he said, “John, if you don’t start taking an offer from some of these that have been made you’re going to begin to lose out on the opportunity.” I thought of that and I thought of this offer and what I’d be leaving behind. So I accepted the position and left in –

BSP: What was the particular point at where you didn’t really feel comfortable with Stone?

JS: Well one of it was personality.

BSP: Personality.

JS: We had a real personality clash, and another was he wanted to make a lot of changes there and they ranged all the way from something he learned at the Sloan School; he had a big round conference table so that he could sit in the middle of one side instead of down the end in the traditional position of the director of NIH for institute directors meetings – staff meetings. And then he wanted to change – he wanted to follow the old Mary Lasker rule of pushing research that had already been proven to application, and I don’t know, there are a lot of little things like that any one of which we could’ve lived with probably, I think, but the totality was such to make it an unhappy place. Well, about a year and a half later he was fired.

BSP: Right, do you know why?

JS: Yeah, he began to see the NIH way of doing business and doing it and he thought that was better than what he had been told to do.

BSP: So he became a person like – he loved the NIH way of doing business.

JS: Yeah. He got cursed with Charlie Edwards, blue-eyed boy, and I’m sure Bob would kill Charlie Edwards the first chance. He went out to Texas as dean of the brand new school at Texas A&M Medical school at Temple, Texas and I remember word got back to us somehow that somebody asked him how he liked it there and he said, “Well, they like me.”

BSP: Did you get along very well with Marston? Marston was well received by NIH people?

JS: Yeah I think most people thought very highly of him. He was a radically different personality than Shannon. He was more willing to find compromises, but there were some propositions when he we would do it where Shannon would, you know, sometimes go to the bitter end before he'd change, but Bob was a strong director.

BSP: Bob Stone?

JS: No Bob Marston.

BSP: Bob Marston. Huh-uh and he was fired by Nixon because of...?

JS: Well it was one of those things where – it's not unusual. Well I have to start back a ways. In 1972 there came out this report from the Congress under the aegis of Ralph Yarborough, who was a senior senator from Texas at the time and a great friend of Mary Lasker, and she had gotten him to set up this great crusade in cancer, and she had a number of famous people on it. And they made one major mistake as far as the Congress is concerned. Paul Rogers was the chairman of the subcommittee in Florida – the chairman of the committee and lived in Florida, was told by John Cooper, the director of AAMC that their proposal to separate the Cancer Institute from the rest of NIH was bad for science and bad for cancer research. And they had a fellow cancer researcher by the name of Solomon Garb, and I can't remember – he lived in Cincinnati I think or somewhere up there, and he had the effrontery to plaster Paul Roger's district with scurrilous ads rejecting Paul Roger's re-election. Well that made Paul very mad. So when it came up for a vote, Paul had listened to people like Cooper on the outside, listened to some of us on the inside, that this was a bad piece of legislation that ought to be defeated, and by that time – by that time the bill had failed in the House by one vote – in the Senate by one vote and so the burden was on Paul Rogers to lead the fight against this if it were going to be defeated. And he did. He took it on by himself, and I think there were three other people finally who voted against it.

Well, that then put the pressure on the White House by Mary Lasker to find some compromise, and they did. Marston said, "You know, I for a while there was walking on a slippery surface and slipping on every step I took about the legislation," because the administration was trying to change his position to find a way to pass the legislation without offending everybody else, the scientific community and everybody else. As a consequence of that it was defeated and

Marston – I don't think he was singled out. I think it was one of those mass you all send your resignations if you're a presidential appointee. Well, the directorship of the NIH had just become a presidential appointee by virtue of the fact that the legislation that did pass that made the Cancer Institute a presidential appointee, and you couldn't have a presidential appointee here with a non-presidential appointee above, so they made Marston and director of NIH a presidential appointee. Well out comes this letter from the White House: "Resign your position," and Bob, I think in a weak moment, decided like everybody else he should send in that paper of resignation.

Well I actually went in and took the paper out of the Secretary's table, which may be an illegal act, and his wife, Bob's wife, finally came over and said, "Fellows –," talking to Berliner and a couple of the others on the senior staff and myself, "Bob thinks he should turn in that letter, so I hope you'll let him do it." So there wasn't anything we could do but let him turn it in and sure enough he was fired the next day, but he wasn't – as I say he wasn't singled out. A lot of people who had been then a long time who were political appointees were given the same letter, expected the same result.

BSP: Was it a shock to the NIH community? How could you describe Bob?

JS: Well everybody was stunned. Here was the first time in history that a director of NIH had been relieved for political reasons.

BSP: In connection with the 1971 the War on Cancer Act, you just mentioned the background of the legislative efforts. How the efforts to separate NCI from NIH, but at that time the economy wasn't good. And the NIH budget was slowing down considerably and the intramural program, intramural scientists began to leave to academic positions. The general mood was not that good and NCI got the most money, and the other – the Heart Institute could survive without its own legislation, but smaller institutes felt cornered in terms of maintaining their programs. Do you have any observations of how NCI, the War on Cancer Act, the National Cancer Act changed the atmosphere within NIH?

JS: Well, in the first place there were three provisions in the bill that finally passed that were significant. One was that the budget for the Cancer Institute should bypass any review by the NIH; it should be reviewed by the Office of the President and that presumably gave them a carte blanche to up that appropriation substantially. I don't think it – after perhaps a year or so I don't think it really made any difference except it introduced another step in the process. The second thing was the Director was a Presidential appointee, and so theoretically he didn't

have to pay any attention to the director of NIH for running this place still within the NIH. Then the third was they probably did profit by more money they would have gotten in the appropriation, but I don't think that lasted that long either, maybe two or three years. The general reaction within NIH was one of resentment. First of all this could; and I still believe it myself, tear the whole biomedical apparatus apart, because something of value made over here has application here, and the NIH has been predicated with the categorical orientation of the Institute, on covering the whole field of biomedical research. Therefore, it did cause a lot of resentment.

BSP: And at the time the Fellowship and Training Program was cut down, do you remember?

JS: Well that was more, I think, a consequence of the Vietnam War. We had a couple of years there around 1960 when our budget essentially was flat and then the usual way of portraying is that you've got a cut. It's a cut in the increase, it's not cut in the actual amount, but I don't think there's any direct connection between the Cancer Institute situation and that cut in budget; I think that was an entirely war related. I remember one time calling a fellow by the name of Bennet [spelled phonetically] who was in OMB at the time, Bureau of the Budget, and saying, "Come on –," I can't remember what his first name is, "level with me here. Is this – are we being singled out here, or is this a general cut and we can hope for a restoration sometime," and he assured me and I believed him that it was general cut of a temporary nature. So that wasn't a problem, but it does bring to mind one more thing about Shannon's reign in –

JS: In 1960 or '61 Shannon became worried about the quality of grant applications that were being approved and funded. Every once in a while he would look over a batch of applications at random to see whether it was good science, and on this occasion he had done that and much to everybody's dismay, both in the institutes and in the community as a whole, they just hollered to high heaven when he said I'm not going to pay any application – make any grant with a priority of 400 [?] or poorer." We returned \$100 million back to the Treasury. I think, as I recall, he did that two years. He just wasn't satisfied with the quality of what he was seeing. Even though these applications, I can't remember how many applications were actually involved, but even though they had passed some other muster with a poorer priority score, he had said just flatly, "A hundred or anything below that is rejected and the excess money goes back to the Treasury." I thought that was a pretty courageous and wise move on his part.

BSP: So compared to Shannon's era in the '60s up to '68 – '70s including '69 – '68 and '69 – NIH was in difficult times. Is this a fair assessment in terms of political troubles and more so economic troubles.

JS: I think that's probably true. I think there weren't as many grants made as we and the community had hoped. But there wasn't a great clamor and also you had to remember at that time the ratio of money available for grants to potential recipients wasn't as great as it is now, because now I don't think there's any question about good research out there that isn't being supported than there was at that time, but even so it was, I think, a mark of a strong, involved, scientifically wise administrator.

BSP: Right after you left NIH in 1974 your view of NIH from outside was a bit different, or is it all the same.

JS: Yeah, sure.

BSP: And, although you are dealing with outside extramural funds, now you are in a position begging for more money and influencing the extramural policies from different perspective. So is it any, I would say it's a change of roles, you know, but could you comment on that change?

JS: Well, in the first place I regretted the circumstance that prompted me to leave.

BSP: Right.

JS: And they were prompted by the circumstances there. I think I was self-sufficiently dedicated to the biomedical research cause, and the NIH was the leading figure in the whole proposition that I went at getting more money for them with the same degree of vigor if I had been out there. One thing that did occur was the fact that this was at the time when the Bureau of the Budget was trying to cut off the training authority for NIH. They felt with a fellowship program that was enough, and that violated Shannon's infrastructure building concept.

BSP: Right.

JS: And, and then there were the most trying days that I ever had as a witness. Tom Kennedy and I were testifying before Paul Rogers, and we had our minor along with us, in the department. And Paul says, "What is it about all these training grants, aren't they a pretty good thing?" But we were supposed to say, "No,

they're a terrible thing, cut 'em out." Of course that was the administration line and we were supposed to support the administration. Well we hemmed and hawed in some fashion and that darn Rogers, who's a good personal friend, he sat out there and you know he dangled us like puppets on a string. I don't know how we got out of it, but the committee acted against the administration's proposal, and not long after that Rogers had Weinberger for a hearing on something. I guess it was a general state of things in HEW, and he turned to Weinberger, who's on the witness chair at the time, and he said, "By the way, why is it that fellows like Sherman are leaving NIH?" And that damn Weinberger looked up just as quick and he said, "Well, because this allows them to increase their pension." Well Paul was furious about that and he sent me that night a copy of the transcript, of course which hadn't been finalized, and he said, "John, if want to refute those words in any way, please be my guest." I thought about it for a day or so, which is all the time I had, and I finally decided on the adage of a wonderful little public health service administrative officer: you can't win a pissing contest with a skunk. So I didn't say anything, but there was that period that when they were not only – they weren't, as I recall, cutting programs per se, they were cutting the money for the programs. And it was a real cut; it wasn't a cut in an increase, which so often is cited as a cut.

BSP: Did you come to NIH alumni reunion in 1975?

JS: Yeah, yeah. I just, as a matter of fact, I just sent off some photographs that I found recently of Ron Lamont-Havers, who's my successor as deputy director because he acted as Director of NIH at that affair. That was a surprisingly good program; they haven't been able to replicate it since.

BSP: Before we closing our conversation, I would like to touch on some general subjects. We have been talking about the various specific events throughout the time, but talk about some general things such as the relationship between intramural and extramural programs, and why do we have to have intramural program, how to define NIH's role in the whole biomedical enterprise, and some general issues. Let's first talk about relationship between intramural and extramural programs. I raised this issue because from time to time the Commissioned Corps said that, "Well, why," but generally in the 1950's and '60s and even '70s, the outside reports recommended slow growth for the NIH intramural program.

JS: Mm-hmm.

BSP: NIH intramural program doesn't have to expand too much, and that happened, actually, you know, in the beginning intramural program was the half of the size of the budget.

JS: Mm-hmm.

BSP: But now it, by 1960 became less than 15% and it stayed that way. So unlike other people who tend to think that NIH is big because it has very big laboratories. It's big because it's spreading out a lot of money, so intramural program was relatively, not very big.

JS: Mm-hmm.

BSP: And in the 1970's when Hans Stetten became Deputy Director for Science, he had a hard time justifying the existence of the intramural program. Historically when NIH is studied as an intramural program, and it is shown as very good, so it's absurd to destroy what is going well.

JS: Right.

BSP: So that is an argument. And in the 1980's, especially in 1988, I remember Institute of Medicine reports recommended – well there was suggestions to privatize the intramural programs in the 1988, IOM report justified that the intramural program merits existence and development. So there were some discussions of a time about the idea of having extramural program within the federal government, and do you have any thoughts on this issue?

JS: Well I always thought that if you were to split off the intramural program from the extramural program or vice versa, and first place the administrative costs of running that would be enormously high compared to the way it is now. Yes it is, you've got everything pretty well centralized for both intramural and the extramural program, even though essentially for all practical purposes they are separate. And so I think it would be foolish or unwise to split off that intramural component into the private sector. I think the same thing has happened with respect to – I don't know as it's so true now, you know, I'm far enough away that I can't make some comments with a great deal of confidence. But there can be a degree of interchange between the intramural and extramural program on a personal basis, an unofficial basis, that's bound to be of value to the extramural program, and therefore I think that has an impact on high quality of the extramural program.

BSP: Actually there are, still there are lots of extramural officers coming out of intramural program.

JS: Right, right. It's an excellent training program.

BSP: And for background science.

JS: I don't know where they would find a cadre, even, of personnel for the extramural program on the outside. I think one of the most serious consequences would be to have such a hodgepodge of administrative rules and operations. Either you'd have to have the money split up state by state and administered state by state, which would be a disaster, or you'd have to find some way to centralize that operation apart from the intramural program and away from it. That raises all these other questions about efficiency and quality of operations. I had never thought that the intramural program and the extramural program were competitive. The intramural program, you know, is run somewhere between 10 and 20% as you mentioned, way back into the early days when the extramural budget was pretty small, so that the growth of the intramural program has not been at the expense of the extramural program or the other way around.

BSP: Right.

JS: And for that reason, it would be a make-work political solution to a non-problem.

BSP: Is it true that extramural program officers have – could get some advice from intramural friends or some inputs, not necessarily in getting grants, but getting a sense of what's the hot field and what's the direction to go?

JS: Oh, I'm sure they could. I think that would facilitate the development of the whole field so long as they kept their information generally and without attribution to any.

BSP: Right.

JS: I think there would have to be very strict guards about that, but it used to be, you know when the extramural program was first set up, the review panels were composed mostly of intramural scientists, and it was only when that first workload was too great because they were taking time away from their intramural job. Secondly, there was that danger of conflict of interest.

BSP: So there are a lot of intramural scientists working at the study sections.

JS: Now, in certain instances, and I'm not acquainted with many of them, but I know in the Cancer Institute it sometimes is pretty hard to find a reasonable definition of a decision – of a division between intramural and extramural, but that I think is, at least in the past, has been pretty well monitored and controlled.

BSP: In terms of gray areas between intramural and extramural program was the contract system and so called collaborative programs. It's because intramural scientists sort of hiring extramural researchers to do a certain kind of research, and in the name of collaboration, but it was managed by the intramural scientists. It can be seen as an extension of intramural program or it's an extramural program coming into the intramural program, and so it's kind of a gray area. It didn't happen to every institute, it happened to big ones.

JS: Right, right. Yeah, and again I think in proper circumstances and a transparency to the process, that probably could be condoned, but it has to be watched very, very carefully and there has to be ground rules laid down to be certain that there aren't abuses.

BSP: So NCI, because of its great budget power, tended to go along, tended to be setting its own rules?

JS: To a degree, to a degree. But they still participate, is my understanding in the extramural chiefs' monthly meeting.

BSP: Right.

JS: So there is putting it table talking out what could be potential problems or actually developed into problems, or where it's a possible use not to be considered.

BSP: The last topic is NIH's place in biomedical enterprise in the United States and the whole world, and let us talk about the whole world aspects. I found that in the 1970's, increasing number of postdoctoral fellows came from foreign countries – first European countries and Japan and I don't know whether you're involved in these arrangements, but I don't know.

JS: I was in some of the Fogarty Center general activities, and I was in, when these programs were first becoming prominent, I always thought it was an excellent idea unless one condition developed, and that is that the foreign fellows ended up working as laboratory technicians. I think that's always a danger that there are another pair of hands to work rather than there for learning.

BSP: Mm-hmm.

JS: But beyond that, the NIH was the only place for a young budding scientist in Korea or China or Italy or Germany or South America to come. Now it's a little different, another instance of the NIH's own success, but I'm for a strong period however it's maintained and however it's operated exchanges at the scientific level and in particularly the training level. I remember one – twice while I was in the front office I tried to find a way that I could get the State Department to put aside – as I recall I wasn't asking for more than a million dollars a year – to set up fellowships for medical students in South American countries and perhaps elsewhere in the world to come here and study instead of going to Russia, and the darn State Department wouldn't hear anything of it and you know the cost was miniscule. And in many of those instances at that time those fellows, and sometimes women, went back to their country as leaders, not just in medicine but politically as well.

BSP: So it's a tremendous investment.

JS: For a relatively little amount of money and potentially it would seem the rewards are enormous.

BSP: It's sort of the general concept of creating an infrastructure by training more researchers as James Shannon advocated in the early days. I think for a third world country to have the budding biomedical research programs they have to have some researchers and leaders. I guess NIH provides some great help to countries in the biomedical research field. Like Italy I was told that in the clinical center one floor was full of Italians. And there are some areas where there are Chinese, there are Koreans and Japanese, and so when they returned home they learned something about the federal government's role in biomedical research. Now, looking at the United States' part the relationship between government, especially federal government and higher education, medical education, the relationship evolved over time. Before World War II it was very separated; there was a careful observation to avoid a mix up, but after World War II it was a whole different story because federal grants money and fellowship money and construction money pouring into the medical schools and the universities has changed a lot for better or for worse. And so the role of the federal government wasn't like it used to be 50 years ago. So could give your thoughts on the place of NIH or the place of the federal government in the ways in which the federal government is changing the medical education and research since you're also involved with the AAMC?

JS: Well, again I've been away from it too long now, but it used to be that the AAMC was headquartered at the World Health Organization and medical school organization. I think it sort of died out now unfortunately; but at the time they used to hold annual meetings and they would invite good and diverse attendants to attend from other countries. Again that was done with relatively little amount of money and yet with seemingly first rate results as long as the system was set up to carefully apprise the potential student, and I suppose there is an etiological and maybe political uncertainty now as to whether that should be managed by not CDC but one of the other elements of the Public Health Service. Working with, I suppose, WHO, but I when I last left NIH or AAMC there was still interest in the world medical schools, or whatever they call the outfit at WHO.

End of transcript