

**NATIONAL INSTITUTES OF HEALTH
INTERVIEWS WITH PAST AND PRESENT MEMBERS
OF THE CLINICAL CENTER NUCLEAR MEDICINE DEPARTMENT**

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DR. MANDEL: This is an interview of Dr. Jack Davidson, Dr. Giovanni DiChiro, Dr. Gerald Johnston, and Dr. John Harbert, all formerly of the Department of Nuclear Medicine in the Clinical Center about the history of that department. Also Dr. Jack Robbins of NIDDK, who was a thyroid cancer specialist. Interviewing these gentlemen will be Dr. Richard Mandel, who is currently on contract with the Clinical Center Office of communications developing a history of the Clinical Center, Dennis Rodrigues from the NIH Historical Office and Ronald Newman from the Department of Nuclear Medicine, who is currently the Chief of that section.

DR. MANDEL: What I would like to do this afternoon is start as close to the beginning as we can in terms of Nuclear Medicine at the Clinical Center and then we'll come through the 1960s and years subsequent to that. I understand that the expertise that you have is not centered on the 1950s, but I still need to start there for purposes of continuity here. Nuclear Medicine or the department that would become Nuclear Medicine was given an independent status by Jack Masur, in the original leadership of the Clinical Center. And this always was a point of contention between the scientific side of the hospital and the clinical side of the hospital. What I need to know is to what degree was Nuclear Medicine independent from the categorical institutes here in those years and to what degree did they feel sort of beholden to the other institutes and sort of obligated, particularly to the Cancer Institute, to do their work?

DR. DAVIDSON: I'm Dr. Jack Davidson. I arrived at NIH in 1957, and I had had a vast history of training in handling of radioactivity at Oak Ridge in 1950 and I had some 7 years on the Columbia University Radiation Committee for the entire university, not just the Medical Center. That history turned up at NIH and I was promptly made a Co-Chairman with George Williams on the Radiation Committee which controlled radioactivity and administered our various licenses with the then Atomic Energy Commission for all of the institutes and all users and operated the— supervised--the Radiation Safety Laboratory, or Group. That kind of operation continued. The various institutes did different operations clinically with radioisotopes apart from the laboratory uses. The Neurological Disease Institute developed and operated one of the earliest brain scanners which had eventually two crystal detectors and it was a rectilinear scanning device and radioiodine was the isotope that was used. Subsequently two more heads were added to that and that became the fabled "Tetrascanner" which could do the job in half

the time of a 4-view scan with a two head. Dr. Robbins was treating thyroids with radioiodine *and* running that aspect of things, so if somebody in the Cancer Institute had a patient with a thyroid problem they had to get ahold of Dr. Robbins to do something about it. If they had a patient where there was a question of a cerebral metastasis, or a tumor, they had to get ahold of Dr. Shy in the Neurological Disease Institute. There was no common facility. The Radiology Department got an early rectilinear scanner which I believe was used for lung and other body scans and my first real contact with that was Dr. William Ashburn, who is now the Chief of Nuclear Medicine at La Jolla, and Bill was, I guess, maybe a second or third year resident in the Radiology Department at that time. But he got the job of operating that scanner. And somewhere along about 1965, from my vantage point--I had survived as the sole Chairman of the Radiation Committee when George Williams got off--and I felt that this subject of nuclear medicine as an entity was coming into being. We used to just be the "Hot Lab," or the "Radioactivity Lab" in different hospitals. And I made a formal proposal to the management in Building 1 that they ought to consider instituting a Department of Nuclear Medicine in the Clinical Center to centralize these facilities and to be available then to all patients in any institute. A committee was appointed, as always, and this committee consisted of Dr. Masur, Dr. Berlin, and Dr. Joseph Rall, and they interviewed candidates to conduct such an operation for much of a year. The thrust was, I think, misguided. They were interviewing physicists. They had no cyclotron. They had no nuclear reactors. They had nothing to intrigue a physicist. And they got turned down, and turned down, and several people then came and looked over the premises and talked to people but they uniformly said no. So, somewhere toward the end they came back to me, Jack Davidson, and said, "You were the fellow who suggested this. Why don't you do it?" And I was in the Cancer Institute at that time finishing up some-- I'd been, oh, about years there-- years--in the Cancer Institute not having a career of nearly years in anticancer drug development but using radioisotopes in the laboratory, and I had had clinical experience using radioisotopes at Columbia University and I did a lot of therapy there. So, I was kind of interested, and I bargained with them to send me around the country and look at what existed. And I went to different institutions that had Nuclear Medicine Departments, talked to people, saw what they were doing, how it was done, came home and submitted a "White Paper" to the management in February, , and I'm reading a record written at the time, or thereabouts, that I was offered the proposal of the job by Dr. Earle Simard and in May of 1966 I submitted this written proposal for what I would envision setting up and what I--the sort of thing--that I would undertake properly. In July of 1966 it was approved and we were assigned space in Room 1849-A in the basement in the B-Wing of the Clinical Center. By July of 1966, we had managed to, I think we evicted the nursing staff from the rest of the space down there and we got rooms 47 to 55. And October 16, 1966, the Chief of the Management Policy Branch of the CAM--I've forgotten what that was--created a Department of Nuclear Medicine

and abolished the old Radiation Safety Division which had been under Plant Safety and handed that to us. So we had the fox watching the chicken coop. We thought it would probably be workable and, I think, indeed it was at that time because I had had a longtime association with Jerry Manson Brown who was the Radiation Safety Officer. We got along. We had no problems with one another. So, I acquired that division with something like employees, 2 trucks and, at that time, there was virtually nothing done on contract; we did everything from scratch. We ran our own films. They had the Monitoring Service develop them, read them, and we did all our own safety surveys of the labs, we calibrated all radio activities coming in. We were the central receiving point for the radioisotopes for the entire NIH.

DR. MANDEL: Was there still a separate Isotope Laboratory at that time?

DR. DAVIDSON: There was a separate facility. Yes.

DR. MANDEL: And was it in a temporary building just outside the Clinical Center?

DR. DAVIDSON: They were in what I know as Building 21. Whether it's still Building 21.

DR. MANDEL: Yes, there is.

DR. DAVIDSON: There is? It existed in about one-third the size that I think it is now. But operations just went up logarithmically and our problem always was space, money and bodies. And this created a great problem here. And on October 20, 1966, I sent Dr. Masur a memo confirming that indeed a Department of Nuclear Medicine did exist. On December 31, 1966 Jack D. Davidson got orders from the United States Public Health Service assigning him to that duty and taking him out of the Cancer Institute and putting him into the Clinical Center entity. January 20th, we sent a memo to personnel to pick up a secretary and Dr. John Harbert, also. People, a Mrs. Davis, a Mr. Murphy who was an instrument man, and Mr. Meiers—I've forgotten who he was—he was transferred to Nuclear Medicine. And all Radiation Safety personnel were transferred in January of 1967 to Nuclear Medicine. And in October of '66 actually—I've got it out of order—a memo from the Director of the NIH to the Director of the Clinical Center established that there would be a Department of Nuclear Medicine with three sections: Radiation Safety, Diagnostics Division, and the Whole Body Counter Division. And in March we undertook some reconstruction and physical modification of space. In February of '67 the first full y-counter was set up and in operation. This was a y—camera. In May of '68 we got a Protec-X ray film processor. We no longer had to take our films to the 6th Floor to develop them. Well, we painted the place in March. And a big event was June of '68 when the contract was let to build an addition on Building 21 for a Radiation Safety Lab, and this was my cross to bear for many, many months.

Somewhere, I think, maybe, they have pictures that I took of the evolving construction from a hole in the ground.

DR. MANDEL: Building 21?

DR. DAVIDSON: Yes. And that incorporated additional office space, and the biggest thing was a big teaching lecture room. But we were giving courses up to, I guess, 3 or 4 times a year, a two week course, for NIH personnel, about 50 people to a class and participation in that course qualified one with the NIH Committee to use and possess radioisotopes. And that was either laboratory or clinical.

DR. MANDEL: Now, when the building opened in 1954 there was a Radiation Wing, and I'm not sure what happened in the intervening years.

DR. DAVIDSON: When you say "Radiation Wing," which do you mean? We had a wing with the Radiotherapy.

DR. MANDEL: Well, I mean, this is the way it was designed. There were many changes and all kinds of adaptations. What seems obvious to me in the broad sense is that large plans were laid for Radiology in 1949 and 1950 that were never developed and we don't get a full development of Nuclear Medicine at least until the 1960s.

DR. DAVIDSON: You see, Nuclear Medicine really had little to do with Radiology. Nuclear Medicine, in the early years I'm talking about--the 50's--- clinical nuclear medicine was practiced by essentially three categories of people: Pathologists. A lot of the pathologists were doing some *in vitro* laboratory tests. They had licenses. They progressed in that they, in many institutions, did the thyroid work and other clinical procedures. Another group was diagnostic radiologists. Quite a few of them had developed the capability because, you see, they had the physics background already very much. And then the other group was internists, internal medicine people. A number of them, particularly hematologists, were in it. And when the American Board of Nuclear Medicine was constituted--that started in formation about 1970 and those three groups, plus a fourth group, which were people who were long-time members with the Society of Nuclear Medicine for years. But it was informal. No qualifications were required. There were a lot of practitioners in it. Some of them were surgeons. It was a group, a number of whom didn't really adapt by themselves as pathologists or internists or radiologists. So, when the Board was set up the founding fathers *ad hoc* group decided the initial Board would consist of 12 members, 3 from each of the 4 groups: pathologists, radiologists, internists, and the everything else group who were members of the society and they were proposed by the Society Board. That group of 12 then developed the first examination and set up the criteria for certification in the realm of nuclear medicine and the first examination was given in 1972. And many of us--I think the biggest bumper crop---took that, with fear and trembling, because there was no past history

and nobody knew-- They didn't even know how to grade it and they didn't know what kind of results they were going to get. They threw a bunch of questions at us. We all took it here in Washington and came away in fear and trembling. But I think something like 70 percent of us passed it.

DR. MANDEL: This was in 1970?

DR. DAVIDSON: 1972.

DR. MANDEL: 1972.

SPEAKER: 1974. Isn't it '74?

DR. DAVIDSON: I've got my certificate. I've brought it along.

DR. MANDEL: The contrast with the 1950s is, I think, striking. When they designed the hospital they did have a Radiation Wing.

DR. DAVIDSON: I can add a little more to that, because Howard Andrews and, oh, Jay Robert Andrews, no relation, Jay Robert was the radiotherapist and he had his machinery. They had a neutron generator.

SPEAKER: The Vandergraff?

DR. DAVIDSON: This Vandergraff monster. Million volts or some damned thing. And they required a cobalt source, a cobalt chemotherapy source. All of this stuff was housed down on Bi and B. And that was known to us as the Radiation Wing and it was built as such. It had mammoth thick concrete walls. Okay. Now they were not in the isotope business except for the cobalt.

DR. MANDEL: Right. But in the political process that Masur had to deal with, getting appropriations through Congress, this is how they thought of Radiation Medicine. In Congress they believed that somehow you could apply atomic power, radioactivity, to medicine and they were completely ignorant of the medical implications of this and they were always eager to support in appropriations these equipment investments for things like a Vandergraff generator. Now, what is the relationship, exactly, between that kind of establishment here and the Nuclear Medicine that evolves?

DR. DAVIDSON: Virtually none

DR. MANDEL: Virtually none. Okay.

DR. DAVIDSON: You see, nuclear medicine, the use of radioisotopes clinically, dated well back. They had it in World War II. And I happened to be at Columbia and we had a team there of Viella and Quimby, Edith Quimby, and a lady clinician Virginia Nielen France, and she treated

thyroids and she got her radioiodine from Columbia's cyclotron. And this was before they'd invented reactors and exploded A-bombs or anything else. And the success of that in delivering a radiation dose selectively to tissue just had everybody excited. What we've got to do is make more isotopes. And in 1946 the reactors were unveiled and the first shipments of isotopes came out of Oak Ridge. And I went there in 1950, nominally, as a junior man on the totem pole of the research group, and somebody had to go and learn how to use carbon because we wanted to do it. I also learned about a lot of other things. We had clinical instruction. And so I came back and opened what we called a "Hot Lab" at Goldwater Memorial Hospital first in the middle of the East River, now Roosevelt Island, and a posh residential area. But Columbia University Med School had a division there.

DR. NEUMANN: Where was that, Jack?

DR. DAVIDSON: Goldwater Memorial Hospital was under the middle of the 59th Street Bridge, a 2000 bed chronic disease hospital. And six months later I got moved out because Columbia opened a new cancer hospital, Francis Delafield Hospital, at 167th Street, in the heart of the current drug district, if you read *The Wall Street Journal*. And there things got perking much more and we did a lot of isotope therapy, and that was where I got involved with the Columbia Committee. I was on the committee with these early people, physicists Quinby and Viella, who were, I guess, the top dogs in radiation dosimetry.

DR. ROBBINS: Was Marinelli there also?

DR. DAVIDSON: I don't recall that he was. He invented the Marinelli beaker, right? Remember that?

DR. ROBBINS: Yes, the one with the hole in the mouth?

DR. DAVIDSON: Yes. That's the only thing I know about Marinelli. So, anyway, that's how things got going here, and we were fully constituted in operation by--oh, tapering on very much--in '68 and '58, and we had equipment then. In July of '68, the air conditioner compressor blew up.

DR. MANDEL: What would you say is the most significant technology the NIH department had at its inception?

DR. DAVIDSON: Well, the biggest technology event was the advent of the y-camera. We had one of the early ones. This permitted static imaging, as opposed to rectilinear scanning. It was alive. And we got a publication by Bill Ashburn and the NIH television group-- Bill Whitehorse?

DR. MANDEL: Whitehead?

DR. DAVIDSON: Whitehead, Whitehorse?

DR. MANDEL: Whitehouse?

DR. ROBBINS: Whitehouse.

DR. DAVIDSON: Oh, Bill, I saw him just a couple of years ago.

DR. DiCHIRO: He's in Arizona. He's someplace near Phoenix?

DR. DAVIDSON: Oh, he moved?

DR. DiCHIRO: Yes.

DR. DAVIDSON: He used to live right near me, about three blocks away. Well, finally, he--NIH--had a central television set-up and he brought his big camera—you know, you used to see this in television and he had all these great big things on a tripod and it looked at our little 5-inch cathode ray tube and we did blood flow studies, the study of blood going through the heart. And these were among the earliest. This was '67, '68, you published a paper. I've got a copy of it here. And that work continued. And that early camera was also equipped for positrons, which are a big deal now, PET scanning. The only hooker was that the positron detector head was smaller than the y-head and that head, the electronic circuitry, just couldn't handle all the garbage and we couldn't give enough of a positron to get an image in any kind of a reasonable time. And we had to make our positrons over at the AFRI. We tried fluorine 18. It was a dead duck right from the beginning. We just found out we couldn't really do anything with it. But it was the beginning. It was NIH's first foray into PET scanning. They hadn't invented the word then.

DR. MANDEL: And what would be the predecessors to the tetrascanner?

DR. DAVIDSON: The predecessor, the first rectilinear scanning whatsoever, was with a single head with a crystal detector that was mounted to do a grid pattern, and the first such scanning was probably of the thyroid, Jack, wasn't it? We had one up at Delafield-17.

DR. DiCHIRO: It was Bellamy Carson who invented the first.

DR. DAVIDSON: Yes, Giovanni, you know all this. You can pitch in here.

DR. DiCHIRO: I gave the Carson Lecture. That's the reason. I gave one this Carson Lecture in Los Angeles.

DR. MANDEL: What?

DR. DiCHIRO: Well, regarding the tetrascanner, when I arrived here in January of '58, Dr. G. Milton Shy was the Chief, the Scientific Director of the institute with which I was, and am still, connected. At that time the institute was called the NINDB, the National Institute of Neurological Disease and

Blindness, because they had not spawned yet the National Eye Institute and the National Institute of Communicative Disease, of hearing, and it's a new institute today. And here developed this dual head scanner with two groups, which had been built by two physicists connected with Oak Ridge. One was Craig Harris and the other one was Francis. I don't remember the first name.

DR. DAVIDSON: Okay. I worked with Craig Harris at Duke later on and more years with Craig as my physicist when I was down there after NIH. But this was the era of the famous "gold collimators."

DR. DiCHIRO: I mean, Craig Harris became later President of the Society of Nuclear Medicine, and so the most striking features of this device were these two huge, like two huge watermelons, literally like a watermelon, this size, tungsten outer component with inside a core, and in that a core of gold. So they were tungsten-gold collimators. And so they obtained these scans. And I came here just to work with Shy in this specific area. He wanted to have a neuroradiologist to carry out the cerebral arteriography that I was carrying out in Sweden and so on. And so I came. My main scientific interest wasn't in the way of isotopes because I didn't know anything about. I had been trained in Sweden as a neuroradiologist, so essentially as a pneumoencephalographer and then brain angiography, those would be my capabilities. But I didn't know anything about isotopes. And I wanted to learn. And I learned, helped by Dr. Shy, who was giving lectures all over the place. He was a very fascinating speaker, by the way. So, I arrived here and we were working and I started work and we had a large population of patients from the entire Washington area where we used radioiodinated serum albumin, what was called RISA at that time. After two years, however, we also introduced new radiopharmaceutical, radioiodinated anti-fibrinogen, and we used that--with the hope to study cerebral infarctions and so on. And so, Dr. Shy-- Incidentally, the key man in operating this device was a very bright young man, William Matthews, who had no formal education. He was just a bright young man. And he was operating also the thyroid-

DR. MANDEL: Name?

DR. DiCHIRO: William Matthews. And he used to read the scan. The first nucleus of what in the future became in '66 the Department of Nuclear Medicine was, from the clinical and scanning point of view was this Mr. Matthews under the direction of Shy. And I joined the group and the thyroid studies were submitted to Jack [Robbins] for interpretation. They arrived to Bill Matthews. Bill Matthews used them. Bill Matthews, by the way, has been the Budget Officer for many years. He just retired a couple of years ago.

DR. DAVIDSON: One important point here, a lot of this technical problem, the massive collimators and tungsten shield and everything, was because the isotope available was I^{131} , a hell of a high-energy γ , and that

necessitated this heavy shielding. Now, when technetium came along it just revolutionized the business.

DR. DiCHIRO: Yes. That's a very good point.

DR. DAVIDSON: And we--our department--was in on the ground floor with that because we got this chemical technetium from Brookhaven Lab, molybdenum⁹⁹, and we'd get that as a molybdenum generator, unsterile, uncalibrated, everything, just raw and our "Hot Lab" set it up and we'd milk it. You see, we considered it self-sterilizing once you got the outside gook off because the radiation field would kill any self-respecting bug. And we milked that daily to get our technetium for the day and calibrated it and made various preparations out of it, and that just revolutionized scanning all kinds of things. It's only a question of your ingenuity in devising chemical compounds that would take the technetium where you wanted it to go.

DR. MANDEL: So that after that point there was less, much less, need for shielding?

DR. DAVIDSON: Much less mass, so the crystals could be much thinner and still be highly efficient. Everything was much lighter weight. Shielding was greatly reduced.

DR. MANDEL: And there were also many more applications for scanning?

DR. DAVIDSON: Yes. You could give a much bigger dose. There was no β emission and the half-life wore itself out quick, got rid of itself, and so it was a panacea, and it's just been amazing that now Henry Wagner, who used to be good for a new isotope every year, he's kind of run out of isotopes now. I haven't heard of a new one in a long time. But the other concept you touched on, about going to Congress. That a magic bullet for every disease, you know, out of a different category of isotopes, just really rapidly turned out to be not so. Now there's a renaissance, now that we can maybe tag it onto a protein which will, by virtue of the protein's affinity for your target, will drag the radioisotope there and blast it, and that's now being done. But, it's not a function of the radionuclide; it's a function of the protein and the specificity and antibodies.

DR. ROBBINS: I can make a few comments about what happened before the scanners. I'm Jack Robbins. And I started out on the other side, across town, from Jack Davidson, at Memorial Hospital in New York.

DR. DAVIDSON: Our friendly rivals on the East Side of Manhattan.

DR. ROBBINS: Which was part of Cornell and I was working with Rulon Rossin and Ed Rall in the Department of Endocrinology with a main interest in the thyroid. And this was, as Jack said, before the field of nuclear medicine, as such, really developed. And thyroid was one of the key areas in which isotopes were used from the beginning because the radioactive iodine could be used. And up until the time of the scanning

everything was done by hand, so that we had a simple hand detector with a certain amount of shielding and we would do full-body scans and thyroid scans just by hand. And that's the way they were first done.

DR. NEUMANN: When did crystals first come in?

DR. DAVIDSON: The technology of the sodium iodide detector, that was highly dependent on the development of the photomultiplier tube detector.

DR. ROBBINS: That must have been in the early '50s.

DR. DAVIDSON: I think so. You were using Geiger technology.

DR. ROBBINS: When I came here in 1954, we were using a Geiger counter; Geiger tubes, and think the scintillation crystals were available about that time.

DR. DAVIDSON: Because that was another aspect of the whole body counter room and the crystals for the tetrascanner. But crystals were monumental. They were big, long things because they had to stop this y. And when you got to technetium you could get away with a half an inch.

DR. MANDEL: And what was the innovation that was represented by the tetrascanner in the context of these early scans?

DR. DiCHIRO: The tetrascanner, it's really not such a big advance. I mean it was a very practical advance. Shy left in, I'm not sure the date, it could be '62 or '63, to become Chairman of the Department of Neurology at the University of Pennsylvania, and so I became deeply involved with radioisotope studies. Indeed, in 1962, we carried out the first radionuclide cisternography, which was introduced here, the radionuclide cisternography, radionuclide ventricular, and again, using radioiodinated serum albumin. We demonstrated in 1962, for the first time, the cerebrospinal fluid rhinorrhea using cisternography in 1962. And, well, because of these studies we wanted to work fast and detect activity simultaneously in both planes, so then we decide to add the--I decided to add--additional, to expand the dual scan, and I baptized the tetrascanner. But it was no theoretical advance or anything. It just was transformed from a dual scanner to a tetrascanner, and it was extremely practical. It increased the speed of examination and improved significantly our diagnostic capability, particularly in cisternography, because, you see, the radionuclide moves. To detect, for instance, a cerebrospinal fluid rhinorrhea you have to catch the isotope while it flows along with the moving and leaking CSF. You have to detect that. So, if you wait to do it another view to confirm that, you do not detect any longer the same stage of movement of the CSF target with the isotope. So, on two heads again, we asked Craig Harris to add exactly the two additional collimators, the same watermelons, and that was it.

- DR. MANDEL: How long did the isotope last, or how long could it be traced, with this equipment? What was the effective half-life?
- DR. DiCHIRO: The effective half-life was the same of the iodine¹³¹. The isotope was...we were really still stuck with iodine¹³¹.
- DR. NEUMANN: It has an 8-day physical half-life.
- DR. MANDEL: I see.
- DR. HARBERT: In retrospect, some of the most phenomenal scans ever recorded are those early scans of CSF rhinorrhea. The leak occurs between the fluid-filled spaces in the brain with the nose and the object that Dr. DiChiro developed was to put the isotope into the spine and it would travel up into the head, and when it finally flowed out into the nose he anticipated seeing, in fact he actually did, in later studies.
- DR. DiCHIRO: In fact, in the very first case studied.
- DR. HARBERT: Yes. And in later cases we would scan practically for hours and hours and maybe just see the transient event of the isotope leaking out for, you know, a few minutes. So, it was just extraordinary that he was able to capture these events with, you know, such primitive equipment.
- DR. MANDEL: Now, when we reviewed this the last time, you mentioned that subsequent work that you did immediately after this wasn't particularly productive. Is it the case that this sort of culminates a whole phase of development, the tetrascanner, and that right after that there's an innovation of a new kind of technology that you then used?
- DR. DAVIDSON: The y-camera.
- DR. DiCHIRO: When the y-camera came, that was the end, and actually the tetrascanner, Jerry ordered to dispose of it.
- DR. JOHNSTON: Yes. We made some pictures with the tetrascanner in the '70s.
- DR. NEUMANN: Have you got it in your garage?
- DR. JOHNSTON: No, no. The machine shop took it back, took it apart. They put the gold collimators in the safe down in the front office and the just as valuable tungsten collimators were in Ray Murphy's storage closet one floor below for the longest time. I don't know what ever became of those.
- DR. DiCHIRO: By the way, Jerry, I was standing around that the body of the tetrascanner was stored in a furniture store called "Neil," N—E—I—L, the furniture store, in Rockville. Do you remember this building?
- DR. JOHNSTON: I know there was a storage place.

DR. DiCHIRO: There was a story that NIH used the basement of this shop, this market, a huge area, like a supermarket, but it was furniture called "Neil," the basement was for storage for the NIH. And there it ended up. But the gold was rescued.

DR. JOHNSTON: The value of gold went up and they got very nervous in the front office and called people back from Oak Ridge and they came up then with guns and everything and claimed it and took it away. But before that, you know, I carried collimators around. I took them home for a while, you know, and brought them back. They weighed about 20 pounds apiece.

DR. DiCHIRO: I know they cost \$3,000. At that time gold was \$34 dollars per ounce and it cost \$3800, dollars each. But the tungsten was more valuable, I mean specifically and I don't know what happened to the tungsten.

DR. JOHNSTON: Ray Murphy would know what happened to that tungsten collimator.

DR. MANDEL: Was there anything that was involved in the tetrascanner that was useful in later development of the technology?

DR. DiCHIRO: No. It did not have really repercussion outside the NIH. John Harbert, we worked together. It speeded our work, particularly in the cisternography. It helped us tremendously. But it, as a machine, did not spawn any advances. It remained a totally unique type of machine.

DR. JOHNSTON: It was impressive to scanners in the way that the Super Constellation is impressive among airplanes.

DR. DiCHIRO: That's a very, very good analogy.

DR. MANDEL: Would that have been the first multi-headed instrument, or were there other multiple head instruments?

DR. JOHNSTON: Well, the dual scanner probably was probably the first multi-headed scanner. Uh-huh.

DR. MANDEL: When you said it was unique, was it a unique instrument or did other institutions around the country have similar types?

DR. DiCHIRO: No, it was not copied. But, you see, we used it. It served a means to an end, to a different end. We used it as a research tool. It did not represent a technical advance of any sort. You know, it was just a really practical tool to speed up, improve, the accuracy of our studies and so on. But it was not a technical advance like the well counter. I mean nothing comparable to that.

DR. MANDEL: In the 1970s what would you say the basic technology of the department was? What would be the equipment you were using after this tetrascanner?

DR. JOHNSTON: I'm Gerald Johnston. I came here in 1971. And just before I came, apparently Jack had left and a committee was gotten together that recommended enlarging Nuclear Medicine along the lines that Jack had been asking for. It should occupy more space, have more technicians, have a physicist assigned, and take on the many demands that were developing from the different institutes. And by the time that I arrived, with a fairly large group by comparison. What did you have? Four people, two techs and you and Jack. We had about 8. We got additional equipment. We got whole body scanners for doing bone scans. Gallium had come along then and we were instrumental in helping show the utility of gallium, particularly in cancer and infectious diseases here. We got many more y-cameras and a couple of generations of y-camera had gone by, by then, so that the resolution had improved remarkably and the technology of developing crystals had improved so that the thickness of the crystal was much thinner. We also had finer collimators so that our pictures got better and better as time went along. There was a lot of work going on developing different isotopes and different approaches to isotopes. And hand in hand with this was the technology of treating heart disease. As Jack pointed out, he and Bill Ashburn were making pictures of blood flowing through the heart and so on, and John Harbert, but people said, "Isn't that nice. There's not a whole lot we can do about it." But when we started replacing heart valves, doing cardiac bypass operations to bypass the occlusions in the coronary arteries and then came along with balloon angioplasty there was a great deal of interest by the Heart Institute to come up with noninvasive ways of diagnosing heart disease more minutely. So, under that pressure, some of the early approaches were put together here at NIH and multi-gated imaging of the heart, looking at the heart as a cine display came to pass, and that was a great advance. We didn't, at that time, do much work with thallium because thallium was still pretty difficult to image. But that came along subsequently in your time, I guess, and Steve's.

DR. DAVIDSON: Cardiac output.

DR. JOHNSTON: Yes, cardiac outputs and that were being done earlier. But the pressure to come up with ways of defining which coronary artery might be obstructed. And then, once they did bypass operations here, they were having us do technetium flow studies in an attempt to image the coronary arteries, and I must say I wasn't nearly as impressed with the results as the cardiologists were. They'd come down and say, "I see it. I see it."

SPEAKER: Wishful thinking.

DR. JOHNSTON: "Point to it. You know? I'd like to see it too." They were doing an awful lot of research though at the time with dogs, tying off coronary arteries, bypassing them, and seeing what we could see using our imaging modalities. We even did some gallium studies of myocardial

infarcts. Once the heart was removed you could see the infarcted areas quite readily, but when it was in position in the body, close to the liver, it wasn't a very practical situation. So, we had more equipment, better equipment, and more people. We had computers that had also taken a quantum leap in their capabilities, which was one of the reasons that we could do the MGA studies, the multi-gated heart studies. We had computers that could handle that. And, at the same time, we got a few people like Steve Bacharach who understood them and could use them. Mike Green came along when I came in '1971. He was our physicist. Mike was a very good conceptualizer. He could think up things that ought to be done. And then when we got Steve as well. He could test Mike's ideas in concrete. And between the two of them I think a fair amount of advancement occurred with reference to heart studies.

DR. NEUMANN: Would this have been the first institution then that did a multi-gated study or used the EKG as gating?

DR. JOHNSTON: Yes. The first paper to that effect was in *The New England Journal* as the lead article in April of 1977. That was the very first one.

DR. DiCHIRO: Ejection fraction, that image, was established here.

DR. JOHNSTON: And then, of course, with the Heart Institute's vast number of patients all pigeonholed according to disease and all worked up by other modalities, they just brought them in by the dozens and said, "Today we're going to study people with 50 percent occlusion of one coronary artery," and we'd do those, you know, and then coronary arteries, then valvular disease, aortic insufficiency. We thought that maybe we could come up with the proper response through the MGA on people with aortic regurgitation to know when to replace the valve. You know, it seemed that they'd have a positive response to exercise which would gradually fall down and the simplistic thing would be, well, when it reaches zero, when after exercise your ejection fraction is the same as it is at rest, then that's the time. But it turned out that it wasn't that easy. Some people who had a positive response did poorly when their valves were replaced and some did well, and some, whose ejection fraction fell with exercise, did not do particularly well when the valve was replaced. I don't think they've still worked that out quite. That's done, by guess and by God clinically. A fair amount of activity went on and, of course, because we were able to do a lot more studies following patients with gallium scans--not just the cancer patients but the lung patients they biopsied and bronchoscoped them to follow them along initially, the people with active interstitial pneumonitis and so on--and they found that it was just as valuable to follow them with gallium scans, so then we had very large numbers. So, a vast amount of work was done with heart, with lungs, with cancer, and the bone scans of course were in that study. Cancer got to be quite good with the whole body imagers that were then available with high resolution.

DR. MANDEL: Was that first computerized y-camera given to you because of recruitment or do you recall how that came to be?

DR. JOHNSTON: Yes. Actually a team from DCRT came out to San Francisco. I came here from San Francisco and we met and went down to Hewlett-Packard to explain to them the sort of computer that we'd like to have here. So that was part of the package. And we may still have the chassis here. Actually Steve may still be using that first one.

DR. MANDEL: Was that one of the earliest computerized y-cameras then in the United States also?

DR. JOHNSTON: It was one of the early ones. They had a similar set up, but it was very recent, at the VA Hospital in Palo Alto, and that was handy to the Hewlett-Packard factory. We went down there and looked at their set-up and then went over and talked to the people at the factory and, sure enough, one was delivered here subsequently.

DR. MANDEL: What was the year, about?

DR. JOHNSTON: 1971.

DR. DAVIDSON: John, your split-crystal renography was the first.

DR. HARBERT: Yes. But we just took up the rate meters to strip chart.

DR. DAVIDSON: I know. But I think this publication by you and Bell, from our division, a year after we got the y-camera, reporting the separated, split crystal that was our computer.

DR. MANDEL: That year was what?

DR. DAVIDSON: 1968. It was published in June. And it was where we had a couple of rate meters and we were able to get excretion curves with time for the right kidney and the left kidney, you know, in a crude manner, but it was very useful. And here was a difference in curves with the images to show it. That was pretty computer.

DR. JOHNSTON: Well, the initial y-cameras were lousy. And I was in the Army over at Walter Reed then and they had showed us several of them and I said, well, it isn't going to fly. They came with markers that you put on the earlobes so that you'd know if you were looking at the person head—on or from the side. If you saw two, you knew you were either looking at his face or the back of his head. If you only saw one, then it had to be a lateral. They were that bad. But one of the New England Nuclear salesmen came over and said, "They've just gotten a new camera over at NIH that puts to light all this. I've seen what you fellows can do and it doesn't amount to much." He said, "Come along with me." So they brought me over here and I met Jack and, by golly, I think you had just finished this article you were talking about, and I went right back and

bought one, and that was, I think, the first one that the Army had then. But that, once that y-camera came along, we backed off from other methods of imaging. We do our whole body scanning now with a camera.

DR. NEUMANN: How did you get interested in Nuclear Medicine, Jerry? You were in the military?

DR. JOHNSTON: Yes. I was in the late 50's that I asked about it. And once you go on record with anything in the military that's positive, they tagged me as being a guy that was interested. And I remember the first thyroid scan I saw when I was a medical student, and the notion that an organ could essentially take a picture of itself was just mindboggling. You know? How did they make that picture? And they said, "Well, they gave the guy radioiodine." "Well, did they put the film on his neck, or what happened?" And they said, "No, they have a device that intercedes, that picks up the activity and prints it out." My God, what an intriguing notion, I'm still intrigued by it, but I notice when I tell students about it at the university they take it as, you know, it's accepted now, because it's common. I'm still amazed that we're able to do that, but they're not, you know, because when they were born that was around. I'm amazed at TV and telephones, you know.

DR. MANDEL: Did the military send you to Oak Ridge too? Was that the only site of early training?

DR. JOHNSTON: No. The military sent me to the Navy. And there was two places then, Oak Ridge and the Naval Hospitals, and they sent me over there for months and then I spent a fellowship of sorts at Walter Reed doing OJT, and then they made me head of Walter Reed as soon as I finished that course, which sort of scared the hell out of me.

DR. DAVIDSON: Now, the Oak Ridge Institute of Nuclear Studies was run by Ralph Oberinan, and I was in Class No. 14 in 1950, in March, and my wife has never forgiven me. We'd just had the fourth baby and I took off for a month.

DR. NEUMANN: And Jack and Giovanni, you were trained hands-on, sort of, or did you take these NIH courses?

DR. DiCHIRO: There was a course in Building 21 that we had to take.

DR. DAVIDSON: That was the course that we ran.

DR. DiCHIRO: That was back in '59 or '60, when they gave that course.

DR. HARBERT: That was the beginning of time, and who was going to teach you when the other guy was just discovering it himself? But slowly a body of knowledge built up and fortunately there were those with the foresight to put it together and make formal courses that people could get help.

DR. DAVIDSON: When I first worked with isotopes at Columbia you had to go to the AEC for each isotope. They had one for cobalt, one for ^{131}I , and then we finally got an early general license, and this was a delegation of authority, to our Board where an institution had professional capability of running things and the AEC would look over your shoulder. But clearly we had that and then we could authorize users based on our criteria for training which also the AEC was looking at over our shoulder and Jack Robbins knows AEC very well.

DR. ROBBINS: I don't think I ever took the course. I came in under the grandfather clause.

DR. DAVIDSON: That's why that guy got after you.

DR. MANDEL: You were there when they invented it.

DR. ROBBINS: And just recently they're trying to get the record straight in Radiation Safety and they had to find a lot of old papers in the filing cabinet.

DR. MANDEL: Is it true, as Bruce has suggested, by that long chronology in nuclear medicine, that the AEC tended to exaggerate the dangers of diagnostic isotopes and that many of these restrictions and safeguards were not necessary?

DR. ROBBINS: They were just being careful, I think, because nobody knew what the ultimate outcome would be. And they restricted the use. When people came in with hyperthyroidism we had sharp cut-offs. Originally it was above the age of 40, figuring that it took you 20 years if you developed a cancer for it to get you, and the mean age for humans to develop was around 17 years after radiation exposure. So then you would be 77 years old and probably have been run over by a truck or something. But then, when nobody died, then they started backing it up. When I came in it was 25. Above the age of 25 you could use it. But under 25 you had to be very careful. You would use surgery and then use propylthiouracil on kids.

DR. MANDEL: Did the practitioners at that time sort of go along with this conservatism? Was everybody sort of very wary of the dangers of radiation, the possibilities?

DR. ROBBINS: The possible dangers, yes. I mean we grew atomic cocktails. That's the term they used

DR. DiCHIRO: Well, when I arrived here in 1958, Hahn's Shoe Store yet still had a device to measure the feet, '58, an x-ray device to measure the size of the feet in 1958.

SPEAKER: Well, familiarity breeds contempt. The x-ray machine had been around since the end of the last century and everybody thought they knew what it was about until their fingers started to fall off.

DR. HARBERT: But there is no question that relative to the radiation burden to patients delivered by nuclear studies versus radiographic studies there is definitely a dual standard and radiology and radiographic instruments which deliver a vastly greater amount of radiation that is received by the patient is practically delivered without oversight relative to nuclear medicine which is carefully scrutinized by several different agencies and several committees, and there is endless deliberation about very small amounts of radiation delivered that is never even considered with radiology.

DR. DAVIDSON: As a sidelight to that, when we were running our own film badge situation for all of NIH and our committee analyzed the monthly reports from the Radiation Safety Group and the high men on the totem pole were the x-ray dental technologists because they wouldn't get behind the lead screen; it was simpler to just push the button, it only took a second and they didn't realize that the radiation dose bouncing off a tooth didn't have to be in a beam. You could be at right angles to it and still get it and their badges showed this. And to the contrary, the operators of the cobalt radiotherapy source that were delivering high doses for cancer, they never showed anything because they were out at the console in control and they were never in the room with the source. So, as John says, it was a double standard.

DR. MANDEL: Dr. Robbins, you practiced in those days and you're now the Chairman of the Radiation Safety Committee. Have you seen a change in these restrictions becoming more prohibitive in doing human research here at NIH? In other words, do you think it was easier years ago to do a protocol using radioactive materials than it is now today?

DR. ROBBINS: Well, in the early days there was certainly less regulation and one could make up his own mind whether you thought what you were doing was safe or not. And Jerry Johnston was talking about the use of I¹³¹ in the thyroid and, as he said, it was used first in older people and then gradually the age was reduced. And even now we don't really know the danger of radioiodine therapy. So, it certainly became more regulated. And then there's also a difference between the use of radiation in research and the use of radiation in clinical medicine and the Radiation Safety Committee, as far as the human use is concerned, is mainly with respect to the exposure to radiation in people that are being given it for research purposes, and that includes x-ray procedures as well as radioisotope procedures. And there, there is certainly a conservatism to try to be sure that there won't be any effects from the radiation. And since most of the risks are really not known at these low levels it's a very arbitrary kind of decision that's made as to when you can and when you shouldn't give that radiation.

SPEAKER: Does a radiologist sit on your committee?

DR. ROBBINS: Yes.

DR. HARBERT: At Georgetown, during the years I sat on that committee, we never had a radiologist. In fact, I don't ever recall the radiation burden received from coincidental radiographic studies ever being considered as a risk. There's just-- There's a blind spot there.

SPEAKER: Here they kept records. I mean we looked historically for a period of time, the NIH record, and a tally sheet for exposures from all forms of radiation. When was the Radiation Safety Division dropped from Nuclear Medicine? It started out as one of the three services.

SPEAKER: I had it in.

DR. MANDEL: And Jack used that phrase earlier, "The fox minding the chicken coop." Do you all, in confidence, think it was easier in any way to do research with radioactivity because the safety or regulatory group was part of your own house, so to speak?

DR. ROBBINS: I don't think so. We had a good cross-section of people with pretty incisive minds on the committee, and when you'd present something you'd better have all your ducks in order. They wouldn't rubber stamp anything. You know? Everything had to be presented. It was thoughtfully gone over and was refused or accepted on its merits.

DR. MANDEL: Now, did the entire Radiation Safety Division, including the people that go around checking labs, belong to Nuclear Medicine until '82?

DR. DAVIDSON: Yes.

SPEAKER: Or maybe even later than that. I don't know.

DR. DAVIDSON: One of my big problems with that was that I had a budget, the Nuclear Medicine Division, and they needed a new truck and I wanted to get a second y-camera. Well, what do I get? I got a new truck because, you know, the labs couldn't deliver all the stuff and pick up all the garbage without the truck.

DR. ROBBINS: And we had to get a new compactor down there to stuff it and then beaucoup gallon drums to stuff it in, and all those things cost money.

SPEAKER: Well both of you were Radiation Safety Officers as well as Nuclear Medicine Heads, and when was it divided?

DR. ROBBINS: It must have been after Steve came. Steve decided he wanted to do that.

DR. DAVIDSON: Steve trained under the old system so he was probably smarter when he came back.

DR. MANDEL: You're referring to Dr. Steve Larson who was a resident later in the training program and became Chief here in 1983.

DR. DAVIDSON: Steve started when I left. He was my pick for a candidate because he had trained with Buznell out in Seattle. He's a crazy guy. He took a year out of medical school to work with Buznell in nuclear medicine and then went back and finished medical school. And that was why I picked him for a trainee for our program, and then I went and got lured to Duke and I left him. And he was running the show for a bit when he came.

DR. ROBBINS: That's right. He and Gary Schoen were in charge of it when I came. There was at least a 6-month gap from the time you left until I got here.

DR. DAVIDSON: But Steven had a good bit of training and he was very into this.

DR. MANDEL: The other thing that strikes me is that a lot of the advances in nuclear medicine, not just here but elsewhere, are driven by a very close collaboration with a person in one of the other medical fields who really wants to use the techniques we have. Is it fair to say that's always been true, even from the very beginning? You had a one-to-one link with an institute collaborator that served to drive the progress?

DR. DAVIDSON: Yes. That's absolutely true. In my era they had the patients. If I wanted to do anything I had to get them.

DR. ROBBINS: And then the technology in the '70s to do positron imaging finally, after the failures with the initial attempts, came into being. The group that we bought our positron imager from called me and said they had this device. And I said, "Nonsense." I had the same experience as Jack did in working with the old positron camera that Nuclear Chicago put out and I said, "I don't think you're ever going to get a handle on that technology." But they did of course. But they used quite a different approach to it. And so then positron emission tomography was off and running and Bob Kessler actually came here knowing about the basic research that was done here at NIH by Dr. Sokoloff mainly and came in and told me that that's why he wanted to come here; that we'd get a device and we'd do positron imaging. And by golly we did.

DR. MANDEL: And what year was that? What was the gap between giving up on the y-camera positron detector?

DR. ROBBINS: Well, the y-camera was in the '60s. The first ones, well, it wasn't the first ones.

DR. DAVIDSON: 1967.

DR. ROBBINS: 1967 or '68. And the positron technology came along then in about '78, I guess, '77 or '78.

DR. DiCHIRO: Bob Kessler learned from me that the success with FDG had been obtained, and I was the one who purchased the machine. Okay?

DR. ROBBINS: That's right.

DR. DiCHIRO: So in fact, positron emission technology, the real technical person behind it was Rodney Brooks. Rodney Brooks was the main person behind that made possible the first advance. But Bob Kessler, he was the one who was consulted by Dr. Lipsett and so on. Dr. Lipsett did not want to hear about this crazy Italian and so on. Later, you see, the fundamental issue is that NINDS, it was our institute, that funded it. In fact, *the* entire positron emission tomography in the country was funded by the Sabin grants given by our institute. Sabin grants. And in addition to the Sabin grants the purchase originated on my desk. But that's beside the point, because I had, by the way, many other things to do.

DR. MANDEL: Did you know, Giovanni, about their attempts with this positron y-camera in the '60s?

DR. DiCHIRO: That was purchased, as I said last time, that was purchased from our funds because there was only \$1600 dollars from the fund and there was nothing else than another monitor, a separate monitor on wheels, and you had to move it, but there was no real coincidence detection to speak about.

DR. DAVIDSON: There were a few accidents where it did.

DR. DiCHIRO: Yes. I mean randomly, that was it. But as you correctly pointed out, there was a need to be totally different approach because it was a total failure and you asked to remove the device and you correctly did so was the tomoscanner.

DR. DiCHIRO: (Begins mid-sentence) --was considered by the people who work, you know, the concept, was to obtain tomographic scanning.

DR. MANDEL: Was that the Siemens?

DR. DiCHIRO: No. Tomoscanner by Miloud (sp?) this physicist who is now, by the way, still in Cleveland and he has become an M.D. now. He has taken now also his M.D. He was a physicist under Brownell.

SPEAKER: I think he's in charge of Nuclear Medicine there now.

DR. DiCHIRO: That's right. He's now in charge of Nuclear Medicine and he worked with Brownell. And he came here. And so I consulted him. And there was a committee headed by Brownell to assess the design that Miloud established to obtain this. This was a gigantic machine. Jerry was very kind to us to give us this prize room in this space and we put this gigantic machine in. And after six months Jerry said, I mean, we are

not doing anything and it was eliminated. It was one of the biggest defeats of-- I mean; it was an enormous amount of money. He used a rather original approach which was known as the Davis approach. The Davis approach was you detected-- The trick was to detect the source of the coincidence, you see, where the coincidence had taken place. You see? And you did that with a long rod. Remember that long crystal? There was a long crystal and then the coincidence, how did it work out?

SPEAKER: Time of flight?

DR. DiCHIRO: Yes, a primitive type of time of flight. It was a very long wafer. And you could grossly detect where the coincidence had taken place, you see, a little bit like the slice selection by Mansfield in nuclear magnetic resonance, the same principle. That was a total failure. Also, Miloud was not a dull physicist. I mean he's fairly bright. But it was a piece of- (unintelligible.) And was, as I say, 2,500.

DR. MANDEL: And put together by the Machine Shop?

DR. ROBBINS: No. Cleveland. It was put together in Cleveland and they took it apart, brought it here, and reassembled it.

DR. DiCHIRO: Yes. Our instrument shop and Jerry, in the beginning, he was all for it, like I was.

DR. ROBBINS: Theoretically it was great.

DR. DiCHIRO: Then the thing failed miserably.

DR. MANDEL: What year are you talking about now?

SPEAKER: '75?

DR. DiCHIRO: No. It was before the introduction of the-- So it was-- It must have been '73, 1973.

DR. MANDEL: Had Sokoloff come up with the FDG method?

DR. DiCHIRO: No, no.

DR. MANDEL: What were you trying to image?

DR. ROBBINS: No. This was just for tomographic imaging.

DR. DiCHIRO: This was just for tomographic imaging, not positron emission. I mean with a standard radionuclide.

DR. ROBBINS: No. We weren't thinking about positrons then at all. We were just into tomographic imaging.

DR. MANDEL: So, suddenly, between '75 and '78, people here became excited about positron imaging?

DR. DiCHIRO: Yes. In '77 came the famous article by Sokoloff and then immediately on the C-deoxyglucose technique. They published after, but it was rejected by *Science*, and he published the article in *The Journal of Neurochemistry* in 1977. And then behind more than 10 years of work, his work, and also the work of the Director of our Institute Dr. Tower, who had worked on the principle of this peculiar analog of glucose that is the deoxyglucose. It has a very peculiar behavior, you know. And so, it had a very long background. And Dr. Reivich, who had studied for a brief period with Sokoloff, he thought to tie the deoxyglucose with what was a positron emitter that was close to hydrogen, okay, to carbon, and fluorinating came along and so Reivich asked Dr. Wolfe at Brookhaven National Laboratory to create a pharmaceutical that could be used for a technique to study with deoxyglucose.

DR. MANDEL: Was Reivich still here then with Sokoloff, or he had already gone?

DR. DiCHIRO: He left for some time. And then the paper was presented in Denmark and I showed to Kessler the abstract and said, "Look what is happening." And, of course, he picked up the idea and he moved very fast. By the way, on the machine, the positron emission tomography machine, they had already been working with mostly and there was the involvement of TerPagossian and TerPagossian called with this group, Phelps and Hoffman and they joined TerPagossian who had been doing the original attempts and so on.

DR. MANDEL: You bought here an ECAT-2, if I recall, in '78. Can you remember how many other cameras, positron cameras, were in the country at that point?

DR. DiCHIRO: The Sabin-23.

DR. MANDEL: They were funded by the ECAT-2

DR. DiCHIRO: Essentially our institute was the one that—— I don't believe the Heart- - When did-- When did Bechtal come? I don't know.

DR. ROBBINS: It came along subsequently. I think Phelps too was doing that in St. Louis at TerPagossian's lab before he went out to California.

DR. DiCHIRO: You're right.

DR. ROBBINS: And they picked it up in California.

DR. DiCHIRO: But they had started with 15 before that, but when our institute became very interested when there was this marriage of the Sokoloff autoradiographic technique and positron emission tomography. So,

fundamentally the three nuclei were the machine, mostly Sam DAVIS, the radiopharmaceutical, mostly developed most totally at Brookhaven by Wolf and Fowler—Wolf was assigned to Dr. Fowler. There was Fowler and Ito, this Japanese who was very obviously Japanese and then the various groups that applied for the grants.

DR. MANDEL: Did you make plans here then to start a cyclotron facility at the same time you made plans for buying the camera?

SPEAKER: Actually, the cyclotron was something that was offered way back in 1971, but the notion was that a cyclotron would be bought and Nuclear Medicine would run it in our spare time. You know, “We’re not going to give you any more people.” Now, when you run a cyclotron?

DR. MANDEL: What was the driving force behind the desire to have the cyclotron? Was it an institute project?

DR. DiCHIRO: No, because we were using the cyclotron that was located in the Naval Research Laboratory in Anacostia. So, we went there and that was then for the production. But it was more efficient to get to the isotope.

DR. MANDEL: This is the late ‘70s now that you’re talking about?

DR. DiCHIRO: That’s right.

DR. MANDEL: What was the motivating force in ‘71 when they first offered a cyclotron?

SPEAKER: The notion that possibly it could be used for neutron therapy and, you know, it was a little bit cloudy, but if we get a cyclotron we’ll be able to use it in radiation oncology, we’ll be able to make isotopes. You know, whatever one does with cyclotrons. But then that got scotched very soon when it became obvious that it would take quite a bit of money to get a cyclotron and run it. And then it re-emerged in connection, as Giovanni was indicating.

DR. MANDEL: Was that a radiation therapist then in ‘71 that was the person trying to get this, or was it a group that all had a little bit of interest?

SPEAKER: It was the group.

DR. DiCHIRO: Incidentally, Jerry, the very first cyclotron in the hospital was at Hammersmith Hospital in London. That was the very first hospital worldwide that had a cyclotron within the hospital.

SPEAKER: And what were they using it for?

DR. DiCHIRO: To get 15•

SPEAKER: I think that the reason the project might have been aborted in 1971 was that there were very big budget cuts those years, in 1970 and 1971.

SPEAKER: Yes. It was rather superficially discussed, you know, and if it was going to cost any more than whatever the basic amount was that was being offered then we shouldn't consider it. And it would have cost a lot more than that. They hadn't even considered housing it, where they were going to put it. They wanted to take out the Vandergraff and put it on the ground floor behind the Vandergraff shielding. That would have been adequate. The room, though, would have been the right size. We measured it and looked at it and I talked to people. It really wasn't a very serious thing back then. But it did get very serious when it was obvious that this was the only way we were going to get fluorodeoxyglucose was to have a source of fluorine that was readily available to us.

DR. MANDEL: In terms of growth of the department over the years, Jack made proposals for a number of FTE, space and equipment he needed. He didn't get it. He left. Suddenly a lot of it appears. Was it true, during the time you were chief, that threats had to be made of leaving, or did you experience growth?

DR. DAVIDSON: Mine wasn't a threat. I got told. Duke University said a 40 percent higher salary and you're sending your kids to school on our nickel.

DR. MANDEL: But the equipment came then after they had to go find a new chief?

SPEAKER: It ran very well.

DR. DiCHIRO: You expanded the--(unintelligible.)

SPEAKER: And we had great support. All of the institutes supported us. And if there was a problem with funding, money would appear from Cancer or from Neurologic Disease and Stroke, from Heart, because different people were interested as we went along. And actually it was a very pleasant experience. We wanted for nothing.

SPEAKER: And the field was expanding tremendously. It was growing and it was tremendously interesting and we had many, many projects running with almost everybody in the place that was-

DR. DiCHIRO: And Jerry allowed me to place a CT scanner in the Department of Nuclear Medicine. So we placed an extra CT scanner, because when CT was introduced, sort of in ' , we got these EMI, a new generation, a second generation, of EMI scanners, which we purchased and there was no space in the X-Ray Department, and John Doppman didn't like that. And I said to John, "We have too many things going on, and so we've got space down there," and so, I mean, the Department of Nuclear Medicine expanded tremendously.

SPEAKER: The expansion of the PET scanning is probably one of the reasons I got involved in the Radiation Safety Committee, because Steve Larson was then Head of Nuclear Medicine and Chairman of the Radiation Safety Committee, and he cornered me in the hall one day and said, "I feel very uncomfortable that the great majority of the research projects that are coming before the Radiation Safety Committee are for PET scanning with all these normal controls, and I feel like there is a bit of a conflict of interest," and planted the seed in me to take over. I'm sure that wasn't the whole story, because he was also getting tired of the effort that it took. And the reason it was taking so much effort is that during this whole period there was a great expansion of the regulations and ideas related to research with humans, so that there was a whole new overlay of review boards and application requirements, and so it became very onerous for him. But there were two things, and one of them was related to the great increase in the PET scanning. Meanwhile most of the other use of radioisotopes had become really part of practicing medicine, so it was more like the Radiology Department rather than research.

DR. MANDEL: One thing that we didn't touch on that I'm curious about a little bit is the demise of the whole body counters in terms of they were built, as Jack told us earlier, at apparently great cost in the post-World War II period. John Harbert was recruited specifically from the West Coast because he had some knowledge of their application, training that he told me privately about, but you might want to just discuss that a little bit, John, for the tape.

DR. HARBERT: Well, you mean about my going to the Radiological Defense Laboratory? Well, I was really trying to avoid going to Vietnam, which I turned out not to do--I did go to Vietnam--but I convinced my Chief of Medicine, who was Franz Bower then who was Chief of Medicine at Harbor General Hospital and later Dean of USC, to try to get me a job doing research. And so he had written one of the first books with Dick King, who is at Navy now I think, and so there were very few jobs in the Navy for research billets without prior residency training. In fact, there were only 17 at that time. But, at any rate, he was successful and I went to the Radiological Defense Laboratory in San Francisco, but where Candlestick Park is now. And it was terrific. But they didn't know what to do with me. They really didn't. They were sort of told to hire me and, you know, "You'll find something to do with the young man." So, one of the things that I did was to go over to the Army Hospital in Oakland, Oakland-- Because they also had a naval air wing there. But it was an Army hospital in Oakland.

SPEAKER: Alameda?

DR. HARBERT: Alameda, maybe it was Air Force.

SPEAKER: It was Air Force.

DR. HARBERT: Air Force. Yes, Alameda Air Force. But they also had a naval wing. But, at any rate, they were trying to-- They had a whole body counter because they were trying to solve the problem of taking weight off pilots without taking muscle off. And so the way that this was done was to--the idea was--to try to lose weight without losing potassium, because this was practically the only way of measuring lean body mass was to measure potassium⁴⁰ which exists in a fixed ratio with potassium ³⁹. And so I learned something about this, not very much actually because when I came here I think I represented myself to Jack as probably considerably more experienced than in fact I was.

DR. DAVIDSON: You got flushed in the beginning.

DR. HARBERT: But the pact that Jack and I made was that I would run the whole body counter if I could spend most of my time in clinical internal medicine, which is what I was interested in. Because at the same time that I was working in Oakland I was also working out at the University of California Hospital in the Nuclear Medicine Lab there. And so I was learning and of course there was no other way to get involved in nuclear medicine then except by OJT.

DR. MANDEL: Was there a great deal of interest then here in the body counters when you came? Were they already built?

DR. HARBERT: Well, there was some. But Jack's got the figures there. There wasn't a great lot of activity. And I have to say that I certainly never did anything inventive. I never encouraged any protocols. It was just sort of my ticket. It was my meal ticket.

DR. DiCHIRO: You had the physicist. What was his name, the Scandinavian?

DR. HARBERT: Roger Ainmett. Yes. He came after.

SPEAKER: Roger had a Ph.D. in whole body counting from Rochester, and he came here when I did and ran the whole body counter most of the time I was here. And he was very innovative.

DR. MANDEL: Does anyone remember what the push was to build these things?

DR. HARBERT: Well, there were two primary uses. One was contamination, because one of the problems in building these things was that the steel had to have been founded before the first atomic explosions because after that virtually all the steel that was manufactured in the world was contaminated with cesium, which has a 30-year half-life. Not very much. But you can detect, you know, 10 almost 10-microcuries of cesium with this thing. I mean it's extremely sensitive for cesium. And so these were constructed.

DR. DiCHIRO: You mean after Hiroshima and Nagasaki?

DR. HARBERT: Yes. It would just plate onto the side.

SPEAKER: And the testing that took place.

DR. HARBERT: And the testing, that's right. And so these were pre-World War II battleships.

SPEAKER: You mean the steel?

DR. HARBERT: Yes, that they cut up and made into-

SPEAKER: Pieces of armor?

SPEAKER: Yes, 5-inch thick steel.

SPEAKER: They didn't remold it or anything, and the copper plating?

DR. HARBERT: That was put in by Roger Aimnett to take away the scatter, you know, so that when y struck it you wouldn't get any back—scatter.

SPEAKER: From cosmic radiation.

DR. DAVIDSON: One of the big trials of his was the concept of putting it down in B because they figured way down there'd be no radiation getting down there, but all the damned concrete of this building has radioactivity in it. It turned out we had a hot spot down there

SPEAKER: And the surrounding was the tiles.

DR. DAVIDSON: The ceramic tiles. Yes, it had this uranium glaze on the tiles. Radioactive tiles, oh boy.

DR. MANDEL: Did this inhibit the use of it, John? Was it less accurate then?

DR. HARBERT: No. It was very good.

DR. MANDEL: And when did you start to see the demise of its use? We have a card there that says there was 1,507 patient studies, and what year was that?

DR. DAVIDSON: That probably was dictated by a call from the front office. You know, "What are you doing? I want to know how many and why?"

DR. MANDEL: And what year would that have been?

DR. DAVIDSON: This was 1967, 1,507 studies, 88 with calcium 47, 517.

DR. HARBERT: This is probably Fred Barter's group.

DR. DAVIDSON: Yes. And then Mental Health and NINDB and some surgeons were doing K40s, 517, sodium24, that was Les Bayer. Wasn't he a dentist?

DR. HARBERT: I don't know.

DR. DAVIDSON: His name was here and I remember the guy. He ran it. And he had 283. And we used it for 8chromium platelet survival times, tagging platelets with 9 chromium. No Fe59 that year. 1131retention studies, 182.

DR. MANDEL: Those were your studies, weren't they?

SPEAKER: Yes, but John had set up a...

DR. HARBERT: A shatter shield, yes.

SPEAKER: For counting patients with fairly large doses of 1131.

DR. DAVIDSON: That body burden was done on 70 patients, and that was probably Health Physics workers. And thyroid is a separate item, 104, and P32, we did 42 patients with Clyde Brunstrom.

DR. MANDEL: When would you place the demise of the whole body counter?

DR. HARBERT: I was going to ask Jerry whether he saw a shift over to Radiation Safety uses where you're just counting workers, because certainly when I arrived in '85 it was primarily used to monitor workers.

DR. BROWN: Well, Roger went over to Administration. You know, he took one of those Administration Internships and got away from it. But he was doing all kinds of studies in addition to counting the radiation workers. One of his ongoing studies was the zinc study, do you remember, with Hankin, the taste study?

DR. DAVIDSON: And Sjögren's syndrome. We did all the salivary glands.

DR. MANDEL: Why did this technology, which was reasonably expensive in its day, fade out of the picture like so many of the others we talked about earlier?

DR. BROWN: You have to have somebody, I think, fueling it, much as we said with what drives the different studies that are done and the direction that nuclear medicine takes. It's the interest.

DR. HARBERT: When I went to Georgetown after being here I had to invite visiting professors just to come in and advise, like, pediatric nuclear medicine, we never did these studies here. And so there were large gaps in my understanding and my practice.

DR. DAVIDSON: Be glad you had big gaps. I did pediatrics at Duke and I'm still liable for malpractice. It doesn't start to run until they're 21 years of age, and in North Carolina it's 6 years for tort. So, I haven't been sued, but I mean technically.

DR. HARBERT: So, at any rate, I would just, you know, invite visiting professors to come in and give a talk and then we'd sit and talk about, you know, how they did things and set up a program.

DR. DAVIDSON: We did that at one point. Didn't you ever do it at one of the other hospitals? We had something set up with somebody.

DR. HARBERT: I went to spend some time with Masood on mono.

DR. DAVIDSON: We didn't have OB.

DR. MANDEL: What was NIH's Nuclear Medicine role in the greater Washington area community? Did it take the lead in areas, or did you have conferences?

DR. DAVIDSON: We had a club, a Scan Club, and we'd meet and, you know, go somewhere for a luncheon and there would be the Baltimore gang and the Washington gang, and it'd be an evening thing and maybe take off at 4:00 and have a meeting and a meal together. It was a pretty small community really.

DR. MANDEL: Was that true in the '70s then?

DR. BROWN: Well, we sort of ran the Society of Nuclear Medicine's local chapter through the '70s. I was president for a couple of years and vice president and Eric was in the thing. So, through most of the '70s that was our society. And we presented most of the papers.

SPEAKER: The pre-Henry Wagner era where—

DR. BROWN: No, we muscled Henry out for a while.

SPEAKER: Henry had run out of isotopes?

DR. BROWN: His guys didn't show up at a couple of meetings. We had the whole darned thing. But they've come back.

DR. ROBBINS: Before Nuclear Medicine, when I first came here, Monte Greer was working in the Cancer Institute with Gloria Hurwitz and they started thyroid uptake and scanning, which was nuclear medicine, I guess, at D.C. General Hospital and I joined in with them and we were together for about half a dozen years.

DR. MANDEL: So you didn't come directly from New York then with the group that moved down during that period?

DR. ROBBINS: No. I came directly here. That was extracurricular.

DR. MANDEL: Oh, it was a club, so to speak?

DR. ROBBINS: Well, not a club, but just donated service to help another hospital. And most of what we were doing here was laboratory bench work and not clinical.

SPEAKER: We had a succession of people that came through and spent different amounts of time over here, some up to six weeks, learning techniques.

DR. MANDEL: Now, John Harbert was the first resident and then there are gaps in our understanding after.

DR. HARBERT: Well, I have always represented to the Board of Nuclear Medicine that that's what my role was, but I was actually considered a staff member.

SPEAKER: Staff?

DR. HARBERT: Yes, I was on staff.

DR. DAVIDSON: I got Joe Ashburn, and he finished his radiology residency and he was gung-ho about it, and then we recruited one job slot and that was you, and we swiped two technicians from Radiology and that was it. And then we gradually got down, down, down, and we had nothing. You left and that was when Masur said, "Davidson, you're it hours a day, days a year." And that's when Bill Briar went to Duke and Jack Goodrich down there wanted an extra pair of hands and they made me this offer and I was vulnerable.

DR. MANDEL: But Steve Larson is here then at this point as a bona fide resident?

SPEAKER: No. He was a Clinical Associate.

DR. DAVIDSON: No. He didn't come until I was gone. He'd been appointed.

DR. MANDEL: So, let's say, if we don't count these early people, then Jerry, when did the first real residents come on board?

DR. BROWN: We never had any real residents. The thing that we did was the people were coming here in the yellow beret for their two years of service as Clinical Associates. We applied to the Board to give them credit for training during their time that they were here. And so then they were given credit for years of nuclear medicine and they went and took their boards.

DR. MANDEL: They would have Institute Fellows then that were here under the PHS?

DR. BROWN: Right.

DR. MANDEL: And they rotated into Nuclear Medicine for the experience.

DR. BROWN: Well, they came to Nuclear Medicine full time. We had two or three guys every year coming in.

DR. MANDEL: Institute FTEs that were assigned then for a two year period?

DR. BROWN: But they were Clinical Associates.

DR. ROBBINS: This was going on all over the NIH hospital in other departments. People would come for Clinical Associates and not-- I mean the Clinical Center has never had a residency system, you know, where you have it in Nuclear Medicine and Radiology and you still don't have it in the rest of the NIH.

DR. DiCHIRO: We had residency until '72, then Doppman eliminated it when he came back. But we had two years residency in radiology up to '72.

DR. MANDEL: And what was the reason for eliminating that? Maybe one last thing we could touch on with Jerry, and not everybody needs to stay, is the design of this area, which is intriguing.

DR. DiCHIRO: It was a good.

DR. DAVIDSON: Good to see you, Jack. This was a great day.

DR. MANDEL: There are more pictures actually there. I don't know if you've seen them all.

DR. DAVIDSON: And you have the picture of the whole body counter.

End of Transcript