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Dr. Martha Linet Oral History

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National Cancer Institute

Division of Cancer Epidemiology & Genetics

National Institutes of Health

Oral History Project

Interview with Dr. Martha Linet

Conducted on October 26, 2022 by Holly Werner-Thomas for

History Associates, Inc., Rockville, MD

HWT: Okay. Let's get started. My name is Holly Werner-Thomas, and I'm an oral historian at History Associates Inc. in Rockville, Maryland. Today's date is Wednesday, October 26, 2022, and I am speaking with Dr. Martha Linet for the National Institute of Cancer Division of Cancer Epidemiology and Genetics, part of the National Institutes of Health, or NIH. The NIH is undertaking this oral history project as part of an effort to gain an understanding of the National Cancer Institute's DCEG. This is one in a series of interviews that focus on the work of five plus now individuals at the NCI DCEG, including their careers before and during their time with the institute. This is a virtual interview over Zoom. I am at my home in Los Angeles while Dr. Linet is in the Bethesda, Maryland area. Before we get started, can you please state your full name and spell it?

ML: Well my full name is Martha Sara Linet. Martha M-A-R-T-H-A, Sara S-A-R-A, Linet L-I-N-E-T.

HWT: Thank you. Dr. Linet received her medical degree from the Tufts University School of Medicine. She completed a three-year residency in internal medicine and a Master of Public Health (MPH) degree and postdoctoral training in epidemiology as well as a three-year residency in general preventive medicine from the Johns Hopkins School of Public Health. She is board-certified in both internal medicine and general preventive medicine. Dr. Linet served as both assistant professor and was subsequently promoted to associate professor in the Department of Epidemiology at the Johns Hopkins School of Public Health. She became internationally recognized as an expert on the determinants of adult and childhood leukemias, having published *The Leukemias' Epidemiological Aspects* with Oxford University Press in 1985, which is considered a key text in the field. In 1987, Dr. Linet joined the Biostatistics Branch in what was then the Epidemiology and Biostatistics Program in the Division of Cancer Etiology at the National Cancer Institute (NCI), where she made landmark contributions in etiologic studies of myeloid and lymphoid hematopoietic neoplasms, and in epidemiologic studies of ionizing and non-ionizing radiation and cancer risks. She received tenure and became a senior investigator in 1993. Dr. Linet served as acting head of the Analytical Studies Section between 1994 and 1996. In 1996, she transferred to the Radiation Epidemiology Branch (REB) in the newly formed Division of Cancer Epidemiology and Genetics (DCEG), She was appointed as chief of the Populations Studies Section, REB in 1999. Dr. Linet was appointed as acting branch chief of the REB in 2002 and was chief of the branch from 2004 to 2014. After 2014 she continued in her position of senior investigator before retiring in January of 2020. Dr. Linet's research contributions include more than 450 peer-reviewed scientific publications, as well as four books, more than 20 chapters and many other publications and contributions. She has been honored with the HHS career achievement award, NIH directors' awards, NIH and NCI merit awards, and an NCI mentoring award. Dr. Linet was inducted into the Johns Hopkins Society of Scholars and the American Epidemiological Society. She received the outstanding contributions to epidemiology and distinguished service awards from the American College of Epidemiology. Dr. Linet was also awarded a Henry L. Moses first prize for publication in the clinical medicine category. In 2020, she was appointed NIH scientist emerita and special volunteer in REB.

Okay. Let's go ahead and get started with the questions. I always like to go back just a little bit for people to describe a little bit about their family background. Especially if they had influences, and not everybody does. If you did, can you describe a little bit about your family background, where you grew up, who influenced you and whether you had mentors or other support systems, for example.

ML: How I got into epidemiology will be the longest part of our discussion, and my answers to your other questions will be more concise. I grew up in a blue-collar family in Cleveland. My father was a World War II veteran, and my mother had four children and was a homemaker. I learned decades after my father left the military, when information became unclassified, that he was not only a prisoner of war for nine months after being captured in the Battle of the Bulge in Germany but was subsequently transferred to a slave labor camp in Germany, where he was one of the few survivors. Among the people that he served with was Kurt Vonnegut. I found the whole story unbelievable, but there is a book published a couple of decades ago confirming this story. I think this gives you a sense of where I came from but did not know about for many years.

My father lost a third of his weight in the slave labor camp. After hospitalization in the U.S. he was about to be posted to the Pacific theater, when the war ended. He then joined the family business that manufactured wooden whiskey barrels. My father oversaw the transportation by trucks of these barrels to various places in mostly the Midwest and Kentucky.

My mother was a homemaker who was obsessed with her children's involvement in classical music. For many years of my childhood I spent hours each day practicing piano, clarinet, and then viola. I had very little involvement in science.

After high school I enrolled in Brandeis University. One of the reasons that I chose Brandeis was that the university offered me a full scholarship. With four children in a blue-collar family, that was very important. I changed my major two to three times, but four seminal events finally led me to apply for medical school. The first was that during my freshman year my roommate was severely injured in a car accident and rendered a quadriplegic. I visited her on many weekends in New Haven at a rehabilitation center and began to understand what medicine could and could not do.

The second important event related to lack of a high-school course in biology due to time constraints from hours spent daily practicing music. When I took biology in college, I was behind with regard to terminology and content, which was a bit of a struggle. An assistant professor in biology tutored me individually until I caught up and encouraged me to take chemistry courses.

The third event was a summer course after my sophomore year in organic chemistry at Case Western Reserve University that had been encouraged by my college biology professor. I loved the content, and I was the top student in the class.

The fourth seminal event occurred in my junior year in college when I was invited to live with our Cleveland neighbor's family in Nairobi, Kenya. The couple, she was a pathologist, and he was a mathematician, left Cleveland to teach at the medical school of University College, Nairobi. I spent the second half of my junior year with them. I took my first flights abroad travelling by myself and decided to see the world on the way to and from Nairobi. I spent time in hostels in Rome, Athens, and Greek islands traveling to Nairobi. Coming back from Nairobi, I spent time in Isfahan, Iran, Paris and London.

I attended University College, Nairobi studying botany and physiology, and also went on a series of adventures. This was 1960s! I hitchhiked over all of Kenya, Uganda, Tanzania and the Congo and did not take antimalarial drugs. The pathologist should have suggested these but did not; fortunately I did not acquire malaria. Anyway, it was a tremendous experience as you can imagine and introduced me to the world. I've enjoyed international travel ever since.

Upon returning from Nairobi to finish my senior year at Brandeis, I decided to complete the requirements for medical school. I applied to, was accepted in a few medical schools, and attended the University of Michigan. The culture was stressful for women who wished to enter medical school. I remember at one of my medical school interviews being asked how I was going to combine medicine with motherhood!

I married right after college. My husband worked in Boston, so I transferred from the University of Michigan Medical School to Tufts University School of Medicine for my second year. I loved living in Boston and attending Tufts. It was an unusual medical school with a pass/fail program and no grades. Instead of causing people to study less, the medical students studied more because we didn't need to focus on grades.

I was a top student. The class at Tufts, like most other medical schools, included only 10 percent women. I thought I was comfortable in medical school, but parenthetically, 40 years later at my 40th medical school reunion, two or three of my male classmates apologized for treating the female students and female faculty poorly, which I thought was an interesting admission.

When my then husband accepted a teaching position at Yale as an assistant professor of mathematics, Tufts agreed to allow me to spend my senior year of medical school at Yale. Thus, I had the opportunity to spend substantial time at three medical schools.

One of the impressive things about Tufts that had a profound effect on my professional life was the strong research and clinical group of investigators in hematologic and lymphoid malignancies. These included a prominent female hematologist, Jane Desforges, and a leading male hematologist /immunologist, Robert Schwartz. The latter became an editor at the New England Journal of Medicine and ironically was the associate editor who handled a paper I published many years later in the journal. These and other professors in hematology/oncology/immunology inspired me and a disproportionately high fraction of my class at Tufts to specialize in this area of medicine.

After medical school, I entered a residency in internal medicine. I had hoped to 'match' with the internal medicine program at Yale where my husband worked, but instead 'matched' with Montefiore Medical Center in the Bronx. I was initially disappointed, but the training in the North Bronx at Montefiore and the required three months annual training at an affiliated hospital in the South Bronx was incredible. We saw diseases of many immigrants coming into the country and treated a diverse and relatively poor population that one would never see at a place like Yale. In fact, it was tremendous training.

I was one of three women residents out of 43 residents in the initial class. By my third year, I was the only female resident left (the other two entered specialty fellowship training), but the department head did not choose me for a third-year position within the pyramidal system. I fought the decision and prevailed. After completing my training in internal medicine, I passed board exams and was certified in internal medicine. The question was what to do next.

On a whim I decided to attend a three-day symposium sponsored by the National Cancer Institute called Cancer Epidemiology and the Clinician. The presentations caused a 'lightbulb to go off'. I was smitten and decided that that was going to be my future. That's when I moved on to Johns Hopkins to obtain a Master of Public Health degree. This was a turning point and I believe an unusual pathway to becoming an epidemiologist.

HWT: I love the whole arc of your story, but I do have, of course, a few follow-up questions. One is you just mentioned that you attended this symposium from NCI on a whim. Can you tell us a little bit more about that? How did you hear about that, for example, and what made you attend it? Was there something else? And why were you smitten? Why did a lightbulb go off?

ML: Well, before attending the Cancer Epidemiology and the Clinician symposium I had thought that I wanted to obtain training in infectious diseases and practice this specialty as a clinician. This was around the mid-1970s, the era before AIDS. General interest in infectious diseases as a specialty was not high. Even though we didn't have the internet at that point in time, the training programs would send around flyers to inspire the residents and the fellows about what they should do next with their career. As I began to see that infectious diseases might not be a good career trajectory, I began to think about how I could combine infectious diseases with a chronic disease focus. At that time, it was recognized that several types of cancer were caused by infectious organisms. I thought if I focused on cancer epidemiology, I could still spend a fraction of time investigating infectious causes of chronic diseases. And that would be a way to incorporate infectious diseases within my work. But coming from a modest economic background, I was concerned about financial support for my training in public health. As a resident in internal medicine, my salary had been small. The National Cancer Institute (NCI) had training fellowships, so my thought was to obtain a fellowship for my MPH training and to continue with training in cancer epidemiology as I received a stipend. NCI had an excellent outreach program to convince people to enter cancer research, aiming to recruit the best and the brightest. And NCI sought to recruit physicians to enter the field of epidemiology because by that time few physicians became epidemiologists; most epidemiologists were PhDs. I am one of a limited number of physician epidemiologists. I believe it was good outreach by the National Cancer Institute and addressed my pragmatic concerns about who was going to pay for my training in epidemiology.

HWT: Fantastic. I'm glad I followed up. I have a couple of questions that you've answered in part, but I'm going to ask them anyway and see if we can flesh them out a little more. For example, you mentioned that 8 percent of students at your time at Tufts were women. I wanted to ask you how being a woman affected your choices, your experiences, and your plans when you were first starting out.

ML: Well fortunately, the women medical students at Tufts were friendly, helpful, and supportive. Tufts is in essence a regional medical school whose graduates frequently practice in New England after graduation and training. Tufts medical school focuses on training of clinicians and is not a major research institution, such as Harvard Medical School. Although the male medical students were not terribly supportive, because they were motivated to treat patients, they realized they were going to have to get along with women in the class because we were going to be some of their work colleagues. However, it was interesting to hear 40 years later from a couple of my classmates that they were ashamed of their behavior toward their fellow women medical students and the women faculty. (*laughs*)

HWT: Fascinating, actually.

ML: Yeah, it is.

HWT: I just want to mention as an aside that for some reason your screen is moving a little bit and just to be aware that we want to keep it as straight as possible. If it's stable, it's helpful. I mean, this is mostly going to be used for audio, but the video will be available.

ML: Sure.

HWT: I also wanted to follow up by asking you, and again, you've answered this in part, why you chose internal medicine and then added public health and epidemiology to that. If there's anything you wanted to add?

ML: Well, as to the choice of internal medicine (residency during 1973-76), I've always been interested in improving patients' lives and trying to forestall the downward trend with aging. What I liked about internal medicine was the idea of keeping people healthy as long as possible. However, but I was always interested in causes of disease from early on in medical school. I hadn't really thought through the idea that if you identify causes, you can do a lot for prevention. I guess in the back of my mind was the idea of prevention, but 'causes' were appealing. I think the issue was, after I'd treated about 2,000 cases of high blood pressure as a resident, you know, it gets a little bit tedious. I began to realize that maybe I could make a bigger difference on a larger scale through studying causes more generally, and thus have more of a positive effect on prevention. This thinking moved me into the public health area. It was initially hard to envision since I felt that I was a better clinician (in particular a great diagnostician) than an epidemiologist. Because I was still ambivalent about transitioning to work in public health, I spent one year (1979-1980) in a fellowship program in oncology at the Johns Hopkins Oncology Center. After a year, I left the fellowship training, but continued to work a day per week seeing patients until shortly before my oldest child was born (1983) But clinical practice in internal medicine and oncology just didn't 'float my boat.' It felt like every day was going to be like every other day, so how could I make a bigger difference? That's how I edged into first a Master of Public Health and then post-doctoral training, and subsequently a career in epidemiology. Then I got lucky, which we'll talk about in the next phase of my career.

HWT: We'll come back to that. But I wanted to ask you first, you know, you mentioned you had first thought in the 1970s about infectious diseases. But this was pre-AIDS. And you began to think of other courses to move toward. Why, however – you mentioned cancer. Why, however, leukemia? What drew you to the leukemia research at Johns Hopkins? And can you describe that contribution? And also, if you had particular mentors or experiences that led you in that direction.

ML: Well, I mentioned that my medical school was very strong in clinical care and research in hematologic diseases. So that was the first influence on my career choice. When I went to Johns Hopkins, there was, as there is today, a lot of interest and a lot of research on what we call the solid tumors: breast cancer, prostate cancer, lung cancer, colon cancer, but there were no senior epidemiologists who had carved out the area of the epidemiology of hematologic diseases.

At Hopkins, one of the lucky things that happened to me was having a fantastic second mentor (my first mentor was terrible), Moyses Szklo. He was Brazilian, a cardiologist and focused primarily on cardiovascular epidemiology. He also recognized that there was so little going on in the epidemiology of leukemia and hematologic diseases that he started to do some initial work in this area as an assistant professor. I was very fond of and impressed by him, and we carried out several different epidemiologic studies of hematologic diseases. After a couple of years of this—which I'll describe in a few minutes—he had agreed reluctantly to write a book for Oxford University Press on the leukemias. However, years went by, he was not passionate about the topic, and so nothing happened. At a certain point after we had conducted research in this area for several years, he told me about the book and spoke with the editor of the series who asked if I *would I take the book over?* I was very enthusiastic about this idea and decided it would be a great way to really dig in, learn a lot more on the topic, and develop expertise on the literature. I was already developing hands-on fieldwork experience, but one needs to research the literature and understand the history of it and everything that had been done. Writing a book was an opportunity to fill in that part of what was missing in my background and training.

Back to the fieldwork, the way I became involved in these studies was odd. Dr. Szklo had received an RO1 grant to study aplastic anemia and acute myeloid leukemia. As part of the Master of Public Health degree at Hopkins, I was required to take a grant writing course. I decided to write a grant proposal to study postulated etiologic factors for chronic lymphocytic leukemia, a hematologic neoplasm which has since been reclassified as a type of lymphoma. Much to Dr. Szklo's astonishment, mine and that of the department chair, I won the grant, a substantial amount of funding. But I was felt to be too junior to be the principal investigator, and thus I was designated as the project director (with Dr. Szklo as Principal Investigator). I then negotiated with the chair of the department for a faculty position. I said, "I brought all this funding into Hopkins. You need to do something for me in return. You need to give me a faculty position." I was not a shy person. (*laughs*) I negotiated becoming an instructor and agreed I would be willing to teach. But also, I wanted this position to be able to do research and to start my career.

Dr. Szklo and I worked on this grant together. With the combination of the funding he generated for aplastic anemia and acute myeloid leukemia and the funding I brought in for chronic lymphocytic leukemia, we conducted studies on in the greater Baltimore area. Dr. Szklo was an assistant professor at that time, I was an instructor, and we just had a great time working together. We saw eye to eye. We came up with high quality methods and study designs for these studies. It was a tremendous experience working with Dr. Szklo.

HWT: You also mentioned that you got lucky at some point. What was that?

ML: You generally don't get awarded hundreds of thousands of dollars for the first grant proposal you put forward, and this wasn't a small beginning award; it was a full-fledged RO1 award. That is the type of award that many investigators do not receive until they're in their late 30s or early 40s. I was in my early 30s. To win the hundreds of thousands of dollars to do this study was impressive. That's why it was easy to talk the chairman into " I brought in this money, which would support 80 percent of my salary, so, you owe me." (*laughs*) Because the money goes to the university, the grants recognize the principal investigator and the project director as such, but the award is made to the university and therefore the department receives the 'credit.'

HWT: Can you take just a moment before we move on, diving in more to your work at NIH, to reflect on that choice? Because you could have gone in different areas, starting from early on as a student through this time when you'd received this massive award, and focused on leukemia. How did you feel at that time? Can you explain that a little bit about those choices?

ML: Well, remember that my training grant was from the National Cancer Institute for cancer epidemiology. It was only appropriate when I wrote the required grant proposal that it be focusing on a cancer. And because of my interest in the etiology of hematologic cancers and the fact that I was working with Dr. Szklo on epidemiologic studies of hematologic malignancies my objective was to write a grant proposal to cover a hematologic malignancy we weren't yet studying. I was pleased. I saw this as a terrific opportunity to continue working with somebody who I really had high regard for, that we'd already worked together, and we had the opportunity to work on another hematologic malignancy project together. We saw that there were synergies and that we could take advantage by f setting up one infrastructure of personnel to carry out these different research projects. So, it felt comfortable and rewarding. At many universities if assistant professors do not bring in funding within three years, then their contracts are not renewed. That's how it works at universities to this day. And, because I brought in this funding even before I became an assistant professor, it felt good. (*laughs*) During the seven years I worked at Johns Hopkins, I also received an RO1 grant award to conduct epidemiologic studies of migraine headache (with my collaborators, Drs. Walter Stewart and David Celentano). With Drs. George Comstock, Iris Obrams, and Moyses Szklo, I wrote a proposal to the National Institute of Heart, Lung, and Blood Diseases to establish one of the centers of the multi-center Atherosclerosis in Communities project. Time does not permit detailed discussions of these projects.

HWT: What brought you to NIH in 1987 when you joined the Biostatistics Branch in what was then the epidemiology and biostatics program of the DCEG, I'm sorry, the Division of Cancer Etiology at NCI? And also, could you describe a little bit about your initial goals and what you set out to accomplish and investigate?

ML: Well, there were three or four reasons why it was time for me to consider leaving Johns Hopkins. First, I lived in the Bethesda area. During good weather, there is a one-hour commute each way to drive to Baltimore. I married my current husband in 1980 and in 1983, I had my first child and in 1985, my second. I had these two young kids at home, and we had to have a live-in nanny because of my long commute and long hours at Johns Hopkins. I spent my whole salary paying for the live-in nanny. The problem with a career at a private university is that one is required to keep bringing in new funding.

The second goal is to teach lots of classes, because that pays for the student tuition. The third and lower-level goal is to conduct the funded research. Well, with two little kids, my workday would be filled with continuing to write new grant proposals, developing lectures for my classes, and mentoring masters' and doctoral level students. My research time would often take up the small remainder of daytime and many evenings. It was not a good quality of life. Also, I had difficulties relating to the department chair at Hopkins at the time. I felt that the department culture was very 'siloed'. Everybody worked in their own little area. If you were lucky, you'd have perhaps one or two collaborators. It fell far short of true teamwork. It wasn't working together with large groups of people and that just didn't suit my way of working. I worked wonderfully with Dr. Szklo and our small team of people in the field, and we had a clinical colleague who was very wonderful, Richard Humphrey, who worked with us, but it just didn't feel like the right fit for how I could do the best quality research.

A third reason, in addition to the craziness of the life and not being a good fit for me was my decision that if I was going to make the jump to another workplace, that I wanted to work for somebody that I respected enormously as a boss. I looked around at many possibilities and decided that I wanted to work for Joe Fraumeni at the National Cancer Institute. During the Reagan administration years when I was seeking a new job, funds for medical research were cut. Dr. Fraumeni didn't have positions to offer new hires. He was interested in hiring me, but I had to wait three years until something opened up. I held out because I did not wish to take on a new job with another leader. This was incredibly important to me.

Finally, a position opened. Among the reasons I was eager to work at NCI was because I had travelled to far off places in the world. As a young assistant professor at Hopkins at that time (early 1980s), it was difficult to expand one's research beyond a single city study to do nationwide or international studies. I wished to expand my work in hematological malignancies to nationwide or international levels. The job at NCI offered me these prospects. Although I wanted a job closer to home to spend more time with my children, instead the opportunity I was offered as part of the position at NCI was to work on a study of leukemia and hematologic malignancies in China. (*laughs*) I was not required to live in China, but it was important and necessary to travel frequently to launch and conduct the study. Since one of my goals was to work on more international studies, I decided not to 'turn a gift horse down.'

The NCI project was to study hematologic malignancies and other cancer risks in benzene exposed workers working in hundreds of factories in twelve cities in China. I worked on this project throughout the entirety of my career. The initial principal investigator on the project was Chief of the Biostatistics Branch, William Blot. We worked as a multidisciplinary team with experts in occupational health (Richard Hayes), industrial hygiene (Mustafa Dosemeci), statistics (Sholom Wacholder) and subsequently an expert in molecular epidemiology (Nathaniel Rothman). Bill Blot was a terrific scientist and a helpful mentor. He was not so much hands on, more hands off, but he was always there if you needed advice. And as Branch Chief, he led three terrific studies in China. The opportunity to work on a multi-disciplinary team with our collaborators in China (Songnian Yin and Guilan Li) was also amazing. These points illustrate why I eagerly jumped on the opportunity to work at the National Cancer Institute. My initial and long-standing research area, e.g., etiology of hematologic malignancies, benefited enormously from the opportunity to immediately work on international studies, working in conjunction with Drs. Fraumeni and Blot, and my terrific collaborators. There were pluses and minuses, but the former greatly outweighed the latter. (*laughs*)

HWT: So a couple of follow-up questions. Can you just take a moment to describe how you first contacted Dr. Fraumeni and what some of those conversations were like? Also, I'm wondering if you could describe the NIH when you first arrived? We can talk about how it evolved over time as well, if that comes up.

ML: When I was doing my general preventive medicine residency, it wasn't clear to me that staying at Hopkins was necessarily going to be my only option. So, I began to look around and visited a few places. I met Joe Fraumeni while I was in my general preventive medicine residency program. He led a very small group at the time. He asked me to join his group in 1978, but I had received the large grant, planned to start a fellowship in oncology at Johns Hopkins in 1979, and thought that if he's interested in me now, he'll probably be interested in me later. Thus, I had already met Joe. That's why I thought I would like to work for him. I respected him. I thought he had a good group. I thought he was doing great things. But the lure of being able to work on this big grant that I had brought in myself outweighed the potential opportunity of working with Joe Fraumeni at NCI and starting over again.

I periodically would keep up with Dr. Fraumeni. You never know where life is going to take you. I would observe what his group was doing, who were the people in his group, and consider how I might fit in with his group? I believed that I always had a potential downstream option to work in Joe's group but I didn't really get serious until I had two young kids. The commuting and everything else sort of got to be too much. So, was there another part of your question?

HWT: No worries at all. Just take a moment to describe the NIH when you first arrived.

ML: Joe Fraumeni's group was very small when I first met him. I don't know if he even had 20 people in his group. Now there are 300 people working in the division. It was a relatively small group in 1986, and they were physically located in downtown Bethesda in an office building a few blocks away from the NIH campus. When I became serious about applying for jobs, Joe's group was conducting many exciting U.S. studies, geographic investigations, and international studies. Early on, Joe and Bill Blot discovered the tremendous potential for conducting large epidemiologic studies in China. Plus, he worked in other international areas. There were also strong national studies carried out by Dr. Fraumeni's group, some of which focused on geographic maps. I don't know if anybody ever told you during these conversations about the maps that were drawn where there would be geographic areas of elevated cancer incidence and/or mortality in a given state or region that were then linked with specific industries, occupations, or environmental exposures where the high rates were seen.

For example, one of the discoveries early on was that there were high rates of lung cancer and mesothelioma around shipyards, which during the early years were characterized by a lot of asbestos exposure. That was how the link between asbestos exposure and lung cancer was further nailed down and understood.

After I joined NIH, I was in the downtown Bethesda office building for maybe a year and a half, then, we moved to an office building further out from the NIH Bethesda campus to Rockville, Maryland as Joe's group began to dramatically expand. We occupied a couple of floors in a building located on Executive Boulevard in Rockville.

HWT: Then between 1994 and '96, you served as acting head of the Analytical Studies Section. First, could you describe the Analytical Studies Section? Also what were your initial goals and how did they evolve? What did you set out to accomplish, and how did you go about it?

ML: Bill was a statistician, but he also had a strong feel for and expertise in doing epidemiologic field work. Bill had this small group of epidemiologists who worked in the Biostatistics Branch, but who were field work epidemiologists. This group needed to be pulled together administratively. So Bill asked me to organize the small group of epidemiologists and to hold regular meetings. The members of the group worked on different studies, but the idea was to learn from each other and seek opportunities to collaborate. That was the Analytical Studies Section, but it was a bit of a construct that didn't really make a lot of sense.

Then Bill left NCI with other people to form a private group of epidemiologists designated the International Epidemiology Institute. The statistician who was next in line to take over the Biostatistics Branch was Mitchell Gail. Mitch did not enjoy doing field work. While he initially tried to oversee the epidemiologists in the Biostatistics Branch, he finally went to Joe Fraumeni and said he didn't feel he was serving the epidemiology group well. At that point, Joe Fraumeni met with each of the epidemiologists in the Analytical Studies Section and asked us which other branches we wished to join. The analytic studies section was dissolved, and the members assigned to three other branches. .

Meanwhile, when one joins the National Cancer Institute, one does not always get to pick and choose which studies one works on. Sometimes one works on long-standing studies initiated by others, on studies launched by the Branch Chief or Division Director, or on studies requested by Congress. In 1989, two years after I joined NCI, a congressional committee strongly encouraged NCI to launch a study to evaluate public concern about a possible association of electric power line exposures with childhood leukemia. A small but growing scientific literature reported statistical associations which caused people to become very anxious. Meanwhile epidemiologists affiliated with the Children's Oncology Group (COG) received funding in 1989 to launch a nationwide, telephone questionnaire-based case-control investigation examining a broad range of postulated risk factors for childhood leukemia. These epidemiologists were not in a position to add an expensive in-person interview and power line exposure measurement study component on to their investigation through extramural grants.

Joe Fraumeni brought NCI intramural epidemiologists and the COG extramural epidemiologists together and suggested that our intramural group could provide the expertise to piggyback in-home interviews, in-home residential measurements of children's powerline exposures, and measurement of residentially proximate power lines onto the extramural study. I was asked to lead the intramural component based on my epidemiologic expertise in studies of leukemia. This project greatly appealed to me because I enjoyed working on multidisciplinary teams. We identified a physicist, William Kaune, who knew how to measure exposures from power lines to join our group of health physicists, statisticians, and methodology experts along with epidemiologists from the Children's Oncology Group. We designed a sub-study to study power lines in nine states of the COG nationwide study. Why nine states? We wanted to conduct the powerline component of the study in geographic areas where we could easily measure the exposures from the power lines and could achieve high participation rates. The latter was an important consideration because case-control studies can be limited by potential selection bias if participation is poor. If there is substantial selection bias, then one might not obtain accurate risk estimates. Thus, high levels of participation were critical.

Before we launched the nine-state study we did some pilot dosimetry studies for a year and a half with our physicist collaborator. We put devices on children; we put devices in their homes; and we put devices under their beds. From this pilot work, we developed an exposure assessment protocol for how to do these measurements. Then we piggy-backed our study in the nine states onto the nationwide COG study of childhood leukemia. Field work was completed in about four years.

The main publication from the study appeared in *The New England Journal of Medicine*. The study received extensive media attention and was also cited in a judicial ruling in Britain about a power company's legal liability for a plaintiff's allegation that a child's leukemia was due to power line exposure.

In 1993, a Congressional committee pressed Dr. Fraumeni to launch a study to investigate to potential role of cell phones in the etiology of brain tumors. I was one of the only people in the entire Epidemiology and Biostatistics Program who had ever done hospital-based studies. Because brain tumors in adults are often characterized by very short survival, one needs to interview patients with brain tumors as soon as possible after diagnosis, e.g., when they are still hospitalized. So again, Joe Fraumeni indicated, "You're the one. I need you to play a lead role on this study."

During 1994-95, I was still leading the field work for the study of powerlines and childhood leukemia and was in the initial stage of co-leading the field work for the investigation of cell phones and brain tumors. In 1994 Dr. Blot left NCI and Dr. Gail became the Branch Chief of the Biostatistics group. By 1996 Dr. Gail requested that the epidemiologists in the Branch be reassigned. When Dr. Fraumeni asked me what branch I wanted to join, he was surprised when I requested the radiation epidemiology branch (REB). He just stared at me, and I said, "Well, you know, Joe, even though power lines and cell phones produce nonionizing radiation exposures, these are types of radiation exposure. Thus, REB is where I belong." So that's how I ended up moving in 1996 to the Radiation Epidemiology Branch. By that time, the field work for the power lines study was finishing up and the cell phones study would be finished in a couple of years. I was beginning to think about what's next. I wished to do a study of ionizing radiation exposure, and the Radiation Epidemiology Branch would offer opportunities to carry out an ionizing radiation study.

HWT: You've anticipated a couple of my questions, including your move in 1996 to REB. So, let's dig deeper into your research. And again, you've anticipated a few things, but we're going to go there anyway and see where the conversation leads. This is a more general question. And I'm asking you to describe your process in designing and directing large and complex epidemiologic research projects in general. What is your process?

ML: Well, I'm a 'team sports' epidemiologist. There are people who love doing studies all by themselves, but if you want to do big epidemiological studies, you need input from a variety of different viewpoints by epidemiologists. You need strong methodologic help from statisticians. You need to do a high-quality exposure assessment, which means either a strong dosimetrist or strong occupational exposure assessment person.

My idea for the studies of power lines and of cell phones and cancer risks was to form a team involving two or three epidemiologists, one or two statisticians (one of whom was focused on methodology), exposure assessment experts, and then people with strength in field work. So that's my process, and I would say for all the major studies I've been involved in, they've all involved teams of people like this. I value listening to different viewpoints. My colleagues appreciate hearing my viewpoint. I'm a 'finisher.' I don't believe in dragging out studies. A short epidemiologic study is five to eight years. A medium length study is eight to 15 years. A long study is 15-plus years. But you need to publish your results as you go along. Otherwise, the population and the funders lose interest. To maximize publications, the idea is *sharing the wealth*. Everyone on the team should have opportunities to lead papers or be senior authors on papers; the statisticians and the exposure assessment experts should be co-authors on all papers from the study and have lead roles if they wish to do so. By having a team of five or six people, you can simultaneously write five or six papers at the same time. Everybody can be writing their paper in parallel. So that's another reason I'm a believer in teams. The collaborators appreciate the recognition and the fact that they're leading an important paper which is going to get published in a high impact journal thus providing them with recognition as well as experience in manuscript preparation and completion.

HWT: So, another sort of general question: You led groundbreaking etiologic studies of human hematopoietic and central nervous system neoplasms focusing on radiation, benzene, and other postulated risk factors. You mentioned China. But more generally speaking, where did you choose the place or places for your studies and why and who did you work with? Also, how did you design and run these studies and what did you find out? What made them groundbreaking?

ML: The Intramural Research Program (IRP), as I'm sure others have mentioned to you, should not be doing the kinds of studies that our colleagues at universities can do and get grants for. That's duplication. We should be doing the kinds of studies that would be very difficult for university epidemiologists to get funded from grants, e.g., long-term studies that span many years past the typical 5-year funding period. Also, in part, intramural studies should involve a unique aspect. It's not just this is the tenth study of breast cancer and hormones.

As to geographic location of the study, investigators go where the exposure is. Benzene exposure has been low in western countries for decades. But in China, a low-income country during 1949 (the founding of the Peoples Republic of China) through the mid-1980s (when NCI investigators began working with the Chinese), alternatives to benzene were not economically feasible. Benzene is very cheap. And it's involved in the chemical manufacture of many things: glues, paints, pesticides and is also the basis of many other chemicals. Because it's cheap, it's hard to substitute it for a more expensive but safer product. China was chosen for the study of benzene workers and cancer risks because of longstanding high levels of benzene exposure.

Another benefit for epidemiologic studies in China was their central system of keeping records. There are pluses and minuses to the centralized system in China, but from an epidemiologist's viewpoint, the centralized system and large population provides opportunities to conduct large-scale studies more easily with standardized methods for ascertaining exposure and identifying disease outcomes. Once the Chinese have decided they're going to do a certain kind of exposure measurement, they gather hundreds of measurement experts, develop and manufacture the measurement device(s), tool, and create protocols such that everybody does exposure assessment the same way. In addition, the Chinese retained almost all historical records of exposure measures. This was a unique opportunity where we had decades of measurements that were all done the same way. What the Chinese did not know how to do and what we contributed, was a strategy for linking the extensive exposure information with the cancer follow-up outcome information. We worked with our Chinese collaborators to jointly develop a methodology for exposure assessment using their historical measurements and work history data to create a state-of-the-art sophisticated methodology with a validation component.

What about the rationale for other studies? When a Congressional committee requested that NCI to do a study of power lines and childhood leukemia our Program sought to comply. The federal government funding of intramural medical research is appropriated and provided by Congress. It is not appropriate to respond, "I'm sorry, we're not interested," or, "We don't think that's an important problem." We find a way to do the requested research and try to include valuable and novel additions to the work. In contrast, the extramural investigators who are studying many risk factors for childhood leukemia found it difficult to obtain funding for adding measurements of powerline exposures onto their study. So that was how the collaborative group of intramural and extramural investigators could address notable public concerns within a joint study.

Data collection for the cellphone study was launched in 1994. I don't know if you remember Larry King, the talk show host. A woman came on his show who was a lawyer and alleged that her brain tumor was caused by her use of cellphones. This immediately got the attention of Congress. Now we tried to explain gently to the liaisons from the congressional committee that perhaps it was premature. Because the request came in the early 1990s, cellphone use was very low, and it was mostly among well-to-do young white men. The phones used analog technology phones at that time, not digital, so, we tried to indicate that we should wait a little bit to do this study, but waiting was not an option.

We designed a hospital-based case-control study with hospital controls from a wide range of diagnoses. Unfortunately, soon after we finished data collection, the use of analog phones phased out as the technology changed to digital phones. Also, cell phone users had expanded from well-off young white men to the more general population. The study was reassuring in that for the group of people we studied at the time, we found no association. The main paper, which was published in *New England Journal of Medicine,* received a lot of media notice. The changing technology, expanded population use, and higher levels of use (more frequent and longer calls and longer years of use) required further studies. We were able to use what we learned from the study to advise our international colleagues who were in the process of developing a protocol for a 14-country study led by investigators at the International Agency for Research on Cancer. We were able to provide advice about what worked and what didn't work. Thus, our study which was methodologically sound, but out of date, was helpful to others that followed. If you conduct a well-designed rigorously supervised field study, you learn from it, and how best to carry it out and learn what could be done next. But there were broader lessons about rapidly changing technology which is a little hard to get your handle on. Because people now recognize that virtually everyone internationally uses cell phones, it is almost impossible to identify a large unexposed group of people anywhere in the world. But behaviorally and technically, usage has changed. Young people begin to use cell phones very early and don't hold the phones next to their ears to speak on the phones. However, young people keep their phones near their bodies 24/7. And so, the issues have changed to concerns about whether the way young people are using cell phones might cause sleep problems, cognitive and/or behavioral problems. We know that ionizing radiation in younger people is associated with higher cancer risks than exposures of middle-aged and elderly adults, but this issue was not addressed in the initial studies that all focused on adults. One must keep these things in mind because epidemiologic studies are primarily retrospective in nature. The design is retrospective because cancer takes years to develop. So, you can't undertake a prospective study unless you have a huge amount of funding and willingness to wait years to decades to conduct extremely large prospective follow-up studies which would be required for studies of rare conditions such as brain tumors. This is the reason why we conduct case-control studies (that compare earlier exposure in newly diagnosed cases and appropriate controls) or retrospective cohort studies (that compare cancer occurrence in an exposed versus an unexposed population with exposure in the past and follow-up for cancer occurrence from initial exposure to the present time).

HWT: I just want to follow up as well with this idea of where to focus. And you mentioned before maps. So how did the development of maps at that time play into your work yourself?

ML: I was not so involved with the maps, but studies carried out under Dr. Fraumeni by several groups of investigators to further evaluate the map findings. There were retrospective follow-up studies of shipyard workers that began follow-up back to World War II and it was established subsequently that these workers did have increased rates of lung cancer. And it was shown to be due to asbestos. There was a big copper mine in a western state (Montana, I believe) with an increased cancer incidence in the population residing in proximity to the copper mine. In a follow-up study the exposure from the mine increased cancer risks thereby supporting the preliminary findings from the maps.

I was not so involved with the maps, but the maps do provide information; I would say nowadays a different kind of geographic information can be used to look at health disparities, availability of medical care, socioeconomic status, whether a state has accepted Medicaid and so on and so forth in relation to cancer incidence in the counties with these census-based characteristics. Mapping is still done but addressing somewhat different problems. There are states in the Southeast, where the obesity rates are higher than in other regions in the U.S. The southern states with high obesity also have high rates of obesity-related diseases (e.g., diabetes, cardiovascular disease, certain types of cancer). Thus, the idea of geographic maps, developed by Dr. Fraumeni and his group, continue to be valuable to this day as the exposures and outcomes studied have evolved over time.

HWT: So, getting back to childhood leukemia, I have a question for you. Two questions, really, based on a publication from 2012. So in 2012 you co-published "Acute Leukemia Incidence in Patient Survival among Children and Adults in the United States, 2001-2007." This was in the journal *Blood,* in which you assessed acute leukemia (AL) incident rates, IR ratios and relative survival in the U.S. in those years in one of the first population-based comprehensive assessments. You found that, quote, "The distinct AL incidence and survival patterns based on the World Health Organization classification support biologic diversity that should facilitate etiologic discovery prognostication and treatment advances, but that limitations of cancer registry data must be acknowledged." So, two questions. Can you elaborate on limitations of cancer registry data and explain the importance of the WHO's framework from 2001?

ML: In 2001, the World Health Organization really upended thinking about how leukemias and lymphomas should be classified. The proposed classification for hematopoietic neoplasms was a landmark and major development. Subsequently, there have been revisions to that, one in 2008 and one in 2016. Revisions and discussions are ongoing. The appreciation of what constitutes leukemia underwent a huge change. There were preliminary indications in the 1990s. International meetings of hematologists began initial steps to re-classify the formerly designated 'benign' myelodysplastic and myeloproliferative syndromes as malignant because a high proportion of these disorders transform into acute myeloid leukemia. Multiple subtypes of acute myeloid leukemia, myelodysplastic syndromes, and myeloproliferative disorders have also been recognized. The features of these hematopoietic neoplasms considered in the classification include clinical, genomic, molecular, treatment-related, and epidemiologic characteristics.

Population-based cancer registry personnel do not include pathologists or expert hematopathologists. The primary personnel are cancer registrars who train the people who collect these cases from in-patient and out-patient components of hospitals, pathology laboratories, radiology centers, and physicians' offices. These cases are identified from the entire spectrum of hospitals some of which are university-affiliated, and some are community hospitals. Because there is no centralized review of hematologic malignancies by expert hematopathologists, there is variation in the level of classification used by the hospitals and physicians' offices reporting the hematologic neoplasm cases. As you can imagine, the community hospitals take a little bit more time to recognize some of the subtleties of classification. Cancer registries use the new classifications, but they're brought on 'stepwise;' every case must be looked at in the context of where it came from. There isn't funding or time enough for a centralized expert hematology pathology panel to look at every case from every hospital in the country. That's why cancer registries, they're always going to be a little bit behind in using of the latest classification, but they get there eventually.

Our paper published in 2012 in *Blood* is part of an ongoing series of papers on descriptive characteristics of hematologic neoplasms using data from the population-based Surveillance Epidemiology and End Results (SEER) program cancer registries. Among our goals are to describe the characteristics of sub-types of acute myeloid or acute leukemia. What is the distribution by age at diagnosis, by sex, by race, by ethnicity, by registry? We try to evaluate some of these changes. There are changes over time as well in addition to these other characteristics. We examine whether these differences among the sub-types could be due to differences in causes. More recently, we have studied treatment-related myelodysplastic syndromes/acute myeloid leukemia risks after first primary solid tumors and hematologic malignancies.

Among the problems are that the leukemias (and other types of hematologic neoplasms) are rare. And to be able to say something with adequate statistical power is that you need big numbers, which means you need to collect lots of these cases over time, and they need to be carefully reviewed and categorized. We use the registries to try to give us some clues about hypotheses to test in our case-control and cohort studies. Then we try to design studies to help us pursue these clues and these leads. All these steps (e.g., conduct descriptive studies to generate clues and then test hypotheses in case-control and cohort studies) require years of effort.

HWT: I like that. I've not heard that before. (*laughs*) I wanted to ask you about your career in relation to influencing policy and public discourse and public safety issues and specifically with regard to cancer. You focused on leukemia and public health for a long time. As far back as 1981 you co-published in the *Journal of Public Health Policy* on benzene leukemia and the Supreme Court. So again, just taking a moment to talk about your career in relation to influencing policy and public discourse over public safety.

ML: I could spend three hours on responding to that question. Let me try to provide a short answer to it. Single or even multiple epidemiologic studies are never sufficient to make decisions about what is carcinogenic. These types of decisions are usually undertaken by an international body, the International Agency for Research on Cancer which assembles committees to produce monographs on various exposures and brings together experts from all over the world in epidemiology, animal laboratory science, molecular and genomic science, exposure assessment and other experts to review the body of evidence and decide on whether there is sufficient information to call something a carcinogen, a probable carcinogen, a possible carcinogen or not a carcinogen.

The National Cancer Institute investigators like me are invited to participate as experts in these committees, which are organized by the International Agency for Research on Cancer. Periodically, updates of reviews are also undertaken to re-review decisions which were made ten years ago, 20 years ago. This gets to your questions about where I see the future of cancer epidemiology. The question that more and more investigators and the general public are concerned about whether specific agents that are known carcinogens at high exposure levels are also carcinogens at low exposure levels. For example, high exposures to benzene and to ionizing radiation are clearly carcinogenic. But what about very low-level exposures? Are those carcinogenic? These questions are important because many more persons in the general population (and among workers) experience low exposure levels. To address these questions, epidemiologic studies require huge numbers to study low levels of exposure and estimate cancer risks. Because single studies are limited in size, we need multiple studies to join large consortia and collaborate by combining data. Even then, because of the rarity of many of these cancers that I and others study, one needs to statistically extrapolate downward from higher levels to lower levels. Frequently lacking are data points particularly at lower levels since even in large consortia we don't have thousands of people at exposure X, thousands of people at exposure Y, and thousands at exposure Z. You must extrapolate to calculate a dose-response curve. Animal and other laboratory studies can also be helpful in addressing such questions.

The work that I and others do contributes to this broader effort internationally to try to come to grips with is something dangerous, for example whether a postulated carcinogen causes cancer or perhaps cardiac disease. I think that the way the field has moved in the past 20 years, and I'm proud to say I've played a small role in this development, is the growing number of consortia. We need to all work together and share our data for the greater good.

In 2001, I joined with investigators from Europe, Australia, and other places to form a consortium of lymphoma case-control studies, the InterLymph Consortium. It's now 21 years since this consortium was started. This consortium, which includes an increasing number of studies that combine their data together, have been instrumental in providing the latest if the last word on investigations of exposure in relation to sub-types of lymphoma. Currently, worldwide there are consortia on lung cancer, breast cancer, gastric cancer, and many other chronic diseases such as diabetes and Alzheimer's disease. This is how we're going to make progress in the future, by openly sharing our data. We come together, different viewpoints, different minds, different ideas about how to look at the data, how to think about the data. I think this is where policy is going to be formed, namely from findings generated by these big consortia that share data on a broad scale internationally. What's been impressive and exciting is how the world has come together even countries where we don't get along politically, and that investigators understand the value of combining forces and that we're going to make some progress by joining forces together and sharing our observations and findings and thoughts and ideas.

HWT: Of course, that's been true with Covid.

ML: Absolutely. That's how, you know, these big studies have been critical. Studies from a single hospital are a starting point, but multi-center and multi-country studies are critical. For example, one needs to understand how Covid is affecting people in South Africa versus Australia versus the United Kingdom versus the United States. I mean there are similarities, and there are differences, but we need to agree to look at our data together and share it.

HWT: I have a couple more studies I wanted to ask you about, but I want to respect your time and not just take it for granted. I can stay—

ML: Yeah, I can stay.

HWT: Yes. You were principal investigator of a cohort study of cancer incidence and mortality among 146,000 U.S. radiologic technologists, which was unique in including 75 percent females in contrast with the mostly male cohorts of radiation workers. This was the only medical radiation worker cohort with several other unique characteristics, including a focus on individual cumulative occupational radiation doses, comprehensive work history, and a broad range of demographic lifestyle, medical, reproductive and other cancer risk factors. I understand the study found excesses of breast, non-melanoma skin, melanoma, and the combined category of acute lymphocytic acute myeloid and chronic myeloid leukemia among workers who were first employed and have worked five or more years before 1950. My questions are, where did you choose the place or places for the study and why, who did you work with, and again, how did you design and run these studies, and what were the implications of what you found out?

ML: When I joined the Radiation Epidemiology Branch, the U.S. Radiologic Technologists cohort study had been going on for about 12 years. It was started by a former branch chief of the Radiation Epidemiology Branch, John Boice. He had joined forces with Jack Mandel in Minneapolis, which is the 'home' of the American Registry of Radiologic Technologists (ARRT). The ARRT is a professional society of technologists who have annual meetings and receive certification through this organization. It's a professional clinical organization. ARRT was approached by Dr. Mandel, Chair of the Division of Environmental Science at the University of Minnesota, in the early 1980s. He asked the ARRT if they'd be willing to share their database with epidemiologists. He proposed that he and other epidemiologists would join forces with the ARRT and follow up this group of professionals to examine the association of occupational radiation exposure with cancer and other serious disease outcomes. ARRT was formed before 1926. The U.S. Radiologic Technologists (USRT) cohort included technologists who were first certified as radiologic technologists in 1926 and subsequently with the most recently certified no later than 1980; after 1980 the cohort was 'closed' to newly certified members. Members eligible for inclusion in the cohort had to have been certified for 2 or more years. The plan was to follow up this group of technologists retrospectively (e.g., 1926-1982) and prospectively (1983 onward) to evaluate their cancer and other serious disease outcomes. Dr. Mandel approached Dr. Boice, Chief of the Radiation Epidemiology group at NCI to provide collaboration and funding for a long-term follow-up. The NCI component was led by Dr. Boice until 1996 when he left NCI to join the private group of epidemiologists at the International Epidemiology Institute. After Dr. Boice left NCI, the only person working on the study was a staff scientist, Ms. Michele Doody.

Dr. Boice's replacement as branch chief, Elaine Ron, asked if I would take over the NCI leadership of the study. The USRT cohort study was still in the beginning phases. Although the study had been underway for 12 years and two rounds of survey questionnaires had been sent out, the study was still at a relatively early stage and had produced only about five publications, none of which included comprehensive individual worker cumulative occupational radiation dose estimates. I saw this as a great opportunity to study low dose ionizing radiation exposure in a group of medical workers who wished to be studied. These workers were medically literate so if they reported that they had a condition or a disease or a cancer, it's more likely that they would report more accurately than persons in the general public.

Soon after I took over, I formed a team of collaborators. This is my modus operandi. I encouraged two or three epidemiologists to work on this study along with a dosimetry expert (health physicist) and statistician. Among the early goals was to complete the second survey with a high level of participation, to develop estimates of individual worker annual and cumulative occupational radiation exposure, and to begin to consider biomarker studies of radiation exposure and genomic studies related to radiation-related cancer outcomes. Medical radiation workers are required by states to wear badges to measure their occupational radiation exposures for protection purposes. Collection of the badge doses had been launched by Dr. Boice, but an algorithm to use the badge doses along with work history to estimate annual and cumulative badge and organ doses had not yet been developed.

We embarked on what turned out to be a ten-year effort by epidemiologists working together with health physicists, dosimetrists, and statisticians, to estimate individual worker annual and cumulative radiation badge doses and to develop a strategy that would take into account missing doses. Missing doses resulted from when the technologists didn't wear badges, didn't turn them in, or badges were sent to a different organization for 'reading' that did not provide historical badge dose readings to NCI. . We had questionnaire data on all the procedures the technologists had carried out and their behavior as they carried out these procedures. Did they typically hold patients during x-ray examinations? Some patients needed to be held in the days where some of the early exams were being done. How did they do these procedures?

It was a ten-year process of working together assembling all this data to develop a sophisticated dosimetry resulting in an assigned estimated dose for each technologist for each year they worked as a technologist and a cumulative dose. In addition to these individual badge doses, we used the badge dose data to estimate doses to 12 organs. We were able to develop dose estimates for about 110,000 of the technologists. We described the dosimetry algorithm along with a summary of badge and organ doses in a paper published in 2014. Early on, after I had taken over leadership of the USRT study, we published several papers describing work history characteristic and risk of specific cancers and other serious diseases. In the last five years we have published multiple papers describing the dose-response relationships using cumulative estimated doses in relation to several cancer outcomes (breast, thyroid, brain, lung, leukemia and other hematologic neoplasms), and cataracts. We are also working on a paper describing dose-response for cardiovascular diseases. In addition, we published papers describing cancer and other serious disease risks among two subgroups of the USRT with higher estimated doses, namely those technologists performing nuclear medicine procedures, and those assisting with fluoroscopically guided procedures.

We also realized that this is one of very few U.S. nationwide cohorts that could be employed to study cancer risks associated with ultraviolet radiation at a broad range of latitudes. One satellite that NASA sends around the Earth measures ultraviolet radiation. And so, Elizabeth Cahoon, a member of our group, has linked the big database of NASA satellite information on ultraviolet exposure with each residence of each worker in our study. This has enabled us to carry out studies of ultraviolet radiation exposure and cancer and other serious disease risk such as circulatory diseases and cataracts.

Thus, we have turned this study into the only large, lifetime study of occupational radiation doses in relation to cancer and other serious disease outcomes worldwide. The study has provided important knowledge about low-to-moderate dose radiation and cancer risks and much more. The study offered great opportunities but was in early stages when I took it over in 1998. I worked with Dr. Mandel's replacement, Dr. Bruce Alexander, at the University of Minnesota for 20 years on this study and the American Registry of Radiologic Technologists to continue to follow up this group of workers. We have completed 4 cohort-wide surveys (1983-89, 1994-98, 2003-05, and 2012-14), validated the 2014 dosimetry algorithm, assessed work history and cancer and other serious disease risks in the overall cohort and more highly exposed subgroups (see above), examined estimated residential ultraviolet radiation exposure and cancer risks, carried out USRT-based genomic studies of breast and of thyroid cancer, and pooled the data from the study with data from other cohorts in the NCI cohort consortium to assess a range of lifestyle factors with risk of several types of cancer and in-depth genomic studies of breast cancer. More recently, we have linked the cohort data with 43 U.S. population-based registries and estimated the level of ascertainment from the self-administered 4 questionnaires and death certificates versus the level of ascertainment from cancer registries. To date more than 180 publications have appeared from the cohort data alone or in pooled analyses along with use of the cohort data in 8 doctoral theses.

We're in the process of doing another linkage with the National Death Index, e.g. national mortality data, for follow up of mortality. These workers are now average age of mid-seventies. So, our investigation will continue as a lifetime study of workers who generally launched their careers as radiologic technologists in their late teens through early 20s. Thus, we're able to study these workers from age 20 to the end of their lifetimes to assess their cancer and other serious disease incidence and mortality risks. In general, one may not be able to work if one has serious chronic disease. This is what we call the healthy worker effect. If you're able to keep working, that's generally because you're healthy. Investigators need to take this into account when conducting occupational epidemiologic studies. Our two-fold goal has been to estimate risk of the occupational cohort compared to the general population baseline due to occupational ionizing radiation exposure in relation to the cohort's work history and habits and to assess dose-response using internal cohort comparisons.

We also evaluated their ultraviolet exposure based on the geographic location of their residences at different points in their lifetime. The USRT members, while residing at a wide range of latitudes is not the optimal population for studying ultraviolet exposure and cancer risks because the technologists are indoor workers. , you know. Ideally, one would prefer a mix of indoor and outdoor workers to study the full range of ultraviolet radiation exposures. Nevertheless, there are few U.S. nationwide worker populations who have completed questionnaire assessment of lifetime residential history as well as host (hair, eye, and skin color) and behavioral factors (early life sunburns) that enable assessment of ultraviolet exposure levels and cancer and other serious disease risks. One also needs to state the limitation that our investigation of ultraviolet radiation and cancer and other disease risks are derived from a study of ultraviolet radiation of indoor workers.

Many of the worker studies that were done early in the 20th and even the 21st centuries in the U.S. and elsewhere were studies of men. The USRT is comprised of 75% women. The most famous study of women workers is the Harvard nurses' cohorts. Although radiologic technologists don't have the education of nurses, almost all are occupationally exposed to ionizing radiation (unlike nurses). The technologists generally are not required to complete the length of medical education of nurses, but they have specialty training and education.

My collaborators and I also saw the potential of being able to do biomarker and genomic studies in the USRT. Our work was focused on chromosomal translocations typical of radiation exposure to validate our dose estimates. In addition we have studied genomic pathways and thyroid cancer and participated in genome wide association studies in relation to breast cancer. The breast cancer cases and controls included in our genomic studies have been combined with cases and controls from a large number of other populations in pooled analyses that have yielded valuable information about several aspects of the genomics of breast cancer. We have also carried out detailed studies of lifestyle factors in our population alone and combined with large populations in the NCI Cohort Consortium.

Among the more recent efforts is an emphasis on the newer technologies that are used in ionizing radiation namely nuclear medicine technologies and fluoroscopically guided interventional procedures. As nuclear medicine procedures began to be used increasingly in the 1990s for PET scans and PET CT (to evaluate patients for metastatic disease and for using radionuclides to treat cancer), we recognized that these are very high-exposure exams. Technologists may also receive higher exposure from assisting with fluoroscopically guided procedures. Even though the radiation exposures from general imaging procedures to radiation technologists are extremely low, exposures from some of the nuclear medicine procedures and fluoroscopically guided procedures can be substantially higher. We are focusing future efforts to develop more accurate exposure estimates for workers who conduct these occupational procedures to eventually conduct dose-response analyses of cancer and other disease risks among the technologists with these exposures.

Studies of the USRT also have value for the general population. Most of us undergo one or more x-ray procedures in our lifetime. Patients undergoing x-rays or other imaging procedures involving ionizing radiation are not badged. But we can extrapolate from studies of workers to the general population because we have estimated quantitative doses from workers. That is another reason why worker studies are an important source of information.

We've learned a lot about the USRT cohort population, and we share the information we have learned with the population through newsletters and an ongoing website. The website is accessible by the general public and the workers in the study.

HWT: I have here a short list of studies that I thought we could discuss. However, we have mostly discussed them. For example, the study on the large cohort of Chinese workers who were exposed to benzene. The study that you initiated to address public and congressional concerns over cellular telephones. You just mentioned the expanded use of fluoroscopically guided diagnostic in interventional procedures. So, is there anything that you would like to discuss about these studies or frankly any others that you feel is important to talk about right now?

ML: Just a couple of points. The study of cellphones that we did was very early on and involved low cumulative exposures from analog cell phones because of relatively limited numbers of phone calls and relatively short duration of calls as well as a short number of years of use. Subsequent investigations studied people who used digital technology cellphones for much longer, and with much heavier usage. Our study was null; we found no association. That was reassuring at the time. But it said nothing about future use and longer-term duration of use. One of the things that we have agreed to do and that we have accomplished is to continue to monitor trends in brain tumors in the general population because there may be a very long latency between when a person is first exposed and when that person might develop a brain tumor.

We've carried out three descriptive epidemiologic studies examining time trends to see is there any evidence of increased U.S. incidence of brain tumors over time that might be due to cellphone use. Two of our studies have evaluated trends in malignant brain tumors and our most recent investigation published two years ago focused on non-malignant brain tumors. These studies have all shown no evidence of increased incidence of brain tumors over time. Now this isn't proof but if cellphones, which are so widely used, were a cause of brain tumors, one would expect to see some signal of increasing incidence risks. So, I think that's an important point to make.

As always with epidemiological studies of radiation exposures and cancer risks, the Japanese atomic bomb survivors are the 'gold standard.' One needs to do lifetime studies. The greatest incidence of cancer from the atomic bombings in 1945 is now occurring and will continue to occur for the next few years. Latency of cancer is something to always keep in mind. We will probably do another study of time trends in malignant and non-malignant brain tumors in five to ten years.

Another point I want to make is about the study of childhood leukemia and powerlines. Overall, the body of evidence does not reveal an association except at the very highest exposures involving a small increase in risk among a very small fraction of children. We've never been able to explain it. We've pooled our data with seven European studies and a Canadian study. We see a 2-fold excess among children with high exposure levels. There is no biologically plausible explanation for this increased risk which may, in part, be due to selection bias. It is unclear that epidemiology is going to be able to provide an answer. Epidemiology is a valuable and important science, but it will not answer every question of every exposure and disease risk. I think that that's something important to pay attention to.

The last point I wanted to make is over time we've developed increasingly sophisticated strategies to improve our exposure assessment. We've learned how to estimate exposure at low levels and how to statistically evaluate dose-response. So, in our first follow-up studies of the benzene workers, we used what was at the time a state-of-the-art methodology for exposure assessment for benzene. And we found quite a few statistical associations revealing increased cancer risks. As we've improved our exposure assessment over time, some of those findings have not held up. The question is, is it because of improved exposure assessment? That's one possibility. Another is, that we are evaluated rare cancer outcomes, while exposure levels have declined. We don't have the statistical power to be able to see very small increases in risk. A third potential explanation is the early retirement age of men and women in China. We have not been able to follow up the Chinese workers at older ages. We had to stop soon after they left the workforce. So, are we not seeing increases in some of these hematologic malignancies because we could not follow up the workers to old enough ages? That's always a possibility. Even with the highest quality of epidemiological studies, like every other type of science, epidemiology has methodologic limitations.

HWT: That's very interesting. I'm excited to ask you this question because your focus has been, since you were a student, very international. And so, you talked about the future and consortia of the future. More specifically regarding NCI, why are international efforts towards scientific understanding important to NCI and to scientific discovery more generally?

ML: One of the points I mentioned earlier can be thought of as analogous to the situation for a successful bank robber who should target where the money is. If you want to study radiation disasters, you must study Chernobyl. The Three-Mile Island accident in the U.S. resulted in very low radiation exposure levels. Chernobyl has been instrumental to our understanding of what happens in a radiation accident disaster, and the epidemiologic studies conducted post-Chernobyl are the best quality of epidemiology that's been done to date on radiation disasters.

One of the things I'm proud of as a branch chief was an exercise I conducted with the dosimetrists and epidemiologists and others who worked on Chernobyl in which we posed the question to ourselves: suppose instead of happening in 1986, Chernobyl happened 'today'? What are the lessons we have learned? We published a paper on this to address such questions as how would we improve the exposure assessment? What would we do differently in terms of recommendations for reducing the occurrence of cancer and other diseases in the population residing in proximity to Chernobyl or among the Chernobyl cleanup workers? The occurrence of the Chernobyl accident and the collaborative work others in REB have carried out to understand cancer risks (and to prevent future risks) associated with the accident is one example of why NCI should carry out international studies.

Another reason for engagement in international studies is because of unique resources and databases not available in the U.S. In the United States we do not have the ability to link our personal identification numbers with a wide array of databases as they're able to do in Nordic countries. In Sweden, Denmark, Norway, Finland, and the Netherlands, linkage of the personal identification numbers with the national cancer and population registries, exposure databases (such as features and characteristics of powerlines), patients registries (such as those of patients with congenital disorders), pharmacy databases and many others allows for the potential of amazing epidemiology that would not be possible in this country because we cannot link our personal identity numbers with other databases. That's another reason for international studies, namely to provide opportunities for epidemiological studies through database linkages not available in the U.S.

On the other hand, many of the studies of ultraviolet sun exposure have been carried out in Denmark, Sweden, Norway and northern Europe. As you can imagine, these countries are not optimal due to lack of a broad range of latitudes. In the United States with the NASA satellite ultraviolet measurements and the tremendous range of latitudes, we have been able to study ultraviolet exposures in relation to skin cancer and other medical conditions. Thus, there are research opportunities for studies in the United States that can't be done so easily in some countries.

We didn't talk about two other topics that I will just briefly go over. So, you asked me about my role as Chief of the Radiation Epidemiology Branch. I joined the branch in 1996, and I was appointed as acting chief in 2002. It was a very small branch, and we did not have a group of dosimetry experts. We had one statistician. Our research portfolio included mostly environmental and occupational studies. My most important accomplishments for the Branch, in my opinion, were to bring on epidemiologists to expand our work in medical sources of radiation, and to expand exposure assessment efforts for our radiation studies by establishing a new dosimetry unit. That unit had health physicists and other measurement experts who added expertise in developing sophisticated methods and algorithms for dose assessment of all types of radiation exposures. I also conducted international searches to hire tenure-track investigators (epidemiologists, health physicists, and a statistician) who successfully achieved tenure and currently form the core group of tenured investigators in the branch. This was an important contribution to the branch. At one point, we had people from 13 different countries in the branch. We've always had a large international representation of investigators at all levels in in the branch with people from most continents in the world who contribute their viewpoints to our research. While most of the post-doctoral fellows over the past 25 years have gone back to their home countries, we continue to collaborate with many of these investigators who are past members of the branch.

Another signature branch contribution was initiation of the radiation epidemiology and dosimetry course, which was first held in 2002 and has been held about every four years. Each time we held the course, we thought that attendance would level off or decline, but instead the attendance has continued to increase by 50 percent over the previous four years. The reason is because people hear about the course throughout the world, registration is free, and the speakers are internationally recognized experts in the branch and elsewhere. Some attendees have come each time we hold the course to get a refresher to learn about epidemiologic and dosimetric aspects of sources of radiation that are outside their areas of expertise. For example, radiation scientists who mostly work with environmental sources seek to be updated on the latest developments in medical sources of radiation epidemiology, and dosimetry. The last time the course was offered was in 2019. We videotape the course and post the lectures online so anyone can see the latest version of the course. When we give the next version of the course, we replace the previous version online, so that it's available throughout the world.

As an emeritus, I note that the radiation epidemiology and dosimetry course, which is aimed at post-doctoral scientists and for continuing education of those who have been working in the field of radiation science. What about medical students? So, my first post-retirement project, which was in part organized by the National Academy of Sciences, involved a meeting of 24 multi-disciplinary experts (in radiology, radiation oncology, health and medical physics, medical education and communications) met virtually for a 2-day period at the end of 2020 to discuss a proposal for a broad-based introduction to radiation science for medical students. Subsequent to the 2020 virtual meeting, the expert group worked over the course of a year and a half to develop a publication describing and expanding on our proposal. The resulting paper was recently published in the *Journal of the American College of Radiology.* My goal is to follow up with some professional organizations (such as the American University Radiologists, the American College of Radiology and others) to introduce the course beyond the single medical school in which parts of the course have been implemented by one member of the expert committee. This project was a natural extension of the radiation epidemiology and dosimetry course, which I initiated in REB. I think that both initiatives represent important contributions to the broader group of radiation scientists and the next generation of clinicians who should be more literate in radiation science.

HWT: I really appreciate that overview. I don't know if there's anything you want to add in terms of your role as branch chief. And again, I'm aware of the time. I did wonder, you know, I mention in the questions that you had done extensive outreach, which you were talking about here. But also, you know, in terms of your shaping the occupational, environmental research work of the branch, if there's anything you wanted to add from that point of view. And specifically in your role as branch chief, you also served as co-principal investigator of REB's projects on medical countermeasures against the adverse health consequences radiological and nuclear threats, a national program established by DHHS office, on public health emergency preparedness in the National Institutes of Health, and that is coordinated and administered by NIAID NCI. So, I'm wondering about that role, its importance and its evolution as well.

ML: Early on as branch chief, in the Bush administration there was a lot of concern about radiological terrorism and bioterrorism. The National Institute of Allergic and Infectious Diseases was given substantial funding by the federal government to develop strategies for dealing with bioterrorism and radiation disaster issues. REB investigators were invited to some of the initial meetings, and we indicated that if a dirty bomb involving radiation exposure was dropped in Washington or other U.S. city today, we could use some of what we've learned in epidemiology to be helpful in triage. As epidemiologists our exposure assessment often estimates radiation levels for thousands of study subjects. It is not feasible to conduct expensive chromosome translocation tests (costing over $3,000 for each individual) to assess an individual's radiation exposure. Instead, we've learned through our exposure assessment strategies to group people into high exposure, medium exposure, low exposure, or no exposure. This was part of the contribution that dosimetrists working with NIAID have used: the knowledge gained from how we categorized members of the radiation-exposed populations and in our exposure assessment strategies, for example following Chernobyl or other disasters nationally and internationally that we have studied. We have provided reports and publications on how this strategy could be used to separate out the people who really need to be seen immediately for medical care versus the ones that could carry out mitigation on their own through sheltering, showers (getting rid of the clothes and/or putting the clothes in bags) and other efforts. At the request of American Academy of Pediatrics (AAP), I updated a paper from 2003 on radiation disasters and children. Just to give you a sense of this effort, I wrote and revised the draft paper multiple times with colleagues from the CDC and from the Committee on Environmental Health of the AAP. Several AAP committees reviewed and commented on the paper. Towards the end one of the last reviewing committees said, "Well, what should the average pediatrician do?" And I said, "The average pediatrician should tell parents and children, "Don't come to my office. I am in no position to take care of a child who's covered in radioactive debris. I can discuss the CDC and other recommendations with you by phone if possible and provide advice as to whether your child requires evaluation and treatment at one of the selected emergency rooms that knows how to deal with this." REB's contribution is to what should be done in the general population and what should be communicated with persons who request expensive testing to make sure that radiation-related chromosome translocations are not observed. Epidemiology can contribute by addressing general public health measures. We have worked with other organizations such as the National Academy of Sciences, Engineering, and Medicine, the National Commission on Radiation Protection and Measurements, and with agencies in the federal government, such as the Centers for Disease Control, to address the issues related to exposure assessment, long-term follow-up of cancer and other risks in populations, and communications to the public following a radiation disaster.

We've continued to also use funding from NIAID to address population concerns about thyroid cancer risks associated with fallout from above ground testing in the U.S. REB has developed a risk calculator for fallout-related thyroid cancer. You can go online, put in some keywords, answer questions such as 'What year were you born in? How old are you? How much milk did you drink in the 1940s and 1950s [when above ground testing was conducted]? What states did you live in during these decades? These calculators help people figure out their own level of risk from both U.S. and global fallout. .

I think that NIAID has been appreciative of our expertise in radiation. NIAID has expertise in bioterrorism as it relates to bacteria, viruses, and chemicals. By adding our expertise, we have assisted NIAID with development of a coherent, uniform policy that is now shared. It's on various websites. It's in professional organization reviews that are helpful to the general public. Although we do not have expertise in development of drugs to mitigate these radiation disasters, REB has provided important information to the general public about how persons can reduce their risk from radiation exposures associated with radiation disasters.

HWT: Okay. I know we're reaching the end. Just a couple more questions for you. Can you describe your leadership style? You mentioned you're a team player. Is there anything that you want to add?

ML: In terms of the leadership style, I valued receiving input from many people. In the end, a leader needs to make final decisions and to ensure that the work gets done. There are 'soft' parts and the hard parts to running an organization as a leader. I believe that in the federal government progress is achieved if we work together and consider many different viewpoints. In the end, a leader must require that staff adhere to deadlines by setting reasonable, albeit hard limits. My philosophy was that no additional funding would be provided until staff finish their previous projects, for example. Removing unproductive staff is not easy, but there are ways of doing it. My goal was to shape the branch by bringing in 'top stars' who are now tenured investigators in the branch, and gently encouraging several unproductive staff to retire or move on. I also sought to eliminate non-productive projects that were left over from the days that I inherited the branch and to reduce costs of projects that could be 'slimmed down.' My goal was to leave the Branch in a stronger position when I stepped down than when I took over. My efforts were externally validated by the site visit reviewer assessments of 'outstanding' for my roles as Branch Chief and Principal Investigator.

One of the reasons I decided to step down as Branch Chief at the end of 2014 was my evaluation that the branch needed new leadership to expand dramatically the studies of medical sources of radiation exposure and cancer risks. Dr. Amy Berrington de Gonzalez succeeded me as Branch Chief. As an expert in medical sources of radiation and cancer risk, she greatly expanded the Branch's work in that area and began to expand the Branch's cancer survivorship studies. Dr. Berrington de Gonzalez, who I recruited to the branch in 2008, left NCI in 2022 and has been succeeded by Lindsay Morton who I also recruited in 2008. Dr. Morton's long-standing research on cancer survivorship and genomics will expand the branch research in these areas, while maintaining strong research projects on medical, environmental, and occupational sources of radiation exposure.

My philosophy has always been to move on when you are still at your peak to enable the study or the organization that you lead to continue to evolve in exciting new directions. I wanted to end my time my time as Branch Chief in order to turn my focus from hiring investigators, developing research budgets, and dealing with scientific administration back to research full-time research and to finish my own studies. I'm a finisher. So, I'm proud to say that I almost got it right but needed to work nine more months after retiring to finish up the last of the papers I had hoped to finish before I left. I recognized that it is difficult to precisely estimate when the key papers on my 30-year taxpayer-funded studies would be completed. I was satisfied that the taxpayers have received major benefit from the knowledge gained as described in the publications and reports from the cohort studies that I undertook in the 1980s, the 1990s. We've learned much valuable information from these studies. As an NIH Scientist Emerita I plan to keep going with following up some of these populations and identifying new ideas to explore in the existing data.

You asked about scientific and public service. In DCEG, NCI we don't have to compete continuously to bring in new funding. There's one pot of money which is given to our Division Director. He doles it out, not to people, but to studies. But because we have some free time, since we don't have to bring in new funding or to teach, we have leeway to serve on committees and give back, provide advice, and consult with other organizations. I've spent a lot of time in my career serving on committees, giving back. Because I don't believe in lifetime committee service, I have generally served five to seven years on each committee and then rotate off, Serving as a reviewer of hundreds of manuscripts and serving on editorial boards is another way I have given back to the scientific community.

One of my most favorite scientific activities is one-on-one mentoring. I've mentored more than 40 pre-, post-doctoral, and tenure-track investigators in my career. Many have gone on to have tremendous careers not just in radiation but in other areas of research. I am proud of my efforts to assist my mentees in developing fruitful research areas, learning new efficient field work strategies, and aiming their efforts on seeking out projects for which they feel passion. I have also greatly enjoyed serving as a career mentor to many and helping them attain jobs that are an excellent 'fit.'.

HWT: Mentoring seems to be incredibly important in the scientific community in general and of course at the NIH. Let me ask you one more question about that and then we'll back up. What advice would you give to encourage young scientists, particularly young women, to continue pursuing their goals and to seek out necessary resources despite setbacks or barriers that they might face?

ML: Oh, you've hit on a sensitive nerve here. I'm afraid I'm going to have to move into a slight complaining mode. I would have thought at this point in my life that women scientists and female academic clinicians would be much better off than they are. We finally have the first woman as head of the National Cancer Institute. She's a surgeon. Nothing wrong with surgeons but primarily male search committee members and NIH leaders picked her in part, I'm sure, because she's a surgeon. I have a daughter who's 39 years old. I thought that by the time my daughter had reached her current age that the barriers and the lack of appreciation and recognition of women professionals would have disappeared. I don't think we're going to have a woman president of the United States for a long time to come. Although there's a lot of talk about diversity, inclusivity, equity, and these issues are critically important, women professionals are still second-class citizens. This is not where we should be at this point in time. So, does that mean we should throw our hands up and say it's all over? No. Women are half of medical school student classes, but they are not half of the senior faculty of medical schools.

I think that women scientists and academic clinicians have difficulty in working out work-life balance. I always ask my children; did we take enough vacations? My children assure me that we had sufficient numbers of vacations, and I have photographic proof. In retrospect, I realized that we did take more vacations than I thought but continue to wonder whether I did enough on all fronts.

We live in an imperfect world. One of the reasons I wanted to join Dr. Fraumeni's group after leaving Johns Hopkins was because he was the 'poster boy' for women scientists at NIH. He had more women scientists as branch chiefs than any other division director. He was years ahead of the other division directors in this respect. But is that enough? I'm not sure it is. And I think that women must get in there, do their best work, fight for recognition and leadership, but anticipate that appropriate recognition and leadership roles may still not come in their lifetime. Years ago, I thought that opportunities for women scientists would be very different than the current situation. I am sad that there's been inadequate recognition of women scientists. On the other hand, we are making some, albeit slow, progress. I'm happy to hear men who say, I won't serve on panels unless there are women. Well, good for you, but why didn't you say that 30 years ago? When I think of some of the injustices that happened to me, I feel unhappy. Fortunately I have a thick skin and have mostly ignored injustices and just kept going.

I think that for women scientists of color, the situation is even worse. Efforts are being made, and I love that our current division director is really bending over backwards to try to change the culture, but it is rolling a huge rock up a very steep hill.

HWT: So, before we sign off, I just want to ask you why did you decide to spend your career at the NIH?

ML: The Division of Cancer Epidemiology and Genetics is the largest group of epidemiologists anywhere in the world. And because we don't have to compete for grants—I'm not saying that one big pot of money is necessarily the best way to go—we collaborate more fully than investigators at many siloed universities. The culture suited me. I loved working with my NCI colleagues. I not only respected Drs. Fraumeni, Blot, Ron and others who were my bosses, but I respected my collaborators, enjoyed the collegiality of working with people at NIH., and being able to do research 80 percent of the time. I enjoyed not having to teach classes. I appreciated not having to continuously bring in funding to support my salary. So, I stayed there. The studies were fantastic and unique. While working as a research scientist in the U.S. government is s not for everybody, it was wonderful for me.

HWT: Okay. We'd already talked about the future of research, especially of course at the DCEG, but in general in cancer epidemiology. Is there anything you want to add about that? Or about anything else that we've talked about that you feel is important?

ML: It is difficult to imagine what will happen in the near future. The consortia and large-scale collaborations are great as is the availability of incredible databases. As a former clinician I am concerned about being able to link specific information about medical conditions, medications, behavior and exposures in a very deep way because of the lack of ability to link personal identity numbers with U.S. national databases. Even in the Nordic countries where they have pharmacy data and hospitalization and outpatient data, it is not feasible to do the in-depth epidemiologic studies of behavior, lifestyle factors, exogenous exposures (environmental and occupational), medical conditions, medications, and cancer and other serious disease risks. Combining these linkages with interview studies to obtain the level of detail sought will likely be unaffordable. My concern is that these types of in-depth studies to assess cancer incidence, survival, mortality, and other types of outcomes in patients undergoing cancer treatments are going to have to revert to smaller studies. This harks back to the old style of research in which one conducts one small study, and later another small study is undertaken to see if the results can be replicated and so on. This will not be an efficient way to proceed. Consortia are efficient, but the problem with consortia is that the data that are often collected in each study are superficial in terms of what we ideally need to be studying. I have some worries about the future, about how one is going to be able to proceed and whether we're going to be able to understand the complex interplay of factors causal for disease. We like the idea of precision medicine, e.g. tailoring a drug to an individual's specific genetic, racial, age, other characteristics. It is good in principle, but how will this be done? How are we going to afford it? I'm a little bit concerned about how one might proceed in the future to conduct the kinds of epidemiologic studies we need to conduct to understand drug benefits, drug toxicity, holistic treatment, how much exercise an ill person needs to undertake to remain healthy, what dietary components will promote health and other factors. Measuring so many things like diet is extraordinarily complicated. Diet is a glamour field in epidemiology, but one of the reasons those of us who are very quantitative have shied away from studying diet is the difficulty in understanding what one is really measuring. What's in the diet? Which nutrients are being consumed? How has an individual's diet changed over time? How do you line these up with a given person's metabolome, genome, epigenome and other characteristics. Not so easy. While I think that all of these complicated issues should be theoretically surmountable, we're going to have to be very clever about how we do this.

HWT: Do you feel that we've covered all of the topics that you would like to today in terms of your own career, NIH in general, anything at all?

ML: I think pretty much. I think this has been good. One last observation is the growing number of NIH emeritus scientists. I believe that I and other NIH emeritus scientists have things to contribute. One example is my proposal for integrating broad-based radiation science in medical school graduation. I think more advantage could be taken of emeritus scientists and older scientists. We're not going to be able to contribute the intellectual firepower of our youth, but our contributions could still be useful. Many retired persons contribute to the greater good.

HWT: Interesting. I thank you for your time and your extra time. It's been a pleasure to speak to you. Have a good rest of your afternoon and evening.

[End Interview.]