RADM Richard G. Wyatt, M.D.
Oral History
November 1, 6, and 8, 2023

RADM Richard G. Wyatt, M.D., December 2006

This is an oral history with Dr. Richard Gregory Wyatt, conducted on November 1, 6, and 8, 2023, about his career at the National Institutes of Health. The interview is being done over Zoom and the interviewer is Dr. Victoria A. Harden, Founding Director Emerita, of the Office of NIH History and Stetten Museum.

Harden: Dr. Wyatt, would you please state your full name, that you know that this interview is being recorded and that you give permission for the recording?

Wyatt: Yes. Richard Gregory Wyatt. I understand the session is being recorded and I give my permission to do that.

Harden:Thank you. So let's begin at the beginning. You were born June 6th, 1942, in Lebanon, Missouri, as the only child of Russel Glennon Wyatt, a banker, and Pearl Alene Jones Wyatt, a teacher. Your uncle, Dr. Curtis H. Epps was a physician who had graduated from Washington University Medical School. Would you tell me about your early life, through high school, about what goals your parents may have had for you, other family members may have had for you, how teachers and others may have nudged you towards a career in medicine?

Wyatt: I will be happy to do that. I have been considering the people who have influenced me, and I would like to make sure I give them proper credit. But starting with my parents, they were very—what would I say?—hands off with regard to my career direction. They did not push me in any directions, they let me make my own decisions about what I wanted to do. And apparently what I was doing was satisfactory, because I do not recall that they were, in any sense, what we call today “tiger parents.” I did my homework myself, and I was determined to make good grades and do the kinds of things that were satisfying and pleasing. I remember, even when I was very young, that I wanted to bring home good grades.

I also wanted to do things that were pleasing in other ways. I had music lessons, voice lessons, when I was a very young child and still a treble. I would stand by the piano of my voice teacher and sing my heart out. We could not afford sheet music in those days. My mother would write out songs on pieces of paper for me to sing. This would have been in the late 1940s, and so they were often patriotic songs, such as “The Caissons Go Rolling Along” and “Off We Go Into the Wild Blue Yonder,” and other things that would reflect the fact that this was an immediate post-war period, when patriotism ran high.

All three of my uncles served in World War II. One was a chaplain, one was a lawyer, and one was a physician, who was actually in Alaska at the time when the Japanese bombed Pearl Harbor. That was an especially interesting time because they all felt like that perhaps the Aleutian Islands, where he was stationed on Kodiak, would be next. I heard these stories growing up, and I often associate my birthdate, June 6, with D-Day, but actually it was not my birthday, because D-Day was two years later in 1944. Our son, who is more of a historian than I said, "Dad, you really need to say you were born during the Battle of Midway, which was the turning point of the Pacific War in 1942." And I am probably saying too much about those early years, but I mention them because you mentioned my uncle who was a physician, and the country was oriented to service.

Growing up in a small town, we had probably on the order of 5,000 to 10,000 people. I do not know that we had an accurate census. I was not really aware of the professional opportunities that existed at that time. Professionals I knew were teachers, lawyers, ministers, and physicians. My father was in the banking business, and I told him very early on that I did not want to do that because he worked too hard.

My mother was a public school teacher. I did not know anything about diplomatic careers or other varied careers. Guidance counselors in high schools and colleges are now much more skilled at informing people and taking advantage of the skills they have, pointing them in the direction of different kinds of careers. So my choices appeared very limited. I decided quickly that I did not want to be a lawyer. And while I admired my minister uncle, I decided that that was not for me either, even though I was a person of faith and still am. But there was also my physician uncle. I respected his career, and I respected the fact that he was always needed in his community, where he served as a general surgeon. And so I admired him. And I think that probably even without encouragement from him directly, he figured into my decision when I was a sophomore in high school that I would enter the field of medicine. It was a definitive decision, and I remember exactly where I was. I just decided that's what I was going to do. And I really did not veer from that even though I thought about other kinds of career opportunities within a rather limited framework.

I do think it is important to point out that even though it was a small town, where we had only one public high school and no private high schools, we had teachers who were talented, including some special teachers who inspired me. One of them was the teacher of advanced biology, whose name was Kenneth Henry. He taught our advanced biology class when I was a junior in high school, and he came up with some novel projects. I recall that once, we were going spelunking, so we made a pretty detailed study of the creatures that lived in the dark world of caves. He also had the idea that we should study Rous sarcoma virus. Well, at that time—this was 1959—even a high school teacher could get Rous sarcoma virus from the American Type Culture Collection [ATCC]. So we ordered it and injected it chickens in the high school classroom and watched the tumors develop. It was an observational study. But a classmate friend of mine and I had already started doing some pathology on tissues. I set up a small laboratory in our laundry room at home with all of the chemicals, all of the solutions I needed to do a standard H&E stain and a Masson's trichrome stain. We took the tumors that were generated by the Rous sarcoma virus and did sections. We had an ancient microtome that a pathologist in nearby Springfield, Missouri, loaned us. I am not sure how sharp the blade was, but we did not cut ourselves. We made tissue sections, stained them, and looked at the tumor histology that was induced by the virus. Just to fast-forward, many years later I was headed to a meeting in New York with Harold Varmus [Dr. Harold E. Varmus] when he was Director of the NIH. I said to him: "Harold, I studied Rous sarcoma virus before you did." It was one of the viruses in his viral oncology studies. It was interesting, and it is true, I actually had used Rous sarcoma virus before Harold Varmus.

We could not have done that today in high school, but it was an interesting time, and it was an example of creative thinking on the part of this teacher. It encouraged us to think deeply about what was going on in the biological world. I am sure that his influence was far greater in my life than that of my uncle, whom I saw as a practicing physician. And I say that because I think in all likelihood, the research to which I eventually turned probably came out in that and similar experiences as much as anything.

Harden:Why would no one today let high school students have Rous sarcoma viruses? Is it because it is a retrovirus?

Wyatt: It is not distributed except under certain conditions because of the potential danger of it.

So here is a high school biology teacher who was stimulating us. Now I am not sure everybody was stimulated in the same way. One of my classmates is still the editor and owner of the local newspaper, and he probably does not think about that experiment that we did, but I do.

Also in high school, I was a particular fan of science fairs. Science fairs still go on; there are the competitions that take place. This was in the late 1950s, and we had a local annual science fair in my high school in Lebanon, Missouri. My first entry was a team entry. A friend of mine and I built an electric eye. When one broke the beam of the electric eye, various things would be activated. The next year I decided to try preparing tissue culture. And so I set up a home laboratory again. I incubated chick embryos, and dissected them under sterile conditions, and made some very primitive tissue cultures. I won first prize at the local high school science fair for that work. The next year, a friend and I did a comparative histological study of liver tissues from various species.

An important observation about this study relates to a family tragedy, because the following year, my mother died of cancer. She was already sick when I was making tissue cultures, when I had this science fair project on tissue culture. And one of the things on my poster said, "Tissue culture is used in cancer research." It was motivational for me, and we often hear about similar experiences when we interview medical students who have expressed an interest in coming to the NIH for a year of research. It is amazing how many of them have a personal story, a relative, a friend who has had a particular disease, and that has been a motivational influence that had caused them then to move in the direction of medical science. I did not wind up as a cancer researcher; I could have, I suppose. But an interest in infectious diseases took over and that is where I landed. But looking at character development and motivational influences, what happens in one's family may be a very important part of it.

Beyond that, I had a number of excellent teachers, but one who comes to mind specifically was my Latin teacher. Her name was Mildred Donald. There were about 30 of us in the class, but I think there were only two of us who really enjoyed Latin. We were the competitors; we were the ones who were vying for As. Ms. Donald was a very talented teacher. She introduced early on a love of language. My English was always fine. I never had a problem with English, even though I was raised in rural Missouri where often pronouns are especially misused. I never really even had an Ozark accent. And my parents spoke correct English, so I spoke correct English too. But I have to say that I learned the rules of English grammar through the study of Latin. It also induced this love of languages that continued later on. I was a Fulbright scholar in Germany following college. I was able to navigate German and spoke nothing but German for a whole year during that experience. I have also dabbled in Arabic and Spanish since then. I think that my ability goes back to an early exposure to the excitement of learning Latin, like learning a code.

Those are some of my high school experiences. Coming back to my parents, they were not aggressive in pushing me in one direction or another. I do not honestly know what they thought I should have done because I never asked them, and they never told me. When I told my father that I was interested in going to Germany on a Fulbright scholarship. He said, "Oh, I don't think you want to do that." And I simply said, "Yes, I do." And that was the end of the conversation. I think it is often different today among many parents who choose to influence and urge their children in various career directions. I was, I suppose, a self-starter, and I just wanted to learn. When I did my homework, I went in my room, closed

my door and had to have absolute quiet to study. Consequently, I did well in high school. I did have competition. One of my friends and I competed to be valedictorian. I think I beat him by something by 0.004 percentage points or so. That is a long story. I should also say that typing class was my nemesis, not the science courses.

Harden: After you graduated as high school valedictorian, you attended Central Methodist College in Fayette, Missouri, where you graduated summa cum laude in 1964. Why did you choose this college? What do you remember about your undergraduate years—what you majored in, who on the faculty influenced you? What sort of extracurricular and social activities? Just tell me about your college years.

Wyatt: I should note that as best I recall, about 15% of our high school class went to college, so going to college was not the norm. I had a very narrow frame of reference as to universities and colleges. I always thought I might like to go to Washington University since my father had a business colleague whose son went there. It was in St. Louis, and I fondly remembered going to St. Louis each summer when I was a very young child. With my parents and my physician uncle and his family, we would travel to St. Louis in the late 1940s where were exposed to things like television for the first time. I loved the puppet Howdy Doody. And we went to the Muny Opera [the St. Louis Municipal Opera Theatre, locally known as "The Muny"], which was not really opera at all. It featured musicals like "Annie Get Your Gun" and "Oklahoma!" We also went to a baseball game-- either the St. Louis Cardinals or the St. Louis Browns at that time, which subsequently became the Baltimore Orioles. I had very fond memories of St. Louis, including wonderful meals and exposures to city life, so I always thought Washington University would be a good place to go to college. I was turned off to it years later when I went to a rush party with the son of my father's business colleague. I thought that this life was not for me, and I backed away from Washington University for undergraduate studies. My father and I also went to Dallas, to look at SMU [Southern Methodist University]. But we really did not know how to approach looking at a college in those days, and we did not get sufficient information about it.

Central Methodist College then (Central College then, and Central Methodist University now) was about 100 miles from my hometown. I had had family members who went there, and I knew that it was more compatible with my lifestyle. The admissions counselor who came to my hometown to recruit was very positive about my going there, so I chose what was familiar and where some other students from my hometown were going as well.

The academic environment was solid but not necessarily challenging. There were some professors, however, who did challenge me and even more so encouraged me. One of our biology professors was George Vaughan [Dr. George A. Vaughan]. I thought his courses on embryology and histology were fascinating, and he was one who stimulated further my interest in biology. I did a senior research project under him in my senior year. That project took advantage of the Boone's Lick Salt Springs, in nearby Boonesboro, MO, in the Missouri River bottom. There were sulfur-containing hot springs from which flowed a sulfuric smelling water that eventually ran into the Missouri River. Daniel Boone's sons started a business there to collect and process valuable salt. I took water samples, brought them back to the college, and devised a method to grow the bacteria in special flasks. The Thiobacilli that grew out were particularly well-suited to grow in warm sulfur-containing water. The work was never published, but it

was interesting because thermophilic bacteria played a big role in PCR [Polymerase Chain Reaction] later on. This project points to one advisor who challenged me.

Again, language came into play because there was a German professor whose name was Nancy Leatherman [Nancy W. Leatherman]. She was a bit of a rebel because she did not get along well with the college administration, but she taught German very well. And she took some of us under her wing and taught us German customs and German foods. You can already see a pattern developing here. She was the one who learned about a summer language program in the summer of 1963 sponsored by Oberlin College. I applied for it and was accepted. I was the only student from our school. There were 50 students in the program, and I think 40 of them were from Oberlin. All summer long, we studied German in Vienna, Austria.

What was special about the program was that although I did not make 50 best friends for life, I did make about 3-4 good friends that I continued to follow. We made a pact that even though we were Americans, we would only speak German with one another that summer. For me, that was seminal because I consequently only spoke German for three months. It helped me tremendously as I returned to college as a senior to take full advantage of the remaining German courses and then to apply for and be able to study in Germany where I spoke nothing but German for one year. So here again, there was a professor in a small college who took an interest and encouraged me.

I'll mention just a couple of others. The dean of the music conservatory, Luther T. Spayde, was important to me. I sang in an *a capella* choir for four years in college. I learned a lot about music and about organ composers since he took us to concerts in nearby cities to hear famous organists, and I have continued to enjoy classical music ever since. Then there was also a chemistry professor whose name was, Chris Nielsen [Dr. Niels C. Nielsen], who also gave me good advice. One time he took me, in the early 1960s, to the University of Missouri, which was about 30 miles away, where Linus Pauling [Dr. Linus C. Pauling] was speaking one evening. It was memorable and fantastic to be able to hear a Nobel Prize winner.

Dr. Nielsen also taught me a guiding principle. I told him after I had been in Austria that I was thinking applying for applying for a Fulbright Scholarship. He said he thought that was fine, but if I got it, I should be committed upfront to accept it. He was a was a good influence, because I was prepared to accept the scholarship, if I was successful rather than be dissuaded by less adventurous influencers.

I was concerned about the academic environment of this small college, and I wanted to influence its improvement. We had about 1200 students as I recall. There were some other students who were likewise concerned. Five or six of us decided that as graduating seniors, we would petition to meet with the College Board of Curators when they came to town for graduation in 1964. They accepted our request to meet with them. We did meet with them and explained that we thought the school should improve academically, that there were opportunities being missed, that some of the faculty were really quite weak. We did make one strategic error. We did not let the president of the college know that we were doing this, and so he was rather disgruntled with us at best. We looked a bit nervously at our diplomas the next day at graduation, thinking they might not be signed, but they were. We hoped our small effort had a salutary effect. Today, this small school still exists, even as many small schools have closed. So maybe we did a bit of good by boldly stepping out and meeting with the curators.

Harden: Before we move to your time in Germany, let me just verify, was your major in biology or in chemistry?

Wyatt: It was biology, and I was pre-med. I had enough hours to have majored in chemistry or in German, but I chose biology.

Harden: As a result of your outstanding work—and your rebellious spirit, no doubt—you applied for and received a Fulbright scholarship to go to the Hygiene Institute, Goethe University, in Frankfurt. There you received the nickname "der Forscher" (the researcher) because of your interest in research. Tell me about this time in your early career, your year in Germany.

Wyatt: The year in Germany was life-changing for me. It was a cultural experience as Senator Fulbright [Senator James W. Fulbright] intended the program to be. I think if there were more Fulbright Scholars around the world, we would have fewer conflicts and wars today. I sought a cultural experience by living with a family for the entire year in Koenigstein im Taunus near Frankfurt. I drove into the city to attend classes and take part in a practicum—i.e., experience the work of public health laboratories at the Hygenie Institut of the Goethe University, Frankfurt. It was right next to the Paul Ehrlich Institute in Frankfurt. I used to sit in the lab and look out and see the Paul Ehrlich Institute and imagine Paul Ehrlich [Dr. Paul Ehrlich] sitting at the laboratory bench next door doing his work. I really had a sense of history when I was there.

I worked in the typhoid lab, in the TB [tuberculosis] lab, and in the Varia lab where we processed all sorts of infectious specimens and learned about phage typing of Salmonella strains. It was a good scientific experience, but it was even more so a cultural experience, and one that I treasure. There was some evidence of breaking down cultural barriers, as when I was invited to a late afternoon lab coffee in a home. I visited with the husband of a lab colleague, who got to know me. At the end of the evening he said, "Herr Wyatt, Sie sind eigentlich kein Amerikaner, Sie sind ein Mensch!" [English: "Mr. Wyatt, you are actually not an American, you are a particularly good person!"]

Harden: How many students were there with you?

Wyatt: I was the only American.

Harden: You were the only American. And were there German students or was it mostly—

Wyatt: There were German students in the classes, but I was working mainly with the professors and the doctors in the laboratory. I was quite junior to them. And that's important because there was a Herr Professor—do you know the term "Herr Professor?"

Harden: Yes.

Wyatt: The professor was the ruler—the king of the lab. He was perhaps old-fashioned; actually, he was very old. And he would come into the laboratory to inspect the lab. Everybody would stand at attention

when he came in. His name was Herzberg, Professor Herzberg. One day I chose to speak to him, and my physician colleagues were aghast. They looked at me and said, "What do you think you are doing?" because I had actually talked to the Herr Professor. Well, it turned out that he liked me. Another day, he invited me along with a couple of other colleagues from the laboratory to his house for coffee. By that time, I had made one trip to Greece, and it turned out he really liked Greece. And he said I should bring a few slides with me. We had a wonderful afternoon having coffee and cake and talking about Greece, all of this, of course, in German.

You can already see the emerging life themes: biology, language, music, etc. Another image was emerging, “the researcher”—”der Forscher”—in German—the name they called me at the Hygiene Institut. They knew I was interested in research, but of course there was not too much I could do, as I had just graduated from college. But I did do one experiment. We had a small flock of sheep at the Institute, and we used sheep red blood cells to do a heterophile antibody test (Paul Bunnell test) to diagnose infectious mononucleosis. I tested all of the sheep with a set of standard reagents and test samples to learn which sheep would give the best results. This was my one little practical project during my Fulbright year in Germany.

Harden: When you finished your Fulbright year, you came back and enrolled in Washington University School of Medicine in St. Louis. During that time, you also had an opportunity to conduct research in Central America, but I want to set that aside for now and ask you to tell me about your medical school experience in general. How did your application happen? Did anybody recruit you or did you just decide that's where you were going. Who influenced you, what kind of personal professional relationships? Just talk about medical school for me.

Wyatt: First of all, I will say that, in contrast to today's aspiring medical students, I applied to three schools. Today, I think the average student applies to perhaps more than 25 medical schools.

Harden: Wow.

Wyatt: I also took the MCAT [Medical College Admission Test], which did exist at the time. But somebody told me, “Well, if you are going to go to medical school, you have to take the MCAT.” And I said, "Okay, where do I take it?" And I went over to Columbia, Missouri, and took the MCAT, but without any preparation whatsoever. And now people spend quite a long time preparing for the MCAT, but apparently my test results were okay. At any rate, I applied to Northwestern, Harvard, and Washington University [Washington U]. For some reason, my application to Northwestern was incomplete. Harvard turned me down.

Washington U was very happy to accept me. The problem was that when I was initially accepted, I had a pending Fulbright application. When I was accepted to the Fulbright program, I had to call Washington U and very boldly say, "I have an opportunity for a Fulbright. Could I please defer my acceptance for a year?" They not only said, "Fine," they gave me a small scholarship to go with it. So the medical school application process worked very well for me.

As we are discussing decision-making, I would like to comment here that I am a person of faith. I believe that as we go through life, there are all of these decisions that sometimes seem random. When I look back on them especially in retrospect, I see God's Providence at work. Perhaps that is not something that you hear too often in an interview like this, but it is my conviction that I have had throughout my career.

I was accepted at Washington U, and I gladly accepted. We had a class of 87 students in 1965, which is small by today's medical school class standard. We had three women in our class. The mid-1960s marked the end of male predominance in medical school classes. It changed quickly after that, and today it is at least 50/50. I attend reunions at Washington U and enjoy them. I also contribute to our Class of 1969 Scholarship Fund that has worked to improve diversity of the student body.

When we arrived, the dean of students John Herweg [Dr. John C. Herweg] said to us, "We have accepted all of you, all 87, and we fully intend to graduate you." He said there was no intention of failing us. So, we started our time in medical school with this commitment on the part of the faculty, and I always felt like I enjoyed strong faculty support.

Let me add a couple of experiences to illustrate. In October of my freshman year in medical school, I had the misfortune of breaking my ankle. One night I took a study break and went ice skating, and I broke my ankle at Steinberg Rink in Forest Park. I was hospitalized for an open reduction at Barnes Jewish Hospital in St. Louis. It was unfortunate, but I realized at the time how much the faculty was behind me. Some of them came to see me in the hospital, even though I did not know them that well. When it came time for an anatomy quiz exam, and we had to go from cadaver to cadaver, I was on crutches, and they allowed a little extra time just so I could make it between the cadavers. These were signs that they were supportive. One of my professors brought a microscope over to my hospital bed and I did a histology exam with the microscope on the overbed table. As a result, I've always felt very positive about Washington University.

When I was a freshman, I decided I wanted to experience something in international health, in global medicine. I had to decide whom I wanted to ask about it, because although the faculty in general were supportive, there was one professor I was sure knew my name. She was Sarah Luse [Dr. Sarah A. Luse], the neuroanatomy professor. She took it upon herself to learn the names of all 87 medical students. When she would see us, she would call us by name. And I said, "Well, there is one faculty member who knows my name." So I went to her and said, "Dr. Luse, I am interested in infectious diseases, and I am interested in international global medicine." She helped me by sending me to the public health person on the faculty, whom I did not know. Dr. Luse was rather amused by my interests. She said, "You must be one of those missionary types," and I replied, "Well, maybe." At any rate, she was wonderful and very helpful; I appreciated her interest and support.

The other person who comes to mind immediately was the anatomy professor, Mildred Trotter [Dr. Mildred Trotter]. She was already very senior at that time. She took a personal interest in us, and she also taught us respect for the cadavers so that when we had an exam—that involved going from cadaver to cadaver—she told all of the men that we should wear a tie out of respect for the cadavers. She also set limits for us, and she once dramatically drew up the rubber sheet over our cadaver at the end of class

and said, “Mr. Wyatt, it is time to stop now. The saphenous vein will be there tomorrow!” She cared about students.

Harden: Interesting.

Wyatt: There is another person I want to mention: Bill Danforth [Dr. William H. Danforth], who during our time at Washington U. Medical School was the Vice Chancellor for Medical Affairs. He went on to be Chancellor at Washington University. I have to tell this story because I actually spoke with him shortly before he died and brought this episode full circle. A small group of us, perhaps five or six students, made an appointment and went to his office to protest the Vietnam-American War. As much of the medical community at the time was, we were very concerned about what was going on in Vietnam. I wore a black armband as a sign of my disapproval of the war. Now of course, this was not uncommon among physicians and students, but it probably would not have been thought of very positively in my hometown in southern Missouri—there were divisions even then. After we explained our concerns, Dr. Danforth listened very thoughtfully, as he was a mild-mannered man. I can't even remember the specific words that he used in response, but he listened, and we talked for a while always with civility and respect. To come full circle: about five or six years ago, I was at an alumni banquet in St. Louis. And he was sitting at the table next to ours, so I went over and said, “Sir, I met you before when I came to your office to protest the Vietnam War in 1968.” And of course, 50 years later, we both chuckled about it. He had been in a particularly difficult position though, because James McDonnell [James S. McDonell], who headed McDonnell-Douglas Aircraft Corporation, was a principal benefactor of Washington U. And he also built fighter jets for use in Vietnam. So you could see that when we protested the Vietnam War, Dr. Danforth was in a difficult position.

But now let me get to the most important person at Washington U as far as my future career was concerned. That was Ralph Feigin [Dr. Ralph D. Feigin]. I was a student, and I wanted to work in laboratory at the medical school. I knew that there was a young faculty member in pediatrics who had just come from Mass General [Massachusetts General Hospital] and that he worked in infectious diseases, a subject of interest to me for nearly a decade. I went to his lab and asked if I could work there with him. It turned out that I was the very first medical student at Washington U who had asked him to do that. He said, "Fine, that would be great." So I was in this big lab with him, just the two of us. His mentorship in clinical research, clinical trials, and infectious diseases was seminal in what followed. I am sure he was part of the reason that I wound up at the NIH, because he was one who was in the best position to recommend me. And so I worked in his lab, but to put this in context, he went on to Baylor Medical School where he became Physician-in-Chief of Texas Children's Hospital. He was also Chair of the Department of Pediatrics and the President and CEO of Baylor College of Medicine.

He had an illustrious career and was a brilliant man. But I was with him when he was a clinical instructor before he had even had the rank of Assistant Professor. For everything we did, we had no technicians. He and I together built an Amicon amino acid analyzer in the laboratory, just the two of us. It was a wonderful experience. I have always felt that I never properly thanked him for everything he did for me. He actually offered me a job in St. Louis about five years after I had been at the NIH. By that time, I was deeply involved in the viral diarrhea work in NIAID and could not see making the move. But I did go interview, and I was offered a position as Assistant Professor, but I turned Washington U down. Shortly

after that, Dr. Feigin left to go to Baylor, so I have a feeling that I would have wound up in Texas if I had gone to St. Louis, because I would have tried to follow him to Texas.

Harden: Let's drop back a bit at this point. I want to talk about your time in Guatemala, but that has to be prefaced with the question of what it was about infectious diseases that drew you—as opposed, say, to neurology or some other field? And then I want you to tell me about, let me read this into the record: From April to August 1967, and then June to September 1968, you had a traineeship in clinical research at the Institute of Nutrition of Central America and Panama, INCAP, in Guatemala City. And I am pressing you especially about this because you noted that this traineeship was a formative influence on you about global medicine and the importance of diarrheal disease. I also note that this work produced your first publication with Leonardo J. Mata [Dr. Leonardo J. Mata Jiménez].

Wyatt: If we go back to my experience in high school with Rous sarcoma virus or in college when I grew Thiobacilli, those were microbes that first interested me. Then there was my time in Germany when I was working in the Hygiene Institute and doing all kinds of microbial cultures, e.g., Salmonella, E. coli, TB bacilli, etc. I think it followed naturally that when I went to Guatemala that I would be interested in infection and immunity. Also remember that it was a neuroanatomist who recommended that I talked to the public health person who had said, "Would you like to go to Guatemala?" In my ignorance that at that time, I did not know exactly where Guatemala was. But I said, "Sure, that would be great."

I was there two summers. During the first summer, I worked in a physiology lab without strong mentoring. It was in the second summer that I chose to work with Leonardo Mata, whom I considered a true mentor. He helped me understand his work on the microflora of the intestinal tract—both aerobic and anaerobic, well before the study of the microbiome was fashionable. We were doing field work. I was interested in the anti-infective properties of human breast milk, and we developed a collection of breast milk samples from women at various stages of lactation and took them back to the laboratory. We measured their anti-infective properties—e.g., their ability to neutralize poliovirus and their ability to neutralize Salmonella in vitro. Eventually we studied the IgA [Immunoglobulin A] antibody levels in those stored colostrum and milk samples of the Guatemala women. And remember this was 56 years ago.

IgA was known, but it was still rather novel work at the time, so Mata truly stimulated my interest. We would go often up to the village Santa Maria Cauqué in the highlands of Guatemala—the late Dr. Mata wrote a whole book on Santa Maria Cauqué (The Children of Santa Maria Cauqué: A Prospective Field Study of Health and Growth, The MIT Press, 1978). One day, when I was in the village with one of the social workers and collecting milk samples, we walked into a house where there was a young child who had just died with severe, dehydrating diarrheal disease. But the image struck me, and I've never forgotten it, as we walked into that adobe brick home with a dirt floor. There was an open fire where they made tortillas, and there was a child who had just succumbed to diarrheal disease, which should not have happened. He was not the only one—there were many children in that era who died of severe diarrheal disease in the absence of therapy, including oral rehydration, but this particular one had an impact on me, and the image stayed with me. Perhaps he had rotavirus, which I studied later in the laboratory and participated in developing the first FDA-licensed rotavirus vaccine.

Harden: Did you learn Spanish while you were in Guatemala?

Wyatt: I learned to babble. First of all, people spoke English in the lab. But I also needed to be able to do things like go to the barbershop. There was an IGA institute in Guatemala City where one could practice speaking after listening to tapes. I learned enough Spanish that I could get by, but it was not great Spanish, and I still do not read much Spanish, so I suppose I would be labelled “illiterate” in Spanish.

Harden: From 1969 to 1971, you did what used to be called an internship and now is called a residency in pediatrics. Perhaps you can tell me why pediatrics and if that was associated with your experience in Guatemala. You were at the St. Louis Children's Hospital in St. Louis, Missouri. Tell me about this period, both as a physician treating children and the comparison between the kinds of diseases you addressed in St. Louis and what you'd faced in Guatemala.

Wyatt: Well, it is interesting because I almost did not do those two years of house staff training in St. Louis. I remember sitting at my dining table as a senior medical student saying, "Do I really want to do clinical house officer training?" I already knew I did not want to take care of patients all my life, and I really wanted to do research. So I had this internal debate, and I am sure I involved others with it. But eventually, I decided that two basic years at least of house officer training--learning to be a real doctor—was an important part of my career. As I thought about what I wanted to do, I thought about the specialties I had been exposed to in medical school. When I broke my ankle as a freshman, I had an experience with orthopedics that was attractive. I remember meeting in the casting room a boy who had Marfan’s syndrome who required a spinal fusion. And the idea of fusing a spine, of the very technical work involved, struck me as being positive. And of course, orthopedic surgeons are at the top of the pay scale compared to pediatricians who were at the bottom of the pay scale, but that did not enter the equation.

And then as I went through the various rotations during my residency, I really enjoyed ophthalmology and thought that ophthalmology would be interesting, Again, it is also one of those very lucrative specialties. But as I considered the subject matter, I was really taken by infectious diseases and pediatrics. Already the two were merged because of the one dominant person during my medical school years: Ralph Feigin. I worked in his lab, and he was also an attending physician in pediatric and infectious diseases. He inspired me and became my first real constant mentor and advocate.

We had different diseases in those days than they have now. Bacterial meningitis and septic arthritis, caused by Hemophilus influenza type b [Hib] infection, have basically been eliminated by virtue of a vaccine developed by John Robbins [Dr. John B. Robbins] and his colleague Rachel Schneerson [Dr. Rachel Schneerson] at the NIH. The Hib vaccine protected infants from infection caused by H. influenza type b, and it changed the world.

Going back to my final year in medical school, the curriculum at Washington U allowed senior medical students full flexibility in what we studied. So apart from two rotations, one in internal medicine and one in ophthalmology, I did nothing but infectious diseases research in Dr. Feigin’s lab during my senior year of medical school. Even though my interests may have begun back in Lebanon and Fayette, Missouri with growing Rous sarcoma virus in vivo and Thiobacilli in vitro, respectively, my interest in infectious diseases continued all the way through medical school and into residency. As I was applying for a

Research Associateship at the NIH, most of the opportunities for which I applied had to do with infectious diseases or immunology, specifically local immunity stimulated by breast milk studies in Guatemala.

Harden: Let me ask you a question I ask every physician. There are four, mainly three areas that physicians can choose to follow. You can be in private practice and treat patients. You can go into public health, which you have not mentioned specifically. You can go into research, or you can go out into the private sector, into the pharmaceutical industry. What kinds of things really influenced your decision to stay in research over the other possibilities?

Wyatt: I think at one point I could almost hear myself saying, "I do not want to spend my life treating otitis media in pediatric patients." Infectious diseases, yes, but I did not want to do the same thing repetitively as in the emergency room, which was probably my least favorite of the rotations as a house officer. In research, there would always be a new challenge; there would always be questions and opportunities emerging in the laboratory; it is just the way science is done. From the very beginning, my colleagues in Germany clearly recognized that I was interested in the process of scientific discovery. I decided against clinical practice, but I am very glad that I spent two years doing patient care. I still remember some specific patients very clearly—e.g., a five-year old Type I diabetes, an infant with subdural hematomas, children with then fatal acute lymphocytic leukemia, and others.

And as for public health, it of course includes a broad spectrum of disease and disease patterns including epidemiology like in the Laboratory of Infectious Diseases, NIAID, where I worked in the Epidemiology Section. I remember thinking to myself that if I could work on a disease of widespread importance like diarrheal disease, as an example, I could help more people overall than by treating individual patients by starting IVs and doing that kind of thing. So the conclusion that I wanted to engage in infectious diseases and global health was a gradual but clear conclusion.

Harden: As you finished your two years as a house officer, the Vietnam War was still raging, and the federal government was drafting physicians. You had to face the issue of what you were going to do next. At the suggestion of a classmate, you applied for a residency deferment known as the CORD program, the Commissioned Officer Residency Deferment in the United States Public Health Service (USPHS). You were accepted and came the NIH into the Laboratory of Infectious Diseases [LID] headed by Robert Chanock [Dr. Robert M. Chanock] as a Research Associate in the program whose participants gave themselves the self-deprecating moniker of Yellow Berets. Will you tell me about this career move? How did you link up with the Chanock lab? What other labs might you have been interested in? Why did you choose to be a Research Associate, not a Clinical Associate? I want to note that Dorland Davis [Dr. Dorland J. Davis] was Director of NIAID in 1971. And John Seal [Dr. John R. Seal] was Scientific Director. And so in the process of telling me, I wonder if one of them made you the actual offer or did it happen differently? Tell me how it all came about.

Wyatt: I have to return to my senior year in medical school because yes, I learned from a classmate, David Zopf [Dr. David A. Zopf], about the CORD program at the NIH. My professors did not tell me about it; it was David. I got the booklet from the NIH, I still have it and should pass it on to the NIH history office.

Harden: Yes, indeed.

Wyatt: But, yes, all male physicians of that era were subject to the doctors' draft. Even though I was a pediatrician, and likely not destined to be sent to a war zone, we all had two years of military service to anticipate. I applied to the Public Health Service (PHS) for the NIH because I was interested in research. I would also have gladly served in another of the other agencies of the Public Health Service—IHS, FDA, etc.—although my life would have been very different. I also applied to what was called the Berry Plan [named after Assistant Secretary of Defense for Health and Medical Affairs, 1954-61, Dr. Frank B. Berry]. The Berry plan allowed physicians to complete their medical training and then practice their specialty in a residency for two years before fulfilling that military obligation. I applied to both the PHS and the Berry Plan, and I was accepted in both. And of course, there was no question which one I was going to accept, because one meant I could do research and the other one meant that I would be practicing pediatrics. An envious classmate wanted my unused Berry Plan slot, but it was not transferable.

When I chose the NIH/PHS, I was in the mindset that this was the way I was going to serve my country for two years and fulfill that obligation. It did not upset me that I had two years to serve. I was accepted to the program in 1969 but for two years heard virtually nothing from the NIH. I had come to Washington and made a list of labs that interested me. I was interested in the Chanock lab because it focused on common infectious diseases, and I interviewed with him along with Dr. Albert Kapikian [Dr. Albert Z. Kapikian] in that lab. I was also interested in a laboratory in the Dental Institute [then the National Institute of Dental Research], Stephan Mergenhagen's [Dr. Stephan E. Mergenhagen] lab. He was interested in local immunity, and that was also of interest. So I interviewed with him as well. I also interviewed with Dr. John Seal, who as the Scientific Director of NIAID represented two attractive laboratories overseas, again the international lure. At that time, NIAID had a laboratory in East Pakistan, subsequently Bangladesh, the SEATO [Southeast Asia Treaty Organization] cholera lab. NIAID also had a laboratory in Panama, the Middle America Research Unit. Dr. Seal interviewed me for both, but when it came for me to rank the labs, my first choice was the Chanock lab as where I wanted to be, because they were doing research on vaccines against common infectious diseases.

This reminds me of another high school experience as my public health interests developed. In my junior year in high school (1959), I was selected to go to Missouri Boys' State as a representative of my rural hometown in southwest Missouri. The program was held in Jefferson City, the state capital. We spent roughly a week learning about government and then conducting a mock campaign and election, as we learned what it took to be a politician. I was not terribly stimulated by it, maybe because I am still not politically inclined. It turned out, however, that I was in a precinct from which the "governor" was elected. The governor had all of these executive offices to fill, so he made me the Secretary of Health, which meant that I could develop a legislative proposal. My legislative proposal was to require vaccination in the Missouri high schools. At that time, this would have included only the basic DPT—diphtheria, pertussis, and tetanus—along with poliovirus immunization. My argument was that this requirement would make the schools safer places to be. We turned in the proposal, but it was defeated in our Missouri legislature. Maybe this experience was a harbinger of an antivaccine movement 65 years ago!

Harden: Was there discussion about why other student legislators voted against it.

Wyatt: I do not recall any discussion about why people voted against it, but I do remember that Missouri was often a divided state philosophically. It was divided in the Civil War, and it is still divided today. The big cities are blue, and the rural area where I am from is red. My legislative proposal would likely have been defeated today too.

Harden: Let's return to your interest in the Chanock lab because they were working on vaccines and your arrival at NIH.

Wyatt: When I finally arrived at NIH two years after the PHS CORD selection process, I knew where I was going to work, because I had been informed that I was selected for the Chanock lab. When I arrived at NIH. I signed in on July 4, 1971. Little did I anticipate that this would begin a 52-year career. All of us new Associates went over to the Clinical Center (Building 10) to sign in formally. The ACRF [Ambulatory Care Research Facility] had not been built yet. We walked in the front door of the old Clinical Center (original Building 10), signed our name with date and time, and left. I was not the only one who signed in that day. There was also Michael Gottesman [Dr. Michael M. Gottesman] and John Gallin [Dr. John I. Gallin], along with many other people. Tony Fauci [Dr. Anthony S. Fauci] was already here. He had been here perhaps about three years at the time.

Harden: Yes. He arrived in 1968.

Wyatt: Of those people who came in the class in 1971, very few stayed at NIH for a whole career. Most people came for two years or maybe three. Then they went off back to a university medical center, to an academic environment, or to industry, to fulfill their career. NIH was really a training ground for researchers in those days. Several of my infectious diseases USPHS Commissioned Corps colleagues returned to academia, including Dr. Raphael Dolin, Dr. Harry Greenberg, Dr. Doug Richman, and Dr. Tom Thornhill. I consider having been a part of the Yellow Berets to be a badge of honor, because being in the program was foremost an honorable way to serve our country, to be able to make contributions to health of the American people. I do not object to the term at all. I remain happy to be called a Yellow Beret.

Harden: The contribution to academic medicine by the Yellow Berets has been widely documented. I had a physician friend at Michigan who was very proud to say he was trained by a Yellow Beret.

Wyatt: One of my colleagues in the Chanock lab whom I mentioned, Tom Thornhill [Dr. Thomas S. Thornhill], spent two years working on hepatitis—hepatitis particles, and Norwalk virus-like particles. When he left NIH, he went back to Massachusetts and became an orthopedic surgeon and the head of orthopedics at the Peter Bent Brigham Hospital [now Brigham and Women's Hospital]. That is what happened to a lot of physicians. They moved back into academia and took on leadership positions in academic departments.

Harden: Before we get into your research in the Chanock lab, I want you to draw me a verbal picture of the lab itself. It was in Building 7, I believe, the Memorial Laboratory. I know that Dr. Kapikian was a more senior person at that time, along with Dr. Chanock. But tell me, who else was there, what was the setup?

Wyatt: We know that Building 7 has now been demolished, but at the time, in 1971 when I arrived, it housed functioning laboratories. There were signs that it was an infectious diseases laboratory in that there were UV [ultraviolet] lights everywhere. It had some old burners, exhaust burners that were used to suck the air out through these electric grids to kill microbes. And I think there was a history of some people dying as a result of Q fever. But Building 7 also had a history of having been very well constructed. It was basically bombproof with foot-thick, reinforced concrete walls. In other words, if there were an explosion, it would not have spread microbes around Bethesda.

Harden: Interesting.

Wyatt: And the windows did not open, of course. It took quite an effort to deconstruct it, to demolish it, because of its reinforced concrete construction. It had been designed as a laboratory, and it looked like a government laboratory. It had gray-green tile walls, and it had many noisy freezers in the narrow hallways. The hallways were actually too narrow by today's standards, but freezers were essential because of all of the infectious samples. It had small laboratories and even smaller cubicles. Biological Safety Cabinets were installed only later. We would go into a cubicle, close the door to the cubicle and have a somewhat contained space where we could process the samples for the study of norovirus disease. When it came time to prepare our bacteria-free filtrates of diarrheal stools, we did them in a cubicle so that if there were a problem, it would be at least contained at the cubicle level. Today, it would be done in a biological safety cabinet, not in the cubicle. There was an old lab entry system where one could go in through the outer offices and then through an area where one could shower in or out, I suppose. But when I arrived, the showers weren't used anymore. I did hear that the shower had been used once to house alligators as a source of red blood cells for hemagglutination tests. There was an autoclave in the area connecting the labs to the office, and we used ethylene oxide as a sterilizing agent.

At any rate, the Chanock lab in Building 7 was still a state-of the-art infectious diseases lab. It was a good place to work. It was full, the cubicles were full, there were technicians as well as Ph.D.s and M.D.s. I remember when I first arrived, there was a physician there whose name I mentioned earlier, Ray Dolin [Dr. Raphael Dolin], who had been there a year already, working on norovirus diarrhea. He was also in the Commissioned Corps, and I think he stayed a total of three years before he went back to Boston to complete his house officer training and then go on to be a professor of medicine. Neil Blacklow [Dr. Neil R. Blacklow] was also there, who was a young physician at the time also working on what we later knew as norovirus. There were other new Commissioned Officers. Brian Murphy [Dr. Brian R. Murphy] was there. Peter Wright [Dr. Peter F. Wright] left the year I came. He was one of the Commissioned Officers who spent time at NIH, went to Vanderbilt, and then trained others. Peter was one who trained Barney Graham [Dr. Barney S. Graham]. In talking with Barney recently, I discovered this scientific family connection based on the Chanock lab in Building 7.

Building 7 was crowded and was not spacious by any means. The electron microscope was in the subbasement, and it was like going down the narrow steps into a submarine to use the electron

microscope; that is where we did our studies on both norovirus and rotavirus. The Chanock lab was functional, albeit crowded, and it was filled with many people talking to one another—an early example of team science. I was in an interior office with two other people without windows for over 12 years. One of the things I said when I left there was, "I never want an office again without a window."

Harden: Who were the senior people besides Dr. Chanock and Dr. Kapikian in the Laboratory of Infectious Diseases?

Wyatt: Bob Purcell [Dr. Robert H. Purcell] who, incidentally, was a close colleague of Harvey Alter [Dr. Harvey J. Alter], a recent Nobelist, who came around the lab often.

Harden: How did they divide up their work?

Wyatt: Chanock was the most senior and the Laboratory Chief. Both Bob Purcell and Al Kapikian were Section Heads, or Section Chiefs, which is still the kind of structure that exists in some places at NIH today. And I am trying to think if there weren't any other Section Chiefs. There were scientists in other labs and lab chiefs in Building 7, including Wally Rowe [Dr. Wallace P. Rowe], Jan Hartley [Dr. Janet W. Hartley] , Roger Cole [Dr. Roger M. Cole], and others. Notable scientists were coming and going all the time in Building 7. Among the international visitors, I will mention just a few: Sir Charles Stewart-Harris [Sir Charles H. Stewart-Harris] from England, David Tyrrell [Dr. David Tyrrell] from England, Frank Fenner [Dr. Frank J. Fenner] from Australia, Ruth Bishop [Dr. Ruth F. Bishop] from Australia, and Ian Holmes [Dr. Ian H. Holmes] from Australia. Albert Sabin [Dr. Albert B. Sabin] was also a frequent guest in Building 7.

The lab was dynamic. There were a lot of creative ideas floating around and a lot of active discussions going on. And I have to say that Bob Chanock and his wife Beth were wonderful to invite the fellows, senior NIHers, and outstanding foreign scientists to their house for dinner. Beth and Bob were wonderful hosts for such events, and they really made us feel like we belonged to the laboratory family. That was something that Bob Chanock did very nicely as he not only included us for meals, but also assured that we fellows were invited to top scientific meetings. For example, in those days, the Infectious Diseases Society met in Atlantic City, and one could not ordinarily just drop in, but Bob would make sure that all of his fellows got invited. There was another meeting that was held in New York every year, called the Gustav Stern Symposium, "Perspectives in Virology." It was a meeting funded through the Hartz Mountain bird seed company as a sign of gratitude to the scientific community for its work on psittacosis. It was always an elegant meeting organized by Dr. Morris Pollard from Notre Dame University, but it ended in about 1980. Some senior and junior speakers would be invited to present. Bob Chanock always made sure that all of his fellows were invited to go to that magical meeting. It was really quite nice; I remember that one time the banquet was held in the Park Plaza Hotel—I mean quite nice! The first time I went to that meeting, I met Mrs. Peyton Rous. You have already heard me talk about the Rous sarcoma virus that was discovered by her husband. He had died earlier, but I actually met his widow, and I'll never forget having met her. I do not remember whether I told her the story about my brush with the Rous virus in high school.

In 1978 I was invited to present on norovirus at that Perspectives in Virology meeting in New York. I was the junior person on the program, and also on the same program was David Baltimore [Dr. David

Baltimore], one of the senior presenters. Dr. Albert Sabin was sitting in the front row as I gave my talk, and I was afraid he would ask me a penetrating question that I could not answer. He was silent, and I remember that he had a cold that day and may not have been at his best. Dr. Fred Robbins (1954 Nobel Prize for poliovirus work) moderated our session, and he remarked about my comment that norovirus diarrheal stools that were rich in virus were "golden," that "beauty must be in the eye of the beholder !"

END OF FIRST SITTING

This is the second sitting of the oral history with Dr. Richard Gregory Wyatt on November 6, 2023, about his career at the National Institutes of Health. The interview is being done over Zoom, and the interviewer is Victoria Harden.

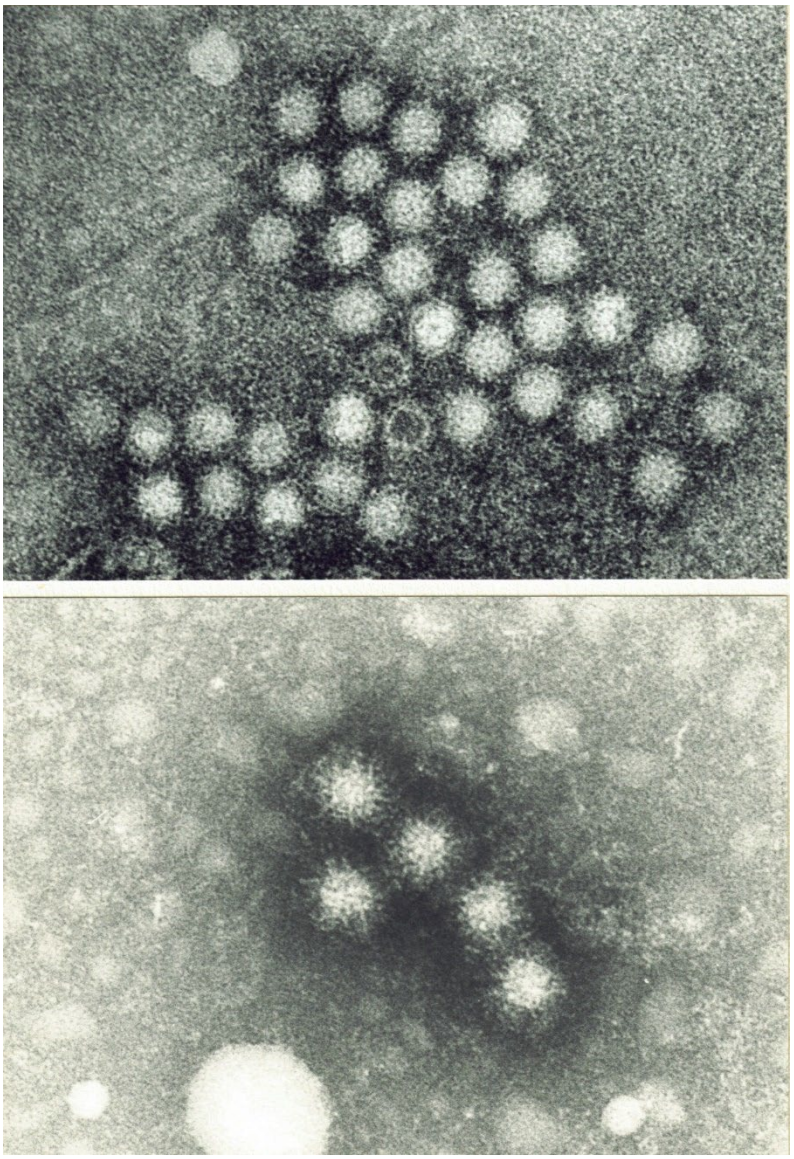
Harden: Dr. Wyatt, we stopped at the point where we were about to talk about your laboratory research. You said that when you came to NIAID, you did not know which of three infectious diseases you'd be working on. There were three options: respiratory syncytial virus; hepatitis, which at the time was largely hepatitis B; and a new project that had just started a year or so earlier on infectious diarrheas. You decided on the infectious diarrheas, specifically as it turned out, those caused by norovirus or Norwalk virus and by rotavirus. The result of the rotavirus work eventually led to the first FDA-licensed rotavirus vaccine in the 1980s and to two patents, for one of which you were first author. So let's start with just the rotavirus research. Would you walk me through your rotavirus research?

Wyatt: Well, I tend to be a chronological person, and I would prefer to talk about norovirus first because the story flows nicely.

Harden: That's fine.

Wyatt: I should add a clarification about my decision to study infectious diarrheas, because it was actually decided for me. Three of us ("Yellow Berets") arrived in the Chanock lab at the same time, and there were three research opportunities: respiratory syncytial virus (RSV), hepatitis, and infectious diarrheas. One colleague, David Hodes [Dr. David S. Hodes], was also a pediatrician like his father (Dr. Horace L. Hodes, Mount Sinai Hospital in New York), wanted to study RSV. Stephen Feinstone [Dr. Stephen M. Feinstone], the next colleague, wanted to study hepatitis; Steve wound up being part of the team that detected and discovered the hepatitis A virus using immune electron microscopy just like we used it in LID. I was fine with infectious diarrhea (remember my story about the Guatemalan boy I saw who died with diarrhea in Guatemala).

As for norovirus, that was a name that came much later. We called it the Norwalk virus or Norwalk agent, because we prepared to study a bacteria-free filtrate from diarrheal stool samples from an outbreak in Norwalk, Ohio, that was studied by the CDC. But we could not grow anything from the filtrate in the laboratory. We could not infect animals with it then. But we discovered—and I should say investigators in the Chanock lab found a bit earlier that by administering this bacteria-free stool filtrate of stool to volunteers, about 50% of them developed various combinations of self-limited vomiting diarrhea, and/or fever.



Norwalk Virus Particles with and without antibody, electron microscope image. Laboratory of Infectious Diseases Researchers.

Now, most people today, when they think of norovirus, think of the cruise ship outbreaks that occur or family outbreaks that we're all familiar with, where as many as half or more of a family or shipboard passengers develop a relatively acute diarrheal disease lasting generally 24 to 48 hours and leaves them feeling washed out and crummy for a day or two. But really crummy, so it is not something that we can just brush off. One does not generally just keep going with norovirus diarrhea. But at the time, all we really knew was that we had a filterable infectious agent, but we did not know what it was. Many of the early studies as we began to try to characterize it using our human volunteer model, pointed us to the possibility that it might be a parvovirus, because it was small and very stable, but that did not really pan out.

All of our attempts to grow this filterable agent failed. When I first arrived in the lab in 1971, that was my assignment: grow whatever it was. I worked all that year at the NIH, trying to grow a virus that firmly resisted growth in the lab. And I was somewhat vindicated when some 40 years later, Mary Estes [Dr.

Mary K. Estes], working in Texas was unsuccessful. I said, "Well, I could not grow it 40 years ago, and it still can't be grown today." There have been some successes in cultivating it more recently, but only under very stringent conditions. We called it a “fastidious virus.” That's the word we used.

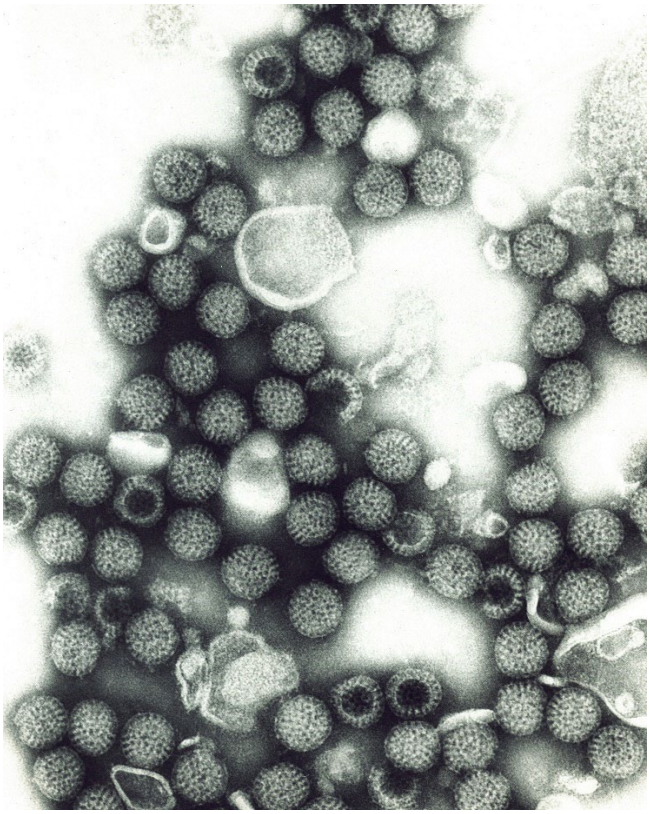
But going back, it was really Neil Blacklow, who was in the lab at the time, who got these studies going along with Ray Dolin, both of whom subsequently went on to work in academia. Blacklow and Dolin were doing these studies in volunteers. They would collect small stool samples from different outbreaks around the country, around the world. And the one that yielded initial success was the one from Norwalk, Ohio. We called it “Stool Pool Number 8.” So that gives you an idea that there were ones that came before and after. It was not the only one that was infectious in volunteers. There were others as well, and they got assigned different names and numbers. One was called the Hawaii Agent, because the family of a young USPHS Commissioned Officer who was stationed in Hawaii provided samples when they had a family outbreak. One was called the Montgomery County Agent, again, from a colleague's family who lived nearby. In short, we had a whole series of stool filtrates in the freezers.

Once we had success in reproducing disease, what we did was to passage that material. In other words, we took the fecal material from the volunteer, processed it into a similar bacterial-free filtrate, and passed it to another volunteer and then to yet another volunteer. We were demonstrating that it was transmissible in a serial fashion. Each time we did this, we tried to grow filtrates in the laboratory in tissue culture with each passage, we tried to grow the agent and test for some indication of disease in animals; each time we failed. But we were able to study the pathophysiology of the disease in these volunteer studies. By treating the infectious filtrate with heat, acid, solvent, etc., we realized it was quite stable to destruction. We even tried to determine the density of the virus by means of ultracentrifugation in potassium tartrate. At one point, some collaborators in the Clinical Center did small intestinal biopsies, thinking that we might be able to visualize the virus by electron microscopy in acutely ill volunteers, but that was also unsuccessful. So for quite a long time, all we could say was that we were simply dealing with a small filterable infectious agent. I realize now how persistent early investigators were in the face of so many failed experiments. This early work was published a Clinical Center Grand Rounds, “The Pathophysiology of Acute Infectious Non-Bacterial Gastroenteritis.”

This is the point at which Al Kapikian entered the project. He had been studying a technique called immune electron microscopy (IEM) with June Almeida [Dr. June D. Almeida] in England. He was studying respiratory viruses and looking for antibody-coated virus clumps using electron microscopy, the clumping was caused by the addition of immune sera to virus. When he came back from those studies with June Almeida, he applied the technique of IEM to the stool filtrates that we had. One particular filtrate was called 8FIIA, which meant it was the second passage through volunteers, and we had over 3 liters of the filtrate at the time, and it became a standard reagent. By mixing antibody from volunteers or from subjects prior to and after experimental infection, and using electron microscopy in a blinded fashion, we made an association with the particle that we saw an immune response, i.e., absent antibody on virus-like particles mixed with acute phase serum, in contrast to a heavy coating of antibody on particles mixed with convalescent serum. And so we finally had a particle 27 nanometers in size that we could associate with the disease itself.

At that point in norovirus studies, we had an agent to assess and something that could be characterized further, even though attempts to grow it or find an animal model still had not succeeded. We had only modest success by inoculating a series of chimpanzees. We collaborated with Peter Gerone [Dr. Peter J. Gerone] in Covington, Louisiana, at the Tulane Primate Center [Tulane National Primate Research Center]. I travelled there with our Norwalk inoculum (8FIIa stool filtrate) that we administered to chimpanzees. They did not get sick, but we found virus particles in their stools. The norovirus was therefore very species specific with regard to illness, but these chimpanzee stools provided us with a valuable source of particles as reagent. Up until that point, we only had virus reagent from human stool, and now we had virus from chimpanzee stools that we could use and do various tests to measure antibody to the norovirus.

We tried extensively to grow the virus and used a wide variety of cell cultures, including at one point, human fetal intestinal organ culture as described in the literature. This, too, was unsuccessful. The organ cultures would grow and support the growth of various intestinal viruses like poliovirus, but they would not support the growth of norovirus. And so we remained at a standstill with the norovirus work.



Rotavirus electron microscope image.

Our norovirus work was paused when another enteric virus emerged on the scene. I recall when I first came to the Chanock lab, coming out of pediatrics, I asked my new colleagues in the NIAID Laboratory of Infectious Diseases, “What about viral diarrhea in children, infants and young children?” They explained to me that the only identified causes of viral diarrhea in children were occasional outbreaks associated with echovirus or coxsackievirus, etc. But that did not seem to be an explanation for the outbreaks of diarrhea that were quite common among infants and young children and that had been described in the

literature. So we started using the same techniques that Al Kapikian had used for norovirus by applying them to diarrheal stools from children. And in fact, what emerged was a very different kind of virus. It was a 70-nanometer virus that was, again, very difficult but not impossible to grow. It was a virus that was associated with diarrheal disease in children by showing that children who had convalesced from it developed antibody that coated the virus particles that looked like a reovirus. At first, we called it a reovirus-like agent [reovirus meant "respiratory enteric orphan virus"]. That name stuck for a while until one of our scholarly colleagues, Tom Flewett [Dr. Thomas H. Flewett] in England, coined the name "rotavirus" because it looked like the spokes of a wheel (rota is Latin for wheel). And as often happens in science, there were discoveries that nearly simultaneously identified this new-detected rotavirus in Australia, England, and the USA. Rotavirus then was recognized quickly as being a much more lethal infection than other enteric viruses, especially for infants and young children.

With the techniques that we had to grow the virus, it eventually became a candidate for vaccine development. There are many different aspects to the effort to develop a rotavirus vaccine. One of the interesting aspects was that there were animal rotaviruses serologically related to the human rotavirus. That enabled us to work with these animal viruses in recombining or re-assorting their genomes and coming up with attenuated viruses that could be used as a potential vaccine. We looked at rotaviruses from cattle. There was a prominent researcher in the UK, Gerald Woode [Dr. Gerald N. Woode], a pioneer researcher in Nebraska by the name of Charles A. Mebus [Dr. Charles A. Mebus]. We all became collaborators. There was also a pig rotavirus, and we had collaborators in Ohio. Ed Bohl [Dr. Edward H. Bohl] was the principal investigator of that group. We were able to develop animal models using the gnotobiotic calf and pig. We had to study these under germ-free conditions because we did not want to be confused by prevalent natural infection.

It was an interesting time as we began to understand the virus. My own attempts to grow the virus initially involved a serial passage of this virus through gnotobiotic or germ-free piglets. That aided in the virus' adaptation to cell culture. The hypothesis was that if we passaged these viruses through animals or through cell culture multiple times, we might very well wind up with an attenuated or weakened virus that could be used as a vaccine. Our main technique was mixing (or reassorting) the genes so that a virus from one species, if it infected humans, would not cause as severe a disease. We also used the process of viral reassortment to create vaccine strains. You mentioned already that our work eventually led to a vaccine strain that was the first FDA-licensed rotavirus vaccine. I can't remember the exact year, but it was in the mid-eighties.

It was as true for the rotavirus vaccine as it is in many vaccine fields, that what is a good vaccine at one point is supplanted by a better vaccine, and a better vaccine still that follows after that. So our work represented the first generation of rotavirus vaccines. There was a problem with that particular vaccine that caused its termination: a small number of treatable cases of intussusception. Intussusception occurs when the intestine infected with this live virus vaccine, telescopes on itself, thus requiring treatment. That made this vaccine unacceptable and future vaccines followed. We now have vaccines that are safe and administered worldwide. The death of that little boy in Guatemala could have possibly been prevented with one of these vaccines. That, in summary, describes my involvement in the norovirus and rotavirus stories.

Harden: Wow.

Wyatt: It was a very exciting time in the laboratory. It was just rapid fire. Basically, within the course of a decade, we were able to study in varying degrees two brand new viruses. There is a book, Microbe Hunters, which I am sure you've read.

Harden: Yes. The Paul de Kruif book [Microbe Hunters (New York: Harcourt, Brace, & Co., 1926)

Wyatt: I felt like that the lab was being true to its mission of hunting out viruses. That had been the history of our lab chief, Bob Chanock. He discovered new viruses, such as the paramyxoviruses. We were doing team science at a very early stage. We had creative investigators, headed by Chanock himself, who had wonderful creative ideas that the rest of us could apply to the field. But creative ideas were not just limited to him. He established the direction for the lab that then enabled a talented team. Now remember, we noted that many of the members of the team were the so-called Yellow Berets, those of us who came to the NIH to do research in lieu of fighting the Vietnam-American war. Most physicians came, they fulfilled their Selective Service obligation, or military draft obligation as physicians, and then they left after two years or maybe three years. But there were some of us who stayed. When I signed in at NIH on July 4, 1971, as a Public Health Service Officer, there were many other people who signed in then too. I already mentioned that Michael Gottesman signed in that day and that John Gallin, who became Scientific Director of NIAID and later head of the NIH Clinical Center, was also a member of that class. There were other classes, and we just attended a symposium to honor Phil Leder [Dr. Philip Leder], who was also of that era. There were Commissioned Officers in Phil's NIH laboratory who came into the NIH through the USPHS Commissioned Corps who turned their careers into research careers. I have described here one narrow window into one specific era of virus discovery. But it was real and very exciting, and progress likely would have been slower without the Yellow Berets.

I have already mentioned Ralph Feigin at Washington University. After I had been at NIH for five years, he offered me a position at Washington University, and I turned him down because it was so exciting to be in the NIAID the Laboratory of Infectious Diseases at NIH. I didn't feel that I was at a stopping or changing point yet.

Having now been at the NIH for over 50 years now and putting the whole of my experience of in perspective, I think that we have a much finer focus on mentorship today than we had in those early days. There is no question that people like the late Bob Chanock and Al Kapikian had a lot of wisdom to offer. We neophytes were coming in as aspiring physician scientists but not knowing too much about what we were doing. We were often trained in virological technique by the Ph.D. scientists who were in the laboratory. But as to the mentoring aspect, I have no trouble at all recalling Bob Chanock's exuberant enthusiasm. He was so enthusiastic that he might come to the laboratory 10 times in one day wanting to see the latest data. He was literally always present in the laboratory. There is another kind of mentor in other labs who isn't present at all, and who doesn't help young scientists much. I came to appreciate the difference between what it is to be a good mentor and one who is not, and I remember some very specific things. I won't go into great detail, but one of the things that Bob Chanock taught me was the importance of credibility. He said, "If you lose your credibility, you've lost everything." Or another lesson: "Be ready to change directions [in research] at a moment's notice!"

Harden: Right.

Wyatt: And that is a point of mentorship. Al Kapikian was incredibly thorough in developing experiments and including a control for every point in the experiment. So he taught the concept of having the right controls in an experiment and how to blind or double-blind the study. These are things that still 50 years later pop into my mind, the idea of controlled studies, of the utmost importance of integrity and honesty. And so I think there is a lot to be said about the values taught by that generation.

I think now the focus on mentorship is to provide earlier much of the information that I learned over a decade and to try to bring people onboard sooner in their careers. We have training courses on mentoring, and it makes a difference. We have now an Office of Intramural Training and Education at the NIH that pays attention to the needs of NIH trainees. We have published a whole "Guide for the Conduct of Research." It is basically everything one needs to know to work effectively in an NIH laboratory. We did not have that in the 1970s and 1980s.

The 12-plus years that I spent in the laboratory led to a personal world view that caused me eventually to want to work in a broader, more administrative way to support science. What happened was that we had wonderful studies going on. We were enthusiastic; we had good ideas, good projects that were progressing in a productive way. As I look back on those times, some of the things that I was assigned to do in the lab were administrative jobs. For example, I was asked to be the chair of NIAID's IRB, the Institutional Review Board. John Seal, who was the NIAID Scientific Director at the time, asked me if I would do that for NIAID because the Institute at that time had one IRB that looked at not only intramural studies, but also extramural studies as well. And I said, "No, thank you. I don't think I would like to do that." And generally speaking, I had not learned a lesson that when the big boss asks you to do something, you say, "Yes, sir." But I had said, "No." So he appointed someone else, but the person he appointed resigned shortly afterwards. Dr. Seal was very convincing after this. He called me on the phone and said, "I want you to be the chair of the IRB, and you cannot say 'no.'" I said, "Yes, sir." I had learned my lesson, and it turned out to be a remarkable experience that I carried with me for the rest of my career. Finally, in the past decade or so there has been a clarifying focus at NIH on the need to create opportunities to help the next generation of principal investigators pursue career advancement rather than leave the leadership positions in the hands of existing leaders for decades. This is a sea-change that is still underway, and had the change occurred earlier, it might have influenced the direction of my independent scientific career, but I doubt it.

Harden: There are three different issues that grow out of your laboratory research that I want you to address before we change the focus of your work at NIH. First, just before you arrived in 1971, the Chanock Lab had published a paper detailing how fetal organ tissue could be used to culture the viruses that were under study. This became one of several sensitive issues that you've had to deal with during your career. The use of fetal organ tissue dates back to the 1930s, and the NIH had supported research using fetal organ tissue since the 1950s. But after the 1973 Roe v Wade decision, research using fetal organ tissue became entangled in federal abortion policy. And this situates the NIH as both a scientific institution and an institution of the federal government that has to deal with such issues. Would you talk

about how the NIH has dealt with the use of fetal organ tissue in the lab through the years from when you were first there? You can come up as close to the present as you want.
Wyatt: Yes, that, of course, is a highly sensitive issue. When I arrived in the lab, we were using human fetal intestinal organ culture as I mentioned earlier. We were able to obtain samples of fetal intestine through local hospitals. These tissue samples were from very early fetuses. They were the product of hysterectomies/hysterotomies, miscarriages, and medical abortions, but we had nothing to do with the procedure, decisions to abort, or the process itself. That was something that was in the hands of the patient's physician. NIH had an agreement with at least two local hospitals where we could stand by as a physician was performing a hysterotomy/hysterectomy, the product being remnants of the aborted fetus. We would collect the tissue under sterile conditions and take it back to the laboratory. I was not a part of creating those agreements. There was a nurse who generally collected the material. She would go to the hospital where the procedure was done and wait in an adjacent room to collect the material. Even then, it bothered me, even though it was a perfectly legal process. Let me just say that I was taught—and this was a part of my mentoring—that everything we were doing was fully consistent with state and local laws. It is important to say that this was something that was possible, it was legal, and it was done. But if you fast-forward 50 years, and look back, some people would say, "What were you doing?" Perhaps it is akin to questions about studying noroviruses in prisoner human subjects or in an endangered species. The scientific justification for the use of human fetal intestinal organ culture was that intestinal cells are very difficult to grow and maintain in an undifferentiated state in tissue culture. I tried it many times. I would take intestinal tissue, trypsinize it as we used to do, put it into cell cultures; all that would grow out would be fibroblasts. They weren't in any way differentiated cells. The idea arose to do organ culture. These were tiny bits of the fetal intestine that were grown in small Petri dishes. They would actually round up and you could see the villi of the intestine. It made perfect sense that if we could keep those cells growing in a more differentiated state so that they might very well support the growth of norovirus, or later on of rotavirus.

Norovirus was the subject that we were working on initially with the fetal intestinal organ cultures, but in point of fact, norovirus did not grow in those cultures. We did serial passages of fluids from the cultures, and in the end we took culture fluids and administered them to volunteers; they did not get sick, indicating that the virus had not replicated in the organ cultures. Oh, we could grow other viruses in the organ culture. We have some nice electron micrographs of those viruses, all the way from adeno-associated virus to herpes virus to poliovirus. We could see their crystalline structures. It worked beautifully on viruses that adapted to grow in this system, but it did not work for norovirus.

By the time rotavirus came along, the human fetal intestinal organ culture system was no longer in use in the laboratory. And you mentioned the 1973 Roe v Wade decision, but there was another case related to abortions that gave us pause. It was a case in Boston against an African American obstetrician/gynecologist who performed an elective abortion on a 17-year-old unmarried girl who was six months pregnant. In 1975, an all-white jury, with 10 Roman Catholics on it, convicted the physician of manslaughter. An appeals court overturned the conviction. The controversy has stayed with me, however, and when I first read about it, it became clear that what we were doing was not going to be possible in the future. That ended this particular line of investigation. It had not been successful, and yet it had been both logical scientifically and legal in the sense that we were abiding by state and local laws. I am still haunted by these studies from time to time.

We used primary human embryonic kidney in our routine tissue culture work, which was a staple in those days, but that's not used anymore, either. We used two cell lines on a routine basis, WI-38 and MRC-5. Both of these were derived initially from fetal lung, as I recall, and their origin has also been questioned in the past. But they are such standard cell strains that they have been accepted and are in continued in use. They are not primary cells, rather they're strains of passaged cells in cell culture.
Harden: They are cells that have been maintained rather than acquiring new tissue?

Wyatt: Yes, in contrast, fresh primary human embryonic kidney is obtained by the process of dicing and mincing human embryonic kidneys and putting them directly in cell culture. They are sensitive, useful cells.

Harden: Let's move along to another issue. Some of the studies in the early 1970s were conducted in prisoners from the Maryland House of Correction in Jessup and from Lorton in Virginia. Some of the prisoners were brought to the Clinical Center for the studies. And according to your June 1974 paper in the Journal of Infectious Diseases, "All studies were carried out in isolation and under close medical supervision. Studies were healthy volunteers aged 18 to 50 years to whom the nature, purpose and potential risks of the studies were carefully explained." In 1973, a lawsuit filed by the ACLU [American Civil Liberties Union] against the University of Maryland, along with Congressional hearings, persuaded NIH to stop using prisoners. This came, of course, after the 1972 revelations about the Tuskegee syphilis study, which led to the 1974 National Research Act. Would you talk about how NIH worked with prisoners, and how this then changed after 1973?

Wyatt: I can only talk about how we in the Laboratory of Infectious Diseases worked with prisoners. Our collaborators, actually, were at the University of Maryland in the lab that Ted Woodward [Dr. Theodore E. Woodward] had created. At the time, Herbert DuPont [Dr. Herbert L. DuPont], or "Bert" DuPont as he was known, was the head of that particular effort. They also had infectious diseases studies going with the Jessup facility in Jessup, Maryland. And so this collaboration had been established when I arrived, and it was something that was assigned to me to be a part of, so I was told, "You'll be going to prison" to participate in these studies.

The prisoner "volunteers," and I think we have to put volunteers in quotes, were prisoners and at the same time research subjects. They were consented, and they knew what was going on. But the question really is whether prisoners can actually give informed consent. I saw all of this go on first-hand, but I never was in a position to testify in the ACLU case. I prepared materials about all of the outbreaks that we had studied and turned them over to the lab chief. I do not know how he used them, but they were a part of the file.

I think it is important to make several points. In the end, we had a study that had been reviewed by our Institutional Review Board and also that we were including an informed consent process. The legal decision on that particular case was in favor of the NIH and the University of Maryland. Well, actually it was NIH's parent, the Department of Health, Education, and Welfare, now Health and Human Services.

This situation caused me some soul-searching and discomfort. When the ACLU filed suit on behalf of some of the prisoners that had been a part of our studies and whom I had met personally, it called into question, for me, whether these studies were ethically correct to do. I have thought through this many times, but basically I haven't talked about it. This is really one of the first times. There was a time just a few years ago when Laura Stark [Dr. Laura Stark] was working on a history of informed consent as a Stetten Fellow in the Office of NIH History and Stetten Museum. I discussed it with her, to a certain extent. And soul searching is probably the best way to put it, because I ultimately had to say, "Yes, I will participate, or no, I will not participate" in such studies. And the fact that there were review processes in place, the fact that there was an informed consent process all made sense, but the concern, e.g., autonomy, perceived perquisites, etc., still remained. These subjects did not have the same civil liberties as citizens on the outside. They were incarcerated. Their freedoms had been restricted. And I have often thought about this. For example, when they came into our studies, we would pay them, but we would pay them at the same rate as if they were working in the prison factory doing whatever, making license plates, for example. That was one of the prison industries. We did not pay them excessively, but they had a hospital ward, they had a clean bed, they had television, they had cigarettes. And so it was not exactly the same. I have to tell you that to this day, these experiments are something that has bothered me. It really called into question, “Was I really doing the right thing?” I realized that this was something that I had to contend with. It was almost a spiritual experience, to be honest.

Harden: These issues go way back. As a historian who writes about NIH history, I have been asked about this a lot. We can talk about Dr. Joseph Goldberger’s using prisoners in his 1914 pellagra research, and there are other examples. It certainly was not new when you were involved. In 1976, there was a report that concluded that because they do not have full civil liberties, prisoners were not appropriate for research subjects.

Wyatt: There are conditions where one can study prisoners today, but there has to be a direct benefit to the prisoner. NIH and the Office of Human Research Protections (OHRP) at the Department of Health and Human Services are very familiar with that. There are special conditions that can be applied. It is not off the table, but it is a very different thing. In the past, there did not have to be any value to the prisoner. The value was a societal benefit if we could understand diarrheal disease better, but there was no immediate, direct benefit to the prisoner.

And so the question that kept running through my mind was whether these prisoners were in any way induced to participate? Was there undue inducement to participate in the studies? And believe me, that has gone through my mind over and over again. While we believed we were justified in doing them, it is the same kind of thing that we were talking about with the use of fetal tissue. If you fast-forward 50 years and look back, you say, "Oh, society doesn't look at it the same way." And there I was in the middle of it.

We can't do a retrospective analysis without considering what was going on at the time, what the standards were at the time. I suppose one bottom line is that the prisoners we studied were not subjected to a lethal disease. It was a short-term disease, as I've already characterized—24 to 48 hours and nobody died. It was not like infecting them with a disease that could be potentially fatal. And yet one of my colleagues in 1971 pointed out to me that when you look at the outcomes of the Nuremberg

Trials, the use of prisoners, the study of prisoners was not condoned. That troubled me when he told me that. I had not really been aware of that in 1971.

Harden: I want to move on to one more long-term social issue with which you have been involved. In a 1978 paper, you and your colleagues were able to demonstrate infection with Norwalk virus in chimpanzees, even though they did not become sick with classic Norwalk symptoms. This work, and perhaps earlier research with animals, led you to agree to serve first from 1982 to 1984 as chair of the NIAID Animal Care Committee. And so when protests by animal rights advocates led to a requirement in the 1985 Health Research Extension Act for guidelines for the care and use of animals in research, NIH Director Jim Wyngaarden [Dr. James B. Wyngaarden] appointed you as chair of the NIH Oversight Committee on Animal Care and Use that led to the 1993 accreditation by the Association for Assessment and Accreditation of Laboratory Animal Care, AAALAC. Between August 1992 and April 1993, you served as Acting Director of the newly created NIH Office of Animal Care and Use. And from 1993 till 2022, you led the federated NIH Animal Care and Use program as chair of the NIH Animal Research Advisory Committee. In 2000, you became chair of the Interagency Animal Model Committee. All this service led not only to NIH and PHS awards, but in my mind makes you maybe the person who has more experience and more knowledge about animals in research at NIH than anybody else whom I have interviewed. Would you tell me about all this starting with chimp studies in the 1970s and coming forward?

Wyatt: Well, it probably started back in high school for me. My first exposure to anything to do with animals was a high school science fair project to compare the histopathology of livers among different species. That involved having the different tissues to work with, which we acquired. It was not a specific research study but more of a histopathology of study with samples of tissues that had been collected. But it did point out the fact that we were using animal tissues and that we had a responsibility for these animals. I mention this in passing because you could always trace something like that back to the very beginning. I remember thinking to myself when I was six years old, “How can my uncle cut the tails off of those baby cocker spaniel puppies without putting them to sleep?” This was a long time ago, so there is a long history.

The work that you described that moved the NIH Intramural Program towards AAALAC accreditation is one of the special projects that I remember as one of my definite contributions. At the NIH, we knew that being accredited by the AAALAC was something that was valuable. But it was only when the director of the NIH [Dr. James B. Wyngaarden] said, "Do it," that we did it. And so that's one point to be made: top leadership in important areas is so essential. I always felt that my work in the laboratory—where I had studied germ-free piglets, germ-free calves, and all kinds of other animal species, including the chimpanzees—had heightened my awareness not only of the need to do these studies, but also of the importance of caring for the animals properly. When I moved over to Building 1 to the Office of Intramural Research in 1984, it very naturally fell to me as something that was of concern.

We did not begin to have the oversight of animal care and use in those days that we have today, but subsequently, we've developed it. I think we went in the right direction. We are currently fully accredited, and when we do studies on animals at the NIH, we know how they're done, and we know that we follow all of the principles and the rules and care for them properly. But the point I was trying to make was that you trace back an interest that started as long ago as childhood, and it comes all the way

up to the current time decades later. I think there is a benefit to having a historical perspective, in which one can see the progress one makes and be able to advance science while at the same time caring properly and abiding by all of those provisions. And that's what this is about. It is a balancing act.

In my early research, we studied chimpanzees. Later on, the use of chimpanzees was basically stopped. While there were useful studies done in the past, it was deemed they are no longer appropriate in chimpanzees today. We may say, "Well, but it was appropriate when I did it." But that is looking backwards and applying a standard today to something that was done maybe 40 years ago. At the same time, everything that we did long ago was done under appropriate review and approval by our animal care use committees and those groups that were reviewing the work.

Today, we're in a very good position at the NIH. We have a fully-staffed Office of Animal Care and Use. We have people who are quite cognizant of all polices and rules. We have animal program directors in every Institute and Center, and we have a system of risk management that asks the right questions about what we're doing and how we're doing it and to make sure that we're following all of the rules. Nevertheless, there are members of the public and members of Congress who do not favor animal research. We have our work cut out for us in attempting to explain why it is important under what conditions and how we move forward.

Harden: How does the NIH respond to the larger ethical question that Peter Singer [Peter A. D. Singer] and others raise when they equate animals and humans? And how you bring what NIH is working towards, whether it is a vaccine or something else to benefit human health without using animals. Would you take on that ethical issue for a moment?

Wyatt: Well, I think basically our whole system of reviewing animals and research and of reviewing human subjects and research is designed to answer the questions not only in the scientific community, but among the public. We have public members who serve on our review groups, we have outside advisors who come in and advise us. Our job is to try to listen as carefully as possible to the whole community and come to the right kinds of decisions. Decisions that were made 50 years ago may differ from decisions that are made today. I do not think there is any question about that but staying contemporary in listening to the people's thoughts and opinions, is also an issue that Congress has grappled with in representing its constituents. This is a dynamic area and one that requires our very careful attention. I like the fact that impartial, outside advisory groups come in is a sign of our openness. We present how animals are used and how science is reviewed in an open fashion. It is the same way that we call on, for example, the National Academy of Sciences to review selected topics. We have outside advisors, not inside advisors, who come in and help us understand whether we are we on track. Is this the way it is supposed to be going? There was a recent study by the National Academies of Science that had to do with animals in research and where we go from here. I just think that we have to be open and listen. We enter what is hopefully a civil dialogue with all parties to be able to move forward. That's my take on it.

Harden: Let's move on to something more personal. On July 2, 1978, you married Linda Stooksberry, who was a postdoctoral fellow and a single parent with two children, William [William P. Richardson, Jr.]

and Katherine [Katherine E. Richardson Carson]. Would you tell me a bit about Linda? How did you meet, and how did the two of you view your careers as you began married life?

Wyatt: Others have asked that same question, and depending on the audience, I give either an expanded version or a simple version. I will give you a straightforward version in a condensed way. First of all, it was interesting because she was working on respiratory syncytial virus at the time. She was on the third floor of Building 7, and I was working on infectious diarrheas on the ground floor.

Harden: She was already at NIH at that time?

Wyatt: Yes.

Harden:And you were both in the Chanock laboratory?

Wyatt: Yes, we were both in Chanock lab. At one point, I needed some tutoring on how one grows viruses in cell culture. This was after we were able to cultivate the rotavirus. Linda taught me how to do a viral plaque assay, and that put us in the lab together. Some of our colleagues were watching us from a distance and wondering if this relationship would go anywhere, and that's a major part of the story. She had done her Ph.D. in virology at Baylor College of Medicine and was quite talented. She had a sharp mind, and we were in the same general field of infectious diseases and enjoyed each other. But the other part of the story might not get included if I weren't being fully open. One day at church, I looked across the fellowship hall and saw her. I said, "Oh, I did not know you went here." At that point, we realized that we had common interests not only in science but also in matters of faith. In scientific terms, you could call it “two-hit kinetics,” so there was more than one reason that we came together. That's the other part of the story.

Harden: Thank you. On February 21, 1980, you welcomed your first child, your daughter, Grace Wyatt, now Grace Snitgen. And three years later on July 29, 1983, your son, Gregory Wyatt was born. Would you talk a bit about how you and Linda managed what is today called work-life balance?

Wyatt: I can, and it is actually a part of our story together that we're rather proud of. She was very talented in the laboratory, and yet we recognized, not only did we have two children, but she had two children from her earlier marriage, so we had a total of four children at home. She recognized that in the work-life balance, she needed to take some time off from her work to devote that time to raising the family. So she took a 10-year sabbatical. When the youngest was in kindergarten, she ended her sabbatical and went back to the lab because she had always really enjoyed lab work very much. We were able to get childcare and pursue two careers independently until she retired four years ago. I am on my way to retirement now.

Harden: She went back to Bernie Moss's [Dr. Bernard Moss] NIAID Laboratory of Viral Diseases, correct?

Wyatt: Yes, she had been in the Chanock Lab, and then she went back to Bernie Moss's lab. But there is a story behind that. She was looking for a lab position. She could have gone into an administrative job, but that did not describe what she wanted to do, since she really enjoyed doing science. She was one

who had loads of fun with her chemistry set when she was growing up, and she just liked to be—and still likes to be—in the lab, thinking about scientific problems. There was a woman, an Israeli scientist working in the Moss lab by the name of Niza Frenkel [Dr. Niza Frenkel]. Linda applied to work with Niza. Niza looked at her CV (curriculum vitae) and said, "You have a 10-year gap in your career. I decided that I would simply forget it. I had to deal with it, and I just decided to overlook it." And so Linda went back full time into the laboratory, given a second chance by a distinguished woman in science. That's a part of our story, and she was very grateful for it.

But she was also grateful for the 10 years that she had in terms of child- rearing. And we do tell this story frequently because it is the subject of a lot of concern, and I know there are other ways of doing it. It certainly requires effort on the part of a husband to work out the work-life balance. It requires giving in certain areas. I think it was Maxine Singer [Dr. Maxine F. Singer]— Ed Rall [Dr. J. Edward Rall] told me the story—that Maxine went 90% when she had children because she wanted to be able to take them to the doctor and not have to worry about asking for time off. I did not hear this directly from Maxine, but that's what I heard, and I liked it. Martha Vaughn [Dr. Martha Vaughan] and Jack Orloff [Dr. Jack Orloff], who were one of the NIH couples in science, also worked it out. Martha simply said that everybody had to pitch in and do their part.

Harden: While you were researching and publishing about rotaviruses in 1981, a new disease now called HIV/AIDS appeared. In your 1990 interview about AIDS, you noted that the Chanock lab might have been a good candidate to investigate AIDS early on, but that Dr. Chanock decided against this, wanting you to stay with the development of a possible rotavirus vaccine. You did some initial studies in Robert Purcell's [Dr. Robert H. Purcell] second floor lab. Now that we are 33 years on from that interview I did with you back in 1990, do you want to comment on anything else about NIAID Intramural Research on AIDS in the early 1980s before your move into administration?

Wyatt: Let me go back to the Chanock Lab. I was always surprised that for a laboratory which had devoted its time and career and life to discovering new agents, new viruses, that basically Bob Chanock was not interested in trying to grow HIV. Now, we did not know it was HIV at the time. It was the same kind of thing that we had with our filterable viruses in stool filtrates. But we knew there was something there. I told him I wanted to work with Tom Folks [Dr. Thomas M. Folks], who was over in another part of NIAID, that we wanted to try to grow whatever this agent was in monocyte cultures—and of course we chose the wrong cell, but we planned to grow it in monocyte cultures and then detect the growth of the virus using immunofluorescence. Dr. Chanock told me to "go up to a corner lab on the second floor and don't contaminate anything." The AIDS agent was so unknown at the time that there was a risk involved because we did not have the sophisticated level of biocontainment that we have today. In fact, as you know, Building 7 was known as Memorial Laboratory in honor of the investigators who died in an earlier era. That's also why the street in the center of campus is called Memorial Drive. There were risks involved, and I think that may have been a part of it, but trying to identify the then-new AIDS agent simply was not a part of the Chanock lab's research portfolio. I often tell people, well, if I had grown it, then my career would have been different.

Harden: In 1975, Richard Krause [Dr. Richard M. Krause] became NIAID Director, and in 1977, Ken Sell [Dr. Kenneth W. Sell] became Scientific Director. In 1983, after AIDS was recognized, you served for a year

as AIDS Research Coordinator for Dr. Sell. Now, I want to note that within a week after Dr. Sell left NIH in 1985, his correspondence files were sadly discarded. I know this because I called one week later, and they were gone. He died in 1996, so any written record of his efforts against AIDS are unavailable. Can you comment on Dr. Sell's initiatives and the research initiatives you mentioned in your 1990 interview? What was going on administratively?

Wyatt: I remember sitting in Ken Sell's office, talking about progress on AIDS. One day Tony Fauci was sitting in the corner of the office. I think Jim Hill [Dr. James C. Hill] was there in his role an assistant to Ken Sell. Later he became Deputy Director of NIAID under Tony Fauci. I want to take the opportunity parenthetically to say that Jim Hill was the kind of scientist-support person that I emulated, just as I emulated Phil Chen [Dr. Philip S. Chen], who became my supervisor when I moved over to Building 1 in what is now the NIH Office of Intramural Research. Both Jim and Phil provided critical support to dynamic leaders. Ultimately, Jim had a very good and close working relationship with Tony Fauci. I had a lot of respect for Jim, and I knew Jim before he went to work for Tony because we were part of the same team. In fact, in those days it was Lois Saltzman [Dr. Lois A. Saltzman], Jim Hill, Ken Sell, and I who made up a little group working on AIDS. I mention Jim because like him, I never wanted to be the head of something. I wanted to be an enabler, a bridge-builder, somebody who would enable studies to take place. I observed that as Scientific Director, Ken Sell was trying very hard to cast a wide net to determine the causative agent of AIDS. That's why he suggested the idea of the symposium we conducted—to have all kinds of people come from different disciplines and talk about what might be causing AIDS. It was such a tremendous public health problem by that time, and he turned over every rock he could.

One particular thing I remember is that he had heard of a disease caused by Ehrlichia canis. I can't remember too much about it, but there was a researcher in Chicago, Charles Kallick [Dr. Charles A. Kallick], who had worked on Ehrlichia, and Ken thought Ehrlichia might be a candidate. A small group of us went out to Chicago and met with him. We quickly determined that AIDS was probably not caused by Ehrlichia canis, or anything related to it.

But it looked like that the lymphocytes were the target cell. I know you've talked to Tony many times. I know that he was also honest, sincere, and committed to do what he could do to determine the cause, treatment, and prevention of AIDS.

Harden: When AIDS appeared, NIAID was not a wealthy institute. Dr. Krause told me how difficult it was at that time to convince Congress that infectious diseases were still a threat to the American public. Chronic disease was the great concern at that time. Would you comment, however briefly, on this situation and how AIDS changed both NIAID and NIH in general?

Wyatt: I think Dr. Krause was correct that it was difficult to get funding for infectious diseases research in 1981. I remember as young Commissioned Officers in the early 1970s, we used to wish for the dollars that it cost just to procure one fighter jet going to Vietnam. What we could do if we had the dollars from one fighter jet! For example, old Building 7 was a poster child for how poor our facilities were. It was crowded, and it was aging. There was no question that there was a scarcity of funds, but there were very effective advocates. And I think Tony himself is credited generating the kind of support and urgency associated with combating a disease like AIDS.

I also remember going to an appropriations hearing about a decade later at which Harold Varmus was testifying. There was some Congressional concern about the large amount of money that was going into HIV research. Harold's point, I thought, was logical and scientifically elegant. He said, "The scientific opportunity drives the work, and that drives the funding. It is all about what opportunity is posed by this issue that is in front of us." And HIV/AIDS research was providing so many fronts for research that went way beyond HIV itself. At least that's the way I remember it.

Harden: Once you became special assistant to Dr. Sell, you seem to have made a decision to move from the laboratory into research administration full time. For an overview, I'll note that in 1984 you then moved to Building 1 into the NIH Office of Intramural Research [OIR] as Special Assistant for Intramural Affairs. You held that position for five years and then went up the ladder to become eventually, in 2007, Deputy Director of OIR. You served under five duly appointed NIH Directors, four Deputy Directors for Intramural Research [DDIR], and five Surgeons General of the Public Health Service. What enticed you to make this shift into administration?

Wyatt: This is an interesting story, and I guess it is in keeping with the nature of an oral history to tell it. Ken Sell, the Scientific Director of NIAID, was a neighbor of ours. We lived a block apart. In 1982 or so and I can't remember the actual year, there was a gas crisis where you had to stand in line for blocks and blocks to get gasoline. Dr. Sell and I decided that since our schedules were compatible, the two of us could carpool to NIH. I actually carpooled for the better part of the year with a Scientific Director.

Riding back and forth to the NIH, taking turns driving, we talked about everything. And as I talked, he got the idea that I had an interest in more than the science that was going on in the lab. I have already mentioned that I was chair of the NIAID Animal Care and Use Committee. I was also chair of the NIAID IRB, and I was writing clinical protocols. I was doing a lot of administrative kinds of things, and they were of interest to me. I also recognized that the administrative structure in the Laboratory of Infectious Diseases was not a structure conducive to career growth. The leadership was not changing, the lab chief's position was stable, the section chiefs were stable, and there was no sign that they were going to change anytime soon. If I wanted to explore any kind of career growth, I needed to look elsewhere. I had actually tried. I wanted to go on a sabbatical and work with an immunologist who worked in Australia—Dr. Peter Doherty. I presented the idea to Bob Chanock, and he said he thought a sabbatical would be fine, but if I wanted to go on sabbatical, I should go to Boston and work with Bernie Fields [Dr. Bernard N. Fields], who was an expert in "real virology." I said, "No." I did not want to do that. I really wanted to work on immunology, especially local immunity. So I never went on a sabbatical, and it is perhaps an indicator behind why my career then moved and developed in new directions. Everything was going splendidly in the Laboratory of Infectious Diseases, but it was quite clear that the leadership was static and not subject to change anytime soon. This was not just my observation, but there were others who had similar impressions of the lab leadership.

So in 1984 I moved to Building 1 to join the NIH Office of Intramural Research. It was not called that back then, by the way. It was called the Office of Intramural Affairs, which we changed to the Office of Intramural Research. John Eberhart [Dr. John C. Eberhart] was a senior advisor in that office working closely with Ed Rall [Dr. Joseph E. Rall], who was the Deputy Director for Intramural Research, and with

Phil Chen. There were also at that time three outstanding administrative assistants—executive assistants as we call them today—working in the office (Janet Smith, Catherine James, and Anahid Ayrandjian). And that was it, an office of six people. The office now has over 150 people, but our little office had only six—seven after I joined.

Office of Intramural Research staff circa 1987. L-R: John Eberhart, Catherine James, Richard Wyatt, Philip Chen, Anahid Ayrandjian, Kathy Conn.

A position had opened up in the OIR in 1984, when John Eberhart became ill. Phil Chen announced one day to the Scientific Directors that because of John's illness, they needed some help in OIR. And because I had been working with Ken Sell on the AIDS area and Phil knew that I had some organizational abilities, he suggested that perhaps I would be a candidate to work in the office. In those days, until 2007, I was in the Commissioned Corps (with the rank of O-5), and one of the features of the Commissioned Corps is that one could easily move people around. Phil Chen called me and said, "Would you be interested in working in our office?" I went over and interviewed with him, and I quickly said, "Yes." And that was the end of the story. I moved to Building 1 a month later (and remained for 40 years). But originally, the shift from the laboratory bench to administration had to do with carpooling with Scientific Director Ken Sell, when he learned about my interests.

There is one other thing, and I say this because I think it is important for people who are early to mid-career: There is help available at NIH if one asks the right people. One of my interests had always been international health, global medicine. I really wanted to do something in that arena, and nothing was immediately apparent to me. And so I went to John Seal, who was then the NIAID Scientific Director. I told him about my interests. And before long I was on my way to Egypt to study diarrheal disease, working with Morris Jones [Dr. Morris Jones], head of the Special Foreign Currency Program in the Fogarty International Center, who was administering these special funds called PL 480 funds. These were

excess funds owed to the United States—for example, instead of Egypt paying its debt to the United States, the money could be spent in Egypt on medical research projects of mutual interest.

I made more than one trip to Egypt. We had collaborations in diarrheal disease and developed a center at a remote village site to collect samples. And then we had a collaborator at the Central Health Labs. But the point is that because I went to John Seal and explained what I was interested in, he did something about it. He listened to what I had to say, and it was a wonderful collaboration, a good project. I think one has to take some initiative in finding these kinds of opportunities. I encourage early and mid-career scientists to keep talking to people. You will recall that I went to the neuroanatomy professor at Washington University to get public health advice, simply because she knew my name.

You have said several things that have reminded me of the importance of leadership in our NIH. My sphere of operations has been NIAID. When I came, Dorland Davis was Director, then Dick Krause became Director, followed by Tony Fauci. I think we all have seen the benefit of that kind of strong leadership. In the Office of Intramural Research, there have been four duly-appointed DDIRs that I've worked with, beginning with Ed Rall. Ed was a consummate scientist. He was insightful. Phil Chen and I were his two professional staff members. We would bring issues to him, and he would help us, although his Science magazine was never far away. He was replaced by Lance Liotta [Dr. Lance A. Liotta] when Bernadine Healy [Dr. Bernadine P. Healy] became Director of NIH. Lance was DDIR for about 18 months. And then Michael Gottesman came in as DDIR and was there for 29 years. And now Nina Schor [Dr. Nina F. Schor] is DDIR and has been there for about 16 months. I have seen different leadership styles. I have seen people working in a very committed way for the Intramural Research Program, which, in its early years, was really the NIH. If we go back to the early history of the NIH when it began as an intramural laboratory in 1887. I am talking about when NIH was Kinyon's [Dr. Joseph J. Kinyoun] lab on Staten Island. And then NIH came to Washington in 1891, and in 1904 it was given the laboratory buildings at 25th and E Streets, N.W. And between 1938 and 1941, it moved to the Bethesda campus. The extramural program was only authorized via the 1944 Public Health Service Act, which transformed NIH into a proportionately smaller intramural program and a vast grants-giving extramural program that supports research going on in the universities. Of course this is wonderful, but there is still a role for the intramural research program. These kinds of discussions about how much support should be given to each part of NIH go on today. The leaders of the Office of Intramural Research have been major supporters of intramural laboratory research. I was reminded of some of this when I listened to the tributes to Phil Leder, who had been at the NIH and was such a dynamic mentor to so many scientists. To hear them recall their years in the Leader lab, talking about his mentorship, talking about the way he guided them in their careers and all, it was really heartwarming. I was inspired to hear that. It is what we want to continue doing even as the challenges change.

I wanted to read into the record these people with whom I worked like the Deputy Directors for Intramural Research. And of course, there were many other talented people along the way whom I met and worked with. Once you are in OIR, you are exposed to the world, basically. Everybody comes through Building 1. It was a bit like being in the Chanock lab, where the world leaders of virology would come through and we would get to interact with them. And the same is true with leaders in many fields who come through the larger NIH.

Harden: The NIH Centennial Commemoration, which was held from October 1986 to October 1987, was in the beginning of the planning stage by the end of 1984 when you arrived in Building 1. I know that Hans Stetten [Dr. DeWitt Stetten, Jr.], who had preceded Ed Rall as Director of the Office of Intramural Research, had pressed senior staff in the NIH Office of the Director [OD] to create a Museum of Medical Research as a part of the commemoration. I know that Hans wanted this because I was talking to him at that time—he and I both were finishing books about NIH history. I do not know what else was in the works about the Centennial, so what can you tell me about how the Centennial observance was planned and executed?

Wyatt: It required a huge amount of work. I was always sad that Huly Bray [Huly E. Bray], who was working in Building 1 on the third floor and was very much a part of the Centennial planning, died in his Building 1 office on October 31, 1984, before the Centennial observance came about. There were a couple of really important events associated with the Centennial. One was a major banquet that was held down at the National Building Museum, which was a key effort. I was able to go to that. But the one that stands out to me is the one that we organized. The Centennial planners created a special funding mechanism to do this. Jay Moskowitz [Dr. Jay Moskowitz] was involved with it when he was also in Building 1. This was an event to bring a high school science student and a high school science teacher to the NIH from each state in the Union. We provided different opportunities for them. They got to go downtown and see how Congress works. They came to the NIH and there was a luncheon that took place over in Building 10. These students heard from Jim Watson [Dr. James D. Watson], discoverer with Francis Crick [Dr. Francis Crick] of the structure of DNA. He was one of the speakers at this luncheon. I can't help but think that it was inspirational for the students to be able to meet somebody like that who was such a well-known figure in science. And the other speaker was Albert Sabin [Dr. Albert B. Sabin], who, at that time was well-known for the oral polio vaccine work. He was also the mentor of my own lab chief, Bob Chanock. Bob trained with Albert Sabin in Ohio. After Bob came to the NIH, Albert Sabin was a frequent visitor to the laboratory. So it was a special treat for me to hear him speak and to be reminded how Dr. Sabin would come into the lab and refer to Bob Chanock as his “scientific son.” And then he'd look around and he said, "So you all are scientific grandsons." Now, I say that because we were entirely male at that point. But today, he would say, "You are my scientific grandchildren," because it is much more diverse today than it was then. But I have always thought, “Wow, that's some grandfather to have in the infectious diseases field.” I associate that memory with the NIH Centennial event that took place. It was a notable event as far as I recall.

Harden: Hans Stetten managed to get the Museum created as a part of the Centennial observance. I know because I was the person who was tapped to get it up and running! You have served on the Advisory Committee for what is now called the Office of NIH History and Stetten Museum. Would you comment on your contribution to helping it grow and how you see the future of history at NIH?

Wyatt: We are always looking for advocates for the Museum, and I think that the Institutes and Centers themselves are advocates for the Museum. In fact, the exhibits one currently sees, which are part of this dispersed Stetten Museum are really backed by them. There is the Harvey Alter exhibit, which just recently opened. The Marshall Nirenberg exhibit that is also there in the Clinical Center. The Fauci exhibit will open sometime this coming year. One of the themes that I work on is to develop partnerships with the Institutes and to create a visibility for the importance of recognizing the history. Now, the history of

the NIH specifically and biomedical research more generally, suffered from an unfortunate thing that happened maybe ten years ago. An advisory committee to the Deputy Director for Intramural Research looked at the history office and recognized that in spite of budget constraints, there needed to be some growth and development of the Office of NIH History and Museum. But then sequestration hit. And so I think much of our effort with the museum and also with the history office itself has been recently to preserve them. We are finally at a point where we can restore the budget and the personnel to what they were before. That leaves open the question of what is the best form for a museum? We have lived for decades now with the idea of the Stetten Museum as something that has exhibits all around the campus.

Harden: A "museum without walls" is what Hans called it.

Wyatt: Yes. And that's fine, but it is not as fine when you have visitors who want to come to the NIH and see something that's more concentrated. So my model, and one that I have talked about with others is the Goddard Space Center model where they have a facility outside the fence where people can come without going through security. And it is more than a museum with static exhibits. It includes demonstrations; it is interactive. This would be my dream for a museum at the NIH. I will tell you that it was not just Hans Stetten, but C. Everett Koop [Surgeon General C. Everett Koop], when he was Surgeon General, who was also an advocate of that kind of museum, a freestanding museum. It would require money, of course. And the idea of finding that kind of money within the government's budget is probably unrealistic because we have scientific, research-based priorities, that define what we are--an agency that supports and conducts biomedical research. But I still think that there must be a way to do it, perhaps through a competitive mechanism that would attract outside donors, possibly with the help of the Foundation for the NIH. Perhaps there are donors who would be willing to support a facility adjacent to the NIH where visitors could see what NIH does without having to go through security. It might even be built around an educational theme of restoring trust in science. The museum without walls was a wonderful idea before the terrorist attacks of September 11, 2001, after which the rigid security protocols were instituted and the fence built around the NIH campus.

Harden: There were two other jobs that you held from 1984 essentially to the present. You were management representative for the NIH Radiation Safety Committee, and you were the NIH liaison to the Foundation for Advanced Education in the Sciences (FAES). And I'd like you to talk about what those tasks involved and anything else you'd like to say about them.

Wyatt: Well, regarding the radiation safety job, it is important to recognize that for a very long time, we have been under the rigorous review and control of the Nuclear Regulatory Commission. The rules surrounding the use of radionuclides are clear, and we are intent on abiding by all of them. At the same time, we recognize that the use of radionuclides has been essential in the development of biomedical research efforts that we have undertaken at the NIH. When I first entered this area, radionuclides were extremely useful in the laboratory. There were a lot of studies using radioactive iodine and phosphorous, and of course tritium. But what has happened over the course of my observation is that the use of radionuclides in the labs has in fact decreased. That is good because there are known risks associated with them. On the other hand, the use of radionuclides in clinical research in patient studies has

increased. So there has been a changing balance in the way we use radionuclides. We have been fortunate at the NIH to have a strong cyclotron program for roughly forty years.

A main point about the Nuclear Regulatory Commission is that they do unannounced site visits. They come in and examine all of our records and visit our facilities. We have generally, certainly in recent decades, done very well when they come. It is important for them to understand that leadership is behind the efforts of our radiation safety program. So I routinely, in my capacity with the Radiation Safety Committee, went to the “entrance briefings” and “out briefings” when the Nuclear Regulatory Commission came for inspections. A principle is that when we invite in or when we are subject to these kinds of inspections, it is important for leadership to be able to stand up and say, “We believe in what we're doing and thank you for your expert review.” You also mentioned the Foundation for Advanced Education in the Sciences. Now, we have two foundations.

Harden: Yes. At this point I was just talking about the FAES. But if you want to talk about both of them, that is fine.

Wyatt: Well, I think of them in the same light. But the FAES, of course, is the older of the two organizations. It was founded in 1959, and it was founded by NIHers to do things that NIH could not do for itself. It was strictly an internal organization. To give a couple of examples of partnerships between NIH and FAES, we cannot provide health insurance for our trainees but we want them to have coverage. The FAES can, and it operates a very good and popular health insurance system program for trainees, pre- and postdoctoral fellows, and visiting fellows. Also, there were educational opportunities and needs that we could not provide, but the FAES could, and it created a graduate school—through which they brought on teachers and filled gaps in the training of fellows. When I came in 1971, my lab chief said, "You do not know anything about immunology. You need to go take the immunology course with the FAES." I dutifully did. It was taught by Bill Paul [Dr. William E. Paul] and by Dick Asofsky [Dr. Richard M. Asofsky]. This was fifty years ago. They taught a wonderful immunology course. It was topnotch, top-rated.

The FAES has also done other things that are not quite of that same magnitude, and one of them specifically is to support efforts in history by supporting the Stetten Fellowship Program. We all hope that that is going to be something that can not only be revived and then continued on a solid basis going forward with Institute funding.

Now the FAES is currently involved in a project to provide housing for trainees, and they are building a series of houses across the street from NIH, Cedar Lane and Cypress Ave. I just mentioned the Phil Leder symposium that was held for the purpose of dedicating a house that is named in his honor and his memory. This is perfect example of providing infrastructure not only for people to live, but also so that students get together and have scientific and social interactions.

The Foundation for the NIH is different. It came about in about in the 1990s through an act of Congress following a study of the intramural research program by the National Academies of Sciences. One of the recommendations is that we should have a foundation. Now we might have said that we already had a foundation, the FAES, but that is not what they recommended. They proposed a foundation authorized

by Congress that would be able to generate income to do big projects and to support science and all kinds of interesting efforts. And so as a result of that, Senator Ted Kennedy [Senator Edward M. Kennedy] sponsored the creation of the Foundation for the NIH, which was chartered in 1993. I've been associated with it from the very beginning. I like to tell the story that before it really got going, there was a meeting in Michael Gottesman's office. He and I were there, along with Benno Schmidt [Benno C. Schmidt] who was the first president of the Foundation for the NIH, and Deeda Blair [Deeda Blair], who has been with FNIH from the beginning. We were simply trying to figure out how this foundation was going to work. They have been remarkably successful. Just recently, they had their annual prize ceremony, and one of their prizes is the Lurie Prize in Biomedical Sciences. This year, one of the Lurie Prize winners was a former HHMI-NIH [Howard Hughes Medical Institute-National Institutes of Health] research scholar who was here as a medical student, Vamsi Mootha [Dr. Vamsi Mootha], who worked in the NHLBI [National Heart, Lung, and Blood Institute] laboratory of Robert Balaban [Dr. Robert S. Balaban]. Dr. Mootha now does beautiful work on mitochondria, but he got part of his start at the NIH as a medical student. This was also true of a former recent director of the National Cancer Institute, Ned Sharpless [Dr. Norman E. Sharpless], who was also an HHMI-NIH Research Scholar and who came back as an Institute Director.

Dedication of NIH Graduate Student Lounge with support by Fisher Scientific, An Early Project of the Foundation for the NIH (FNIH), 1990s. L-R: Michael Gottesman (NIH), Harold Varmus (NIH), Constance Battle (FNIH), Paul Montrone (FNIH), Richard Wyatt (NIH)

Harden: Before we stop today and move into your career in the Public Health Service, perhaps you will tell me about participating in founding the HHMI-NIH Research Scholars Program, which ran 1985 to 2012, the Clinical Research Training Program (CRTP) from 1997 to 2012, and the transition to the Medical Research Scholars Program (MRSP) in 2012. These programs covered the tenures of NIH Directors Wyngaarden to Collins [Dr. Francis S. Collins]. All of these initiatives were aimed at providing training for

medical students at NIH to become physician-scientists. Tell me about the programs and more about what you have already touched on, your thoughts about mentoring.

Wyatt: Before I do that, I was extremely fortunate as a medical student at Washington University to have had a mentor like Ralph Feigin. That was not a program but rather a self-initiated lab experience. I approached Ralph Feigin and asked, "May I work in your lab?" There was no organized program. It was just something I did on my own initiative. But I recognized that if I had not done that, my career would likely have been very different.

The programs you referenced all aimed to provide a more a structured approach in providing medical students a research training experience. Beginning with the HHMI-NIH Research Scholars Program, they all provided for mainly medical students to come to one place, the NIH, but they were hosted in various Institute labs. HHMI also had what they called the Medical Fellows Program that supported promising medical students at universities around the country where they did research for a year or two as they did in our intramural program. The HHMI-NIH Research Scholars Program was initiated because Don Fredrickson [Dr. Donald S. Fredrickson], President of HHMI at the time, and Jim Wyngaarden, Director of the NIH, talked. They put their heads together and thought, “What can we do to support medical student research interests?” One of Jim’s concerns was the decline of the research physician, a crisis that has existed for some time now in getting physician-scientists interested in research. We have to give credit to their ingenuity in coming up with the idea of the joint research scholars program, but also to HHMI for funding it so generously. The program accommodated up to fifty medical students a year to come to the NIH, and it was very well-run and generously supported by HHMI for 27 years.

There was a competitive process to become an HHMI-NIH Research Scholar. The program had exciting Monday night seminars with outstanding speakers. I used to help plan and go to all of the seminars. It was the only time in my life I ever heard George Thorn [Dr. George W. Thorn] speak, and I think it was actually the only time I ever heard Julie Axelrod [Dr. Julius Axelrod] speak as well—he was a former Nobel Prize winner at NIMH [National Institute of Mental Health], NIH. There was a tremendous outpouring of scientists who came to lecture. Harold Varmus used to come regularly and speak to the students. When we failed one year to invite him, he asked, “Why not?” So we invited him every year after that. Rick Klausner [Dr. Richard D. Klausner] was a regular, as were Bill Paul and Francis Collins; Victoria Harden [Dr. Victoria A. Harden, Office of NIH History and Stetten Museum] and Dr. Ed Pelligrino [Dr. Edmund D. Pellegrino, Center for Clinical Bioethics, Georgetown University] came as well. The lecture series represented a very broad array of speakers. The HHMI-NIH program also provided was a full didactic research experience, mainly based on a laboratory experience at the NIH. The students who came into the program initially had finished at least two years of medical school. They were not really clinically-oriented yet.

The basic fields of interest for the HHMI-NIH program were immunology, neuroscience, genetics, cell biology, and epidemiology, in other words, more basic than clinical fields. The students were assigned an advisor, and they would work to find a laboratory to work in; they would also gather for journal clubs and seminars as mentioned. The students were housed together, because HHMI took the old convent on Old Georgetown Road, renovated it entirely, and built an apartment facility as a separate wing at the

total cost of some $13 million in 1984 dollars. It was a big investment, and it was at the outset a wonderful home for the students.

The NIH campus was open in those days. That was long before 9/11, so there was a lot of coming and going in an open environment. It is different now to live on a campus, surrounded by a fence, but still the facility has continued to serve its purpose. The students themselves were outstanding. I already mentioned Vamsi Mootha and Ned Sharpless as two examples of students who have gone on in research. I think that the program met full expectations, but the ultimate extent of its success awaits a thorough analysis of the HHMI records of participants.

Celebration of Clinical Research Training: L-R Richard Wyatt, Executive Secretary, and David Nathan, Chair, NIH Clinical Research Panel, circa late 1990s

In approximately 1997, Harold Varmus decided that something had to be done about the state of clinical research, and so he created what was known as the NIH Director's Panel on Clinical Research. David Nathan [Dr. David G. Nathan] from Harvard was the chair of the panel. One of the Panel's recommendations was that since HHMI-NIH Program had been so successful, why not create something for clinical research training? The assignment that thus came from Harold Varmus to Michael Gottesman and me was to establish such a program. Harold said, "You have three months to do this," and so we created a program called the Clinical Research Training Program (CRTP). The Foundation for the NIH was very helpful in securing a grant from Pfizer to support this activity. Students came into the program much as they came into the HHMI-NIH program, although their clinical interests were more mature. And to

make a long story short, when HHMI stopped its support for the HHMI-NIH program and Pfizer later ceased to fund the CRTP, the NIH Medical Research Scholars Program was established, funded mainly by the NIH Scientific Directors. It, too, became a successful program and provides diverse research opportunities for medical students with a greater focus on clinical areas. The topic of research training medical students and the importance of an early research experience has been one of the richest experiences of my time in OIR. I did not realize this when we started the HHMI-NIH program when Ed Rall told me, "I want you to oversee this." He had confidence in me by asking me to oversee it, just like Harold Varmus had said earlier, "I want you to be the Executive Secretary of this Clinical Research Panel. Move it, help with it." It is wonderful in a career to have that kind of meaningful interaction with leaders who express their confidence, and it makes the efforts worthwhile.

END OF SECOND SITTING

This is the third sitting for the oral history with Dr. Richard Gregory Wyatt, on November 8, 2023, about his career in the National Institutes of Health. The interview is still being done over Zoom, and the interviewer is Victoria Harden.

Harden: Dr. Wyatt, in 1986, you took an intensive bioethics course at Georgetown. Can you tell me what prompted this and what you took away from it?

Wyatt: Certainly. Bioethics, for me, was closely tied to human subjects research, but also to the use of animals in research. I was interested in having some more conceptual knowledge, some principles to be able to apply with people who were thinking about the ethics of biomedical research. In 1986, I had already moved from NIAID to the Office of Intramural Research in the Office of the NIH Director. I had already served as Chair of the NIAID Animal Care and Use Committee and also chaired the IRB that serviced NIAID. I also had experiences in the conduct of research and the ethical issues that we've already discussed, including the use of prisoners in research studies. I had the background, but I wanted to be exposed to professionals. I can't remember exactly who advised me about this course, but it could very well have been the late Charlie McCarthy [Dr. Charles R. McCarthy]. Charlie was the head of the Office of Protection from Research Risks, OPRR. It was an NIH office at that time, and it oversaw animal research and human subjects research. Charlie himself had an interesting history. He had been a Jesuit priest, and then, through additional training and experience, he became the head of this ethics office that was based in the NIH. Subsequently, that office was split into two parts, one that continued to oversee animals and that resided and still resides at the NIH. The other was to oversee human subjects, and that has become the Office of Human Research Protections, which is now located in the Department of Health and Human Services. Charlie was a remarkable, thoughtful ethicist, and I always enjoyed talking with him. He educated me in ethics, particularly in what it is to conduct human subjects research. There was not a lot of work on the ethics of using animals in animal research at that time, although there were some vehement opponents, who in the 1980s broke down the door of Building 1, where I worked.

I think my interest in this course had to do with several things. First, one of the head leads of the Kennedy Institute [Kennedy Institute of Ethics, Georgetown University] where I took this course was LeRoy Walters [Dr. LeRoy Walters], and we had enlisted Leroy as a consultant to the NIAID IRB in 1976. This was the time when the swine flu vaccine had been deployed, and then we had seen the rare

occurrence of Guillain-Barré syndrome. Our challenge on the IRB (I was the chair of the IRB at that time) was to figure out how we would resume testing of influenza virus vaccines after Guillain-Barré had been observed with the swine flu vaccine, yet swine flu vaccine was not different from other flu vaccines. To do this we invited various subject matter experts to serve as ad hoc members of the IRB, e.g., a lawyer from Chicago, Canon Hamilton [Rev. Canon Michael P. Hamilton] from the Washington National Cathedral, and Leroy Walters from Georgetown University. I was impressed with this process, and I wanted to gain knowledge if I was going to work with what we now know as our NIH Human Research Protection Program.

I did not take courses very often, and this was not my practice. When I left medical school, I said, "Enough. I do not want to take any more exams. I am finished with that. I will use didactic experience instead of formal training programs." But I did enjoy the bioethics course at Georgetown, and I still have the reference books that came out of it. I also became interested in PRIM&R. PRIM&R stands for Public Responsibility in Medicine and Research, formerly headed by Joan Racklin. It is a robust, long-standing Boston-based organization that conducts annual meetings to consider both animal and human subjects issues, and I enjoyed attending and interacting with others who had similar interests.

Harden: In April 1987, you were invited by Surgeon General C. Everett Koop to, and I am quoting your words, "A small, seemingly clandestine meeting of selected senior NIH officers," aimed at revitalizing the Public Health Service's Commissioned Corps. You were subsequently appointed as NIH representative to the Office of the Surgeon General, and you marked this time as the moment you really began to undergo a transformation into a real PHS officer. Would you talk about that meeting and its impact on you personally, and then about the revitalization project more broadly, the involvement of different PHS agencies and whether the initiative has carried forward to today?

Wyatt: I would love to talk about that. There is a backstory and a context for where we are and why, in fact, the effort has not really continued in the same spirit. Let me go back to 1969, because at that time I was interested in research, and at the same time, I knew that there was a selective service requirement for every male physician graduating from medical school, and the American-Vietnam War was still underway. When I learned that I could go into the PHS Commissioned Corps and serve my country, I thought that would be a wonderful way to fulfill my selective service obligation, and at the same time pursue my interests in biomedical research. Since we have already talked about this, I am not going to repeat most of it, but I wound up coming into the NIH through what was called the Commissioned Officer Residency Deferment Program. I applied for the program during medical school; I got into the program; and I came in two years later in 1971. When I arrived at the NIH, I fully expected that I was going to look like a Commissioned Officer, and I always envisioned that I would be in uniform. That did not bother me, probably because as I was growing up in middle school and high school, I was active and uniformed in Boy Scouts, but it did bother many of my colleagues. I enjoyed serving and participating in Boy Scouts—the activities, the camping trips, the organizational aspects of mobilization. We always had a mobilization activity in February, which was Scout Month, during which we would go on some hypothetical deployment to rescue somebody from some danger.

At any rate, when I arrived at the NIH, I found that you could not tell who the Commissioned Officers were, because they did not wear uniforms. We Commissioned Officers had benefits afforded to military

officers. We could go across the street from the NIH to the National Naval Medical Center, which merged in 2011 with Walter Reed Army Medical Center and was renamed Walter Reed National Military Medical Center. We could go to the Navy Exchange, and we received our medical care there. We could go to the now defunct Officer's Club. Virtually all of my NIH physician colleagues were Commissioned Officers. But the Commissioned Corps was not then a functional uniformed service, at least not at the NIH. I think in some places it was, but not here, and that surprised me. For all intents and purposes, it was a pay system that allowed us as Commissioned Officers to be paid well. It was a career system too, because there was a carefully crafted system of promotion and advancement. I have often thought that salary was not a concern in the Commissioned Corps of the Public Health Service in 1971. I had one colleague who persistently forgot to deposit his paychecks because he had enough money that he did not need to worry about his bank balance. In those days, we received paper pay checks. There was no electronic transfer. I mention this because I think that having an adequate salary as a physician in training was extremely important. I did not fully appreciate it at the time, but it was. We were essentially well-supported and left unperturbed in our NIH Institute labs and clinics to do our research without many of the formal requirements that our military colleagues faced.

That was my experience during my first 15 or 16 years in the Commissioned Corps. I did not even own a uniform. There was a uniform in the Information Office at NIAID, where, if we needed to be photographed, we could put on the jacket and the cover, and it was the same cover and jacket basically for any officer who needed to have an official photograph made. I am a little embarrassed by it now, but that was the way it was, and this provides some historical context. The Commissioned Corps was the way we got to do research at the NIH during the American-Vietnam War. Physicians were attracted to the NIH because of what the NIH offered as a place that produced high-quality biomedical research and would teach us and allow us to do it. The opportunities were competitive, and historians, who have studied the Commissioned Corps, have made ample observations that very talented people came to work at NIH for 2-3 year periods of time and then returned to academia where they became recognized leaders. My point is that we had a Corps that was not visible by virtue of the lack of uniform, and yet we had leaders in academic institutions who advanced steadily in their careers via their contributions to biomedical research, but they did not necessarily credit the Corps for its seminal role.

When I arrived in Building 1 in 1984, Jim Wyngaarden was a Rear Admiral (Upper Half, O-8), but I do not think I ever saw him in uniform, even though he had an admiral's flag in his office. That admiral's flag outlasted Jim and was still in place when Dr. Bernadine Healy, who was not an officer, succeeded him as NIH Director. Jim's flag then appeared in some official photographs of Bernadine, and this prompted an irate call to me from the Surgeon General, who was upset that an admiral's flag had been misrepresented. His old flag now is archived in the Stetten Museum as an artifact from the last USPHS Flag Officer who served as NIH Director—an iconic reminder of the Yellow Berets and the American-Vietnam War era.

At one point, and I think it was probably in about 1995, we took a photo of NIH Flag Officers who gathered in the lobby of Building 1. Even though I was not yet a Flag Officer, the others invited me to be photographed with them since I was the representative of NIH Commissioned Officers to the Surgeon General's Office. There were many notable people there, including Tony Fauci, Director of NIAID, and John Gallin, former Scientific Director NIAID and later Clinical Center Director, along with Dr. Antonia

Novello [later Surgeon General Antonia C. Novello], who was at the time Deputy Director of the National Institute of Child Health and Human Development. So what had happened? Why were these outstanding physicians in the Commissioned Corps?

March 8, 1995
Photo of U.S. Commissioned Corps
Flag Officers at NIH
Location: Lobby of Bldg 1 (Shannon Bldg.)

Seated l to r:
Dr. Antonia C. Novello, RADM, US Special Representative to UNICEF/NICHD
Dr. Anthony S. Fauci, RADM, NIAID
Dr. Peter Greenwald, RADM, NCI
Dr. Arthur S. Levine, RADM, NICHD
Dr. Kathryn L. McKeon, RADM, CC

Standing l to r:

Dr. Samuel Broder, RADM, NCI
Dr. John I. Gallin, RADM, CC
Dr. William E. Paul, RADM, OAR
Dr. Peter L. Frommer, RADM, NHLBI
Dr. Donald H. Luecke, RADM, DRG
Dr. Phillip Gorden, RADM, NIDDK
Dr. Duane F. Alexander, RADM, NICHD
Dr. Darrel A. Regier, RADM, NIMH
Dr. Jerome G. Green, RADM, DRG
Dr. Richard G. Wyatt, CAPT, OD

NIAID--National Institute of Allergy and Infectious Diseases
NIDDK--National Institute of Diabetes and Digestive and Kidney Diseases
NCI--National Cancer Institute
CC--Clinical Center
OAR--Office of AIDS Research
NHLBI--National Heart, Lung, and Blood Institute
DRG--Division of Research Grants
NICHD--National Institute of Child Health and Human Development
OD--Office of the Director

Well, the Commissioned Corps was for decades the preferred pay and personnel system for physician scientists at NIH, and it recognized advancement to the top based on position. The Civil Service did not provide the benefits that the Commissioned Corps provided. Although most officers came for only two years or maybe three, some of us, including the physicians that are pictured there, decided to make a career of working in the federal government as Commissioned Officers. For perspective, one needs to appreciate that the pay of Commissioned Officers at the beginning of a career was proportionately higher than salaries in other federal personnel systems. As one advanced, it became less attractive to stay in the Corps for a full 30-year career (or thirty-three years for one-star admirals and thirty-six years for two-star admirals), especially as the dual compensation law was repealed, which allowed officers to receive both their Corps retirement pay and Civil Service pay. Also, in the interim, a new personnel system was implemented in the Department of Health and Human Services known as Title 42, which allowed administratively-determined pay for health professionals. It allowed physicians to receive higher pay than before, so retiring from the Corps and entering Title 42 made good financial sense to many physicians. Later, there was another pay system for health professionals known as Title 38, which was advantageous for former Commissioned Officers. I elected to stay in the Corps for 36 years since it had been a stable, rewarding career system with adequate pay and opportunity for recognition and advancement. Note that the Corps did not provide cash awards, and for 36 years I did not receive one.

Returning to the clandestine meeting in 1987, it took place in Robert Whitney's [Dr. Robert A., Whitney] office. Bob was the head of what we now call the Division of Veterinary Resources, then in the National Center for Research Resources, which he later headed. He was a committed veterinary Commissioned Officer. Veterinarians, like physicians were paid very well in the Commissioned Corps before the advent of Title 42. As with physicians, there was a time when we had a large, outstanding Veterinary Corps. Most of the research-oriented veterinarians at the NIH in leadership positions were in the Commissioned Corps at that time, and many had come to NIH with prior military service.

Before Dr. Koop's meeting occurred, he had been made Surgeon General, and perceived the Commissioned Corps as having value, something that he wanted to promote. So he invented the concept of "revitalization" of the Corps, about which he felt very strongly. He said at one point, "If I do not revitalize the Corps and support it into becoming something that I think it can be, I would simply have to reinvent it." He was one who saw the value of a uniformed service of health professionals, who were committed to public health, and he intended to do something about it. Therefore, he invited that small group of us—I still do not know why he invited me; maybe Bob Whitney mentioned my name—who represented some of the NIH Institutes but not all of them. He did not invite the NIH Director, Jim Wyngaarden, and this was perhaps a mistake. It would probably have been better to have worked with Jim to implement the proposal to revitalize the Corps, and perhaps there was some personal friction.

You would have to go back and read some of the very early history of the Commissioned Corps, as in the days of Joseph Goldberger to learn what it was intended to be. The Commissioned Corps was founded in 1887—I take that back, it was 1889, two years after the NIH was founded. Dr. Joseph Goldberger's Corps was different from the Corps I came into and more like the Corps that Dr. Koop wanted to recreate for the benefit of public health in the United States. I was sympathetic with what he was proposing to do, and although I did not know him well, I asked him at that meeting, "When are you going to require uniforms?" It was a rather naive question, because that was one of the more unpopular features of his

revitalization plan. He envisioned several other things, like assignment rotations, and he called for deployments in emergency situations or disasters. Although he planned to make the Corps more of a mobile strike force, he also recognized that the NIH was different and that the value of Commissioned Officers in the laboratories and clinics of the NIH was very important and perhaps unique—something some of his successors did not. So he made accommodations for officers to continue in their laboratory research and clinical work. Thus, as the Corps began to change around us, it did not cast out those of us in research but included us, thanks to Dr. Koop.

We created an entity called the Research Officer Group, which gave identity to officers who had committed their careers to research years earlier. Carl Merril [Dr. Carl R. Merril] and I worked on that entity together, but Carl took the lead. Carl was a Principal Investigator, an O-6 in NIMH. This system allowed officers who were actively engaged in research to be judged for promotion on the basis of their research. Furthermore, physician officers could claim special pay like that of an internal medicine subspecialist based on the “Research Officer Group” category. It was a wonderful retention system that was useful for a decade or two, as other federal pay systems were gradually introduced. After that, officers engaged in research, especially physicians, greatly diminished in numbers.

My wife and I got to know Dr. Koop and his wife Betty [Elizabeth F. Koop] at various events. There were plenty of Corps social events, and we recognized that we had a lot in common. We had mutual friends, and we had a shared faith. I had a lot of respect for him as Surgeon General and as a person, roles he kept distinct. He was Surgeon General when AIDS came about, and he had to address the problem of AIDS. When he was nominated to be Surgeon General, there were many who were concerned that his conservative faith would not be compatible with being Surgeon General. But that that did not prove to be true, and he himself said, "I am the Surgeon General for all people in the United States." And he did a beautiful job of separating science, faith, and public health. At one point he said to me, "Richard, never confuse the political right with the religious right." I've taken that advice to heart. We worked together for many years. After he was no longer Surgeon General there were other talented Surgeons General who followed, and I worked with each of them in succession as the NIH representative to the Surgeon General's Office from 1987 until 2007, but Dr. Koop was unique.

Harden: In 2004, Secretary Tommy Thompson [Department of Health and Human Services (DHHS) Secretary Tommy G. Thompson] appointed you Acting Surgeon General, correct?

Wyatt: Not quite. I was placed in the line of succession, assigned to take on the role of Surgeon General if the Surgeon General and the Deputy Surgeon General were no longer serving. I was third in the succession line, and for three years I held that position.

Harden: Were you ever called upon to act as acting? Were there two vacancies so that you were the third?

Wyatt: There was one vacancy. Rich Carmona [Surgeon General Richard H. Carmona] left the position of Surgeon General, and Ken Moritsugu [Acting Surgeon General Kenneth P. Moritsugu] took on the role I would have been next if Ken had left that position.

Harden: But he did not.

Wyatt: Let me be perfectly honest with you. That is not a role I ever aspired to, because it was a political position. It seems the autonomy and authority of the Surgeon General had been diminished over the years, and it was not something that I wanted to do. I was pleased that I retired from the Commissioned Corps approximately one or two months before Ken Moritsugu retired. Otherwise, I would've had the title of Acting Surgeon General.

Harden: I remember that NIH Commissioned Officers had to wear their uniforms only on Wednesdays. And associated with that, they do not seem to wear them any longer. Did it stop being a requirement very soon after Surgeon General Koop left, or did it fade out over time?

Surgeon General Richard Carmona at NIH Ceremony, circa early 2000s. L-R: Richard Carmona, Richard Wyatt.

Wyatt: What happened was that within the revitalization initiative of the Commissioned Corps, Dr. Koop agreed to a compromise. He said to NIH Officers, "Wear the uniform on Wednesday." Then others promoted an increased wearing of the uniform to full time. And I think it was Rich Carmona who said, "Let's just bite the bullet and wear our uniforms every day." Now by that time, the Commissioned Corps had thinned. When I was the representative for the Corps, we had something on the order of nearly

1,000 officers at the NIH, and now there are 200-some. I think I can safely say that if one is in the Commissioned Corps today, one wears the uniform regularly. It has become expected practice. The people who resisted wearing of the uniform have for the most part retired from the Corps. They may still be at NIH but not in the Corps any longer. Consider an officer like Rear Admiral Rick Childs [Dr. Richard W. Childs], who is the Scientific Director of the National Heart, Lung, and Blood Institute (NHLBI), he respectfully wears his uniform every day. I decided when I was working with Rich Carmona as Surgeon General that "If he wants us to wear our uniforms every day, I am a Commissioned Officer, and I will wear it every day." So now, having retired from the Corps, I frankly have a hard time getting rid of my closet full of uniforms, but that's only an outward reminder of my Corps career. I really enjoyed being identified as an Officer. I enjoyed wearing the uniform. It became part of my identity, and I wore my uniform daily during the last decade or so while I was in the Corps.

I want to note one other thing about my career as a Commissioned Officer. When I became a Rear Admiral in the Corps, and because I was active in the leadership of the Corps at the NIH and because the Surgeon General recognized what I was doing, I served on some interesting advisory groups. For few months I was the acting Chief of Staff in the Surgeon General's Office, but that was an add-on job. In the morning, I would go up to the Surgeon General's office in the Parklawn Building [DHHS Parklawn Building, Rockville, MD] to work in that position, and then I would come back to NIH and work in the Office of Intramural Research. I also made many trips downtown to the Humphrey Building [DHHS Hubert H. Humphrey Building, Washington, DC], because we had frequent gatherings of Commissioned Officers there. There is a group of senior officers called the Chief Professional Officers; each profession that is represented in the Commissioned Corps has a chief officer, so there is a Chief Medical Officer, a Chief Dental Officer, a Chief Nurse Officer, and so on. We also had the group of agency representatives, that is one officer from each of the agencies of the Public Health Service, e.g., IHS, CDC, NIH, etc. We had regular meetings of these groups to advise the Surgeon General. They were often chaired by the Deputy Surgeon General. When the late Faye Abdellah [Dr. Faye G. Abdellah] was the Deputy Surgeon General, she would often chair the meetings, as did Ken Moritsugu later.

This was an exciting time in my career because I was not just an officer assigned to NIH at that point, I was interacting with officers from CDC, FDA, Indian Health Service, etc., and it was spirited, collaborative, and congenial. In short, we enjoyed working together. We had professional discussions and professional challenges. One of the assignments that I had was to chair what was called the Promotion Review Committee. The promotion system for the Commissioned Officers had become cumbersome, and the Surgeon General decided that we needed to look at it very carefully, try to streamline it, and make sure that officers deserving promotion would be promoted. I chaired that group, and we prepared a fairly well-received report, and with a couple of exceptions, it was adopted by the Surgeon General. This was a satisfying assignment.

I also worked on a strategic planning committee for the Commissioned Corps. We came up with ideas for ways to make the Commissioned Corps stronger and more useful for the American people. All of this work was quite apart from my work at the NIH. In that particular report, we failed to gain traction with the Surgeon General, although we had transformative ideas and were trying to reshape the Corps into something that would serve public health well. I retired from the Commissioned Corps in 2007, and I am not connected with much that has happened since then. The system no longer attracts many physicians,

physician scientists, dentists, and veterinarians. It is a good system for nurses, social workers, pharmacists, and others. The Commissioned Corps has changed over the last 15 years or so. I always thought that it would be possible to maintain a useful and meaningful career system devoted to public health, not only for us, but also for future generations. I am not sure that has happened yet, and maybe it will happen in the future. Perhaps it won't, but I believe it would take Congressional support for sure to do it.

When I came to the NIH, I was not a committed Commissioned Officer in any way other than in name. By the time I left, I was dedicated to the Corps as a career system that I thought had promise to promote public health in the United States. I was a fully uniformed Commissioned Officer, who had advanced rather unexpectedly to the rank of a two-star Admiral, an unanticipated culmination of my Corps career. The Corps transcended DHHS agencies. It did not isolate us within one agency but rather bound us into a vital, dynamic career system.

I got in trouble occasionally with the Corps administration, one time about promotions. I had to advise the NIH Director that if he wanted to promote one officer to Flag rank, he was going to have to promote a second officer as well, and he made it happen. But as a result, the Surgeon General got upset with me and said, "What did I think I was doing at NIH?" I responded, "Well, sir, when your promotion system is broken, this kind of thing is going to happen." Maybe that's why I was appointed as head of the Promotion Review Committee.

I think it is probably not appropriate to do character sketches of all of the Surgeons General that I've known, but I would like to comment on Surgeon General Rich Carmona, for whom I had a lot of respect. He was a different kind of Surgeon General because he had been in the trenches providing health care. He had worked as an Emergency Medical Technician and as a physician. Thus, he understood what it was to provide emergency services, and he was very committed to deployments and the like. You may ask, did I ever go on a deployment? Well, I was deployed downtown to the Emergency Response Center in the Humphrey Building during Katrina [Hurricane Katrina, August 2005]. I did not go to Houston, but I was deployed to DC, helping to organize officers who were going to Houston. The NIH challenge was that we had many nurses in the Clinical Center, and they needed nurses in Houston very badly. We staged their deployments, so they did not all go at once and therefore disrupt the work and mission of the NIH Clinical Center.

There is one medal that I would like to show you, Vicky, because I am rather pleased with it. [Dr. Wyatt holds a medallion up to the camera]. On January 4, 2005, Dr. Koop was speaking at an anniversary event. He pulled me aside and said, "Come with me." The two of us went out into the hallway alone, and he handed me this medallion, which subsequently I've had embedded in lucite. He said, "I am giving this to a few people who I want to remember me." He handed it to me, and then we went back in the meeting. He was retired at the time, and it was two years before I retired. It is one of my special personal mementos about which I said, “It can't just go in a drawer somewhere." I'll likely pass it along to our son, Greg, because he remembers Dr. Koop from some of our gatherings.

Harden: Let’s turn to a completely different subject. On May 13, 1991, NIH Director Bernadine Healy fired Dr. Ed Rall as Deputy Director for Intramural Research, and this inaugurated an unsettled period in

the Office of Intramural Research. In July 1992, Dr. Lance Liotta became DDIR and served until August 1993. Dr. Healy herself resigned on June 30, 1993, and Dr. Ruth Kirschstein was acting director until Dr. Harold Varmus was appointed as NIH Director by President Bill Clinton in November 1993, when Dr. Michael Gottesman was named DDIR. Now, throughout all of this, you were one of the people who had to steady the ship and make sure everything continued to function smoothly. So would you talk to me a bit about this period?

Wyatt: The first thing I would say is that when I came into the Office of Intramural Research, Phil Chen was the Associate Director. He worked for Ed Rall, and I worked for Phil. Phil retired, I think it was 16 years ago. During that transition time in 1993, Phil Chen and I were both in the office, working closely together, trying to keep things moving forward. There was a period of one year when an NIH Institute Director was appointed as the Acting Deputy Director for Intramural Research. That was Carl Kupfer [Dr. Carl Kupfer]. And he, being Director of the National Eye Institute (NEI), had many continuing responsibilities with NEI, so he did not engage extensively in the work of the DDIR like Ed Rall had.

Let me back up to the event where you started, that Bernadine Healy fired Ed Rall. I remember the day very clearly. It was not, in fact, Dr. Healy who actually spoke with Dr. Rall. It was Bill Raub [Dr. William F. Raub], who was NIH Deputy Director, who came to Ed's office and explained that he would not be the DDIR for Dr. Healy. It was unsettling to us, because we had a well-functioning, rather small office. I think I mentioned this to you. We had four professionals and three executive assistants, and we had a lot of responsibility and oversight. Ed himself was a devout scientist, and I recall that Phil and I would go into his office for advice. I can picture him sitting with his feet up on his desk reading Science. He gave us his succinct, wise advice, went back to his Science, and we left to carry out his orders. I loved that image.

Harden: Why did Dr. Healy and Dr. Rall not get along?

Wyatt: I can't quote things that occurred, but I think it was quite apparent that Ed was a wonderful scientist, who was focused on his science. He did not have as much interest in the administrative process. As I implied, he was very happy to let Phil Chen and me do the administrative duties. Perhaps I may be being too charitable, and it may have been something Ed said, but I think that Bernadine wanted somebody who would follow her administrative leads in doing what she wanted to have done. Just as an aside, Dr. Healy was an interesting person. She came in with a great deal of passion for biomedical research. For example, she recounted a time when somebody with breast cancer came to see her, and the person said, "Please hurry, Dr. Healy. Please hurry." She responded to that plea with energy and compassion, but she was not the same kind of scientist that Dr. Rall was. So I think I'll leave it at that. It was not a compatible relationship.

Harden: I know that Hans Stetten believed that focusing on basic science was the best way to obtain new therapies over the long run. Dr. Rall was philosophically in agreement with Hans, and I understand your statement that if Dr. Healy was focused more on clinical applications—what might be called "applied research" instead of basic research—there would definitely have been a conflict between Dr. Healy's vision and Dr. Rall's as to what should be emphasized in intramural laboratory research. I have also heard from others that Dr. Rall was not pleased to have a woman as his boss. Would you comment on that?

Wyatt: I never heard him say that. But I do know that he was, as we would say today, laser-focused on science. That was his reason for being there, and I know that he was pretty clear about it. He had a passion for science, and that's what counted to him. I think it was more so an issue of incompatibility or respect.

Office of Intramural Research staff circa 1992. Back row, L-R: Philip Chen, Janet Smith, Toni-Ann Riley, Anahid Aryanjian, Margaret Quinlan, unidentified woman, Richard Wyatt. Seated at table: Lance Liotta, Catherine James, Audrey Boyle.

When Lance Liotta—a talented, physician-scientist and inventor—came in as DDIR, we worked together very well. I should also say that I had worked with Dr. Rall very well too. He was never unkind or uncivil to me in any way whatsoever. We had a wonderful working relationship. And when Lance came in, the same was true with Lance. I do not know how well he knew Dr. Healy, but he was her choice to be the DDIR. I think there was an Ohio connection of some kind. He did not continue beyond her short term of office, about a year-and-a-half.

When Dr. Gottesman came on board, he was DDIR, not only for Dr. Varmus and Dr. Zerhouni [Dr. Elias A. Zerhouni], but also Dr. Collins, and of course, Dr. Collins himself carried over as NIH Director during three presidential administrations (Presidents Barack Obama, Donald Trump, and Joseph Biden). For Dr. Gottesman to serve 29 years as the DDIR was totally unprecedented. DDIRs formerly did not serve that

long. I think it was a tribute to his skill as a scientist and also as one attuned to the administrative challenges that existed.

Harden: Would you talk in some detail about the transition here? Did Lance Liotta resign, or was he removed? Dr. Kirschstein became Acting NIH Director after Dr. Healy left in June 1993 and served until November 1993, when Dr. Varmus was named NIH Director. I would like to know what the process was when Dr. Gottesman was selected in 1993 soon after Dr. Varmus became Director. Was there a search committee, or did Dr. Varmus simply appoint him? What can you tell me about how it all played out?

Wyatt: When I think of Dr. Gottesman's role, I have to start with Dr. Healy's appointing him as the Acting Director of the National Center for Human Genome Research (NCHGR) after Dr. Watson [Dr. James D. Watson] stepped down. At that time, it was not an Institute, it was a Center. Dr. Gottesman was an interim head of the Center. And then of course, Dr. Collins was selected to be the head of the Center, which soon became the National Human Genome Research Institute (NHGRI). I do not personally recall the circumstances, or the conversations surrounding Dr. Gottesman's selection. It would have been Dr. Varmus's final decision. I do recall that his appointment as Center Director was rather hard-pressed, directed, and abrupt under Dr. Healy.

Harden: You and Phil Chen could breathe a sigh of relief once Dr. Gottesman was appointed DDIR.

Wyatt: We were very happy because our office has been characterized as being oriented to teamwork. It was a small team early on, but soon it became a much bigger team. I have heard our new DDIR, Dr. Schor [Dr. Nina F. Schor], talk even more recently about the teamwork in the Office of Intramural Research. We do have diverse talent and diverse backgrounds; we all work together collaboratively very well with both civility and respect in the interest of our NIH intramural scientists. Drs. Arlyn Garcia-Perez [Dr. Arlyn Garcia-Perez], Roland Owens [Dr. Roland A. Owens], Charles Dearolf [Dr. Charles R. Dearolf], Carl Hashimoto [Dr. Carl Hashimoto], and Kathy Partin [Dr. Kathryn M. Partin] are seasoned, hard-working members of that team.

Harden: I have two questions related to the transition to Dr. Gottesman as DDIR. First, tell me when Dr. Gottesman was appointed what new goals did he and Phil Chen and you have for the office? And second, please tell me how the office expanded from the four people to over a hundred today.

Wyatt: We listened very carefully to people outside the NIH. And let me see if I can very quickly get a reference book. Yes, here it is. We call it as you can probably guess from its red cover, the "Red Book" [Advisory Committee to the Director, NIH, Intramural Research Program: Report of the External Advisory Committee of the Director's Advisory Committee and Implementation Plan and Progress Report, National Institutes of Health, Bethesda, MD, Nov. 17, 1994]. When this was published in 1994, it was a year after Dr. Gottesman became DDIR. There had been an effort to do this kind of review of the Intramural Research Program when Dr. Liotta was in office as DDIR, and although he compiled a very nice report, he did it mainly with advice from within NIH. The Red Book report, however, was a report that was issued by an external advisory committee to the Director's Advisory Committee, and it became our guide, especially during the early Gottesman years. So to answer your question about the goals we developed, we adopted the directions that were established by an outstanding group of scientific

advisors, chaired by Gail Cassell [Dr. Gail H. Cassell] and Paul Marks [Dr. Paul A. Marks]. It had other notable people on it like Michael Brown [Dr. Michael S. Brown] and Ken Shine [Dr. Kenneth I. Shine], and Gerry Fischbach [Dr. Gerald D. Fishbach], Maxine Singer [Dr. Maxine F. Singer], Liz Neufeld [Dr. Elizabeth F. Neufeld], James Wyche [Dr. James H. Wyche], Arthur Rubenstein [Dr. Arthur H. Rubenstein], and Roy Vagelos [Dr. P. Roy Vagelos]. It is important to recognize, and we feel very strongly in the Office of Intramural Research, that we needed to listen to people who advised us from outside the NIH. One of the things that Dr. Gottesman did was to create an Advisory Committee to the DDIR, and that was something that he was very keen on so that this outside committee could review our programs in the OIR. There is really no other office in the Office of the Director that has the practice of inviting experts to come in to advise from outside. This is all done under FACA [Federal Advisory Committee Act] rules—the Government in the Sunshine Act. We asked them to evaluate our programs in the Office of Intramural Research. But it all started with this Red Book, and there were many other review groups in the Gottesman years. Remember the date of Red Book publication—1994—this is one of the last reports that we published in a paper book form. When we published it—and this was something that Dr. Gottesman felt very strongly about—we also published an implementation plan and a progress report to go with it. I think it is just remarkable, we still reference it. Out of its recommendations came, for example, our whole Principal Investigator tenure program at the NIH. Remember this is almost 30 years ago now. The tenure program was one of our main initiatives of that era. The report also detailed improvements in the way Boards of Scientific Counselors work. On page 57 are the signatures for the implementation plan: Harold Varmus, NIH Director; Ruth Kirschstein, who was serving as Harold's Deputy; and Michael Gottesman, DDIR. There was also a subsequent report on "Recruitment and Career Development of Clinical Investigators" at the NIH that was edited by Steve Straus [Dr. Stephen E. Straus] in 1997.

So when you ask about our goals, yes, we had goals, but we used this Red Book as our guide. We weren't just creating something out of our own minds but were listening very carefully to what wise outside advisors were saying.

There was an even earlier report on the Intramural Research Program, done by the Institute of Medicine (IOM) of the National Academy of Sciences (NAS), and we were challenged as to whether we could/should privatize intramural research under a contract model. It was published in 1988 and called A Healthy NIH Intramural Program: Structural Change or Administrative Remedies? As in later reports, there were outstanding advisors who were identified by the National Academy of Sciences to create this report. The chair was Harold Shapiro [Dr. Harold T. Shapiro], who was President of Princeton. Mike Brown, Roy Vagelos, and Gerry Fischbach were also on this outstanding group, just to mention a few advisors. Benno Schmidt, a philanthropist, was also on the group. The Office of Management and Budget had requested the Secretary of DHHS to contract with the NAS and the IOM for a study to evaluate the Intramural Research Program. This was a seminal event, and one of the things that happened as a result of this report was the creation of the Foundation for the NIH. Senator Ted Kennedy was a supporter along with a key staffer, Mona Sarfety, and others. I am emphasizing this because it is not what we say about ourselves, it is what we hear from outside organizations and what they recommend. It is so important today, because although the Intramural Program is roughly 10% of the NIH budget, it is a large investment of American taxpayer money. Good stewardship of that money is essential. I want to emphasize again that we do listen to what constituents and advisors are recommending.

OIR Staff circa 2020. Front row: Arlyn Garcia-Perez, Michael Gottesman, Roland Owens, Richard Wyatt; Second row: Carl Hashimoto, Joe Kleinman, Chuck Dearolf; Third row: Nadine Fonrose, Chandan Sastry, Rena Rodriguez; Fourth row: Jackie Roberts, Lisa Coronado, Melissa Colbert, Judie Walters; Fifth row: Chris Wanjek, Patrick Weitzel, Laura Carter; Sixth row: Andy Griffith, Margaret McBurney, and Andy Baxevanis.

Harden: In 1995, you were appointed as Executive Secretary for the NIH Director's Panel on Clinical Research, which was composed of physicians from academia and industry and chaired by Dr. David Nathan of Harvard. Its mission was "To review the status of clinical research in the United States and to make recommendations to the Advisory Committee to the Director, NIH, about how to ensure its effective continuance." A report was published in 1997. Tell me why this initiative was needed at that particular time, and what it accomplished.

Wyatt: From the time I arrived in the Office of Intramural Research, there were concerns about diminishing numbers of physician scientists, especially about physicians doing clinical research. Jim Wyngaarden was concerned about this in 1984, when he wrote about the disappearance of the physician scientist, but it was Harold Varmus, a laboratory scientist, who set up this clinical research panel but who worked in molecular biology. But he was quite concerned about the future of clinical research. In addition to David Nathan (Panel Chair), Harold asked outstanding physician-scientists to

serve on the Panel, including Bill Peck [Dr. William A. Peck], the dean of Washington University School of Medicine; Haile Debas [Dr. Haile T. Debas], the dean out at UCSF; Judy Swain [Dr. Judith Lea Swain] from the University of Pennsylvania; Jean Wilson [Dr. Jean D. Wilson] from Dallas; and other distinguished members. He was very keen on training, and there were others as well, although perhaps not as diverse a group as it would've been today—30 years later.

I had the greatest respect for David Nathan, with whom I worked closely. He brought with him a real heart for clinical research. He had been at the NIH as a Commissioned Officer in the 1950s. He wrote an interesting book, Genes, Blood, and Courage: A Boy Called Immortal Sword (Harvard University Press, 1995). He once said to me, "Richard, in the old days, only medical students who had rich fathers were able to go into research." That's a critical statement, since there were not other systems at the time to support clinical research training, and the burden of paying for the medical education, unless you had a family of means, was a problem. At any rate, David was a dynamic leader, and he's still very well-connected to the NIH, having served on the NHGRI Board of Scientific Counselors. When he came to NIH for Panel meetings, there was always a whirlwind of activity. He would arrive and complete packed agendas. Afterwards, he and I often would get in a taxi cab together to extend our conversation on the way to National Airport. In the taxi, I took notes while he talked. And then I documented the meeting with Janet Smith [Janet Smith], who was working on this project as well.

It was an efficient operation, and one of the things that came out of it was the clinical research training program (CRTP). Harold said, "We have the Howard Hughes program to bring medical students to NIH mainly for basic research, and we need a clinical research program." Other topics of the report dealt with a reworked clinical research grant opportunities, new approaches to study section review of clinical research, etc.

From my perspective, serving as Executive Secretary of the Clinical Research Panel was also one of the most meaningful assignments that I was given in my years in OIR, an assignment given by Harold Varmus. It was right up there in my estimation with overseeing and working closely with medical student training programs (HHMI-NIH Research Scholars Program, CRTP, and MRSP), as well as serving as the Executive Secretary of the Board of Scientific Directors with representatives of all the Institutes and Centers. It ranks up there along with working with the USPHS Commissioned Corps leadership.

Harden: Among many awards you received were two in 2018 and 2019 for your contributions relating to institutional review boards and the protection of human subjects in research. One award was for supporting and establishing the NIH Office of IRB Operations and the reorganization of the NIH Human Research Protection Program, and the other "For careful evaluation and synthesis of IRB operations at academic institutions to guide the centralization of IRBs in the NIH Intramural Research Program." You noted to me that you considered these major achievements of your career, and I'd like you to tell me anything else you wish about them.

Wyatt: You have already heard me talk about human subjects research, going back to when I was an actual researcher conducting clinical studies, some of those as early as 1971. We realized that we needed greater coordinated institutional oversight and established an Office of Human Subjects Research within the OIR. I was the Acting Director initially, but we soon brought on board Alan Sandler

[Dr. Alan L. Sandler] to head up the office. The awards you mentioned represent, in my view, a culmination and a new level of functioning of our Human Research Protection Program. That's actually the name that is applied officially at every academic medical center. We needed innovative leadership to make needed changes, although we had talented staffers who understood our needs.

We used to have 12 IRBs at NIH that operated in a decentralized fashion. Again, we collected advice from internal and external advisors, including the Institute Directors, Michael Gottesman's Advisory Committee to the DDIR, and from an invited review by the Association for Accreditation of Human Research Protection Programs (AAHRPP). They all uniformly recommended a centralization of the NIH IRBs, and we needed new leadership for that. One of the most important telephone calls that I think that I ever made in my career was to Jonathan Green [Dr. Jonathan Green], who was then at Washington University. He was working in this area of human research protections. I called him and said, "We have heard about your expertise, and would you consider a position at the NIH to head up this office?" He said that he would consider it. He added his name to the list of applicants and ultimately was selected. That turned everything around with our Human Research Protection Program. Jonathan was and is a wonderful administrator in this area, insightful, talented, organizationally superb. Because of his organizational abilities, we were able to centralize the IRBs. That is what these honor awards are about. Just a year or so ago, we got an outstanding review by AAHRPP, and this is the gold standard. I felt like that we had arrived at that point. I said to Jonathan, "It may be a bit biblical, but now I can 'depart in peace.'" He had totally turned around the previous program and created a wonderful human subjects research protection program, and the major innovation credit goes to him. My only simple part of that was making the phone call to him, exactly. In summary, whether I was supporting the Surgeon General, the NIH Director, the DDIR, NIH Scientific and Clinical Directors, or our NIH scientists, that was what I have enjoyed so much about my career.

Presentation of the Surgeon General's Exemplary Service Medal, May 14, 1989. Major figures in photo, L-R: HHS Secretary Robert Windom, CAPT Richard Wyatt, Surgeon General C. Everett Koop, NIH Acting Director William Raub.

Harden: Outside of the NIH, you consider your faith activities of great importance. You are a member and elder at Fourth Presbyterian Church in Bethesda. You've been a participant in the C.S. Lewis Institute Fellows Program. You are a member of BioLogos and The Trinity Forum in DC. Will you tell me about these activities and how they guide you and have guided your work at NIH?

Wyatt: I came to the NIH as a single person and if I had not had a community outside of work, I probably would not have stayed. I would advise anyone coming into a metropolitan area, like Washington, D.C., or Bethesda, to identify a community based on their mutual like-minded interests. In my case, my interest centered on a faith community and related organizations. I think that community could be established in other areas as well, e.g., music and the arts, but I think one needs to have, apart from the scientific community, a community in which one can participate and belong. I have had the benefit of working with people who are inspired and inspiring, who have been caring. And so for me, the expressions that come out of community support, belonging to those groups, and then pointing to, in this case, faith-based matters was so important. There are less frequent intersections with the scientific community too. Chick Koop, Francis Collins, a small number of fellows and trainees, and I also enjoyed periodic common faith-based interactions as well. Being in community is so important in understanding ourselves and in recognizing our own imperfections and need for humility. One NIH experience that I would like to recount occurred just last year, when a well-known New York pastor, the late Tim Keller [Timothy J. Keller], was being treated at the NIH Clinical Center. My wife and I were invited to be part a small group of ten folks who sang hymns with Tim and his wife, Kathy. Tim had selected six hymns to sing that told the Christian story on a Sunday afternoon in the atrium of the NIH Clinical Center, as Francis Collins played the piano there. To quote Francis, "Anyone who was there will never forget it." The scientific and spiritual realms intersected in that space that afternoon in the lobby of the Mark O. Hatfield Clinical Research Center. Senator Hatfield himself had also been a member of that very same faith-based community when he served in the Senate in Washington.

Harden: You and Linda were an "NIH family" since both of you worked here. You said she retired four years ago and that you have officially retired but have continued to work as a reemployed annuitant. A 2018 article in the Catalyst about your family said that you had six grandchildren, but you may have more now.

Wyatt: We now have eight, and we are hoping for more.

Harden: You must be very busy following their activities. Would you tell me a bit how you all are transitioning into retirement and what you enjoy doing.

Wyatt: It is important to maintain contact with a family who live in different places around the world. One son lives in this area with his family, but the others are away. One of our daughters lives in Texas with her husband and four children. The other daughter lives in Michigan with her husband and two children. And then our younger son lives in the Philippines with his wife, who is from Vietnam. Within about three weeks, we are headed off to Vietnam for some wedding celebrations. We have not met her personally, largely because of COVID, but we're going to Vietnam on the 28th of November and looking forward to celebrating officially their marriage. It does require travel, and we are committed to that. We

go regularly, not as regularly as we used to, but to Michigan, to Texas, and now to Southeast Asia. These are important activities.

The older grandchildren are gratefully beginning to pursue higher education. We have one granddaughter who has already started at Texas A&M. We have another granddaughter who has recently been accepted to Michigan State University and another grandson who also has recently been accepted to Texas A&M. Another grandson is committing to international travel and working as a digital nomad. This is another phase of life for us, and we are committed to supporting their higher education. This is something we look forward to doing.

Harden: In addition to visiting with family, what do you like to do in retirement?

Wyatt: Both my wife and I are currently doing a course, a year-long course, on the life, faith, and writings of C.S. Lewis. It involves reading, writing papers, group discussions, lectures, and it is very stimulating. So when you put all these activities together, we have not had a lot of free time. People often ask me, "What are your hobbies?" And I say, "Hobbies? Hobbies?" I stumble over that word. I can legitimately say that I like to garden, and we are into flowers, not vegetables. The deer and other animals eat the vegetables before we can, so we like to stick with deer-resistant flowers and shrubs and bushes. So then, with the traveling, studying, and maintaining the relationships and friendships in the various communities that we have discussed, our lives are full, not to mention a reduced but ongoing decades-long association with the Office of Intramural Research—now as a Special Volunteer.

Harden: These are all the questions I have, but I want to ask if there is anything else you want to get on the record before we stop.

Wyatt: One tends reflect on the impact one has had within our spheres of influence during a career. We do not have a Nobel Prize for our research in NIAID like Harvey Alter has to show for our scientific team efforts in diarrhea virus discovery and vaccine development, although we made some significant progress in this area.

I recall during my broader NIH career many meaningful interactions with scientists and leaders across NIH, DHHS, academia, industry, and indeed the world, and I hope I have helped in various ways to improve health and prevent disease. I hope I have contributed to advances in the conduct of biomedical research, like through improved human subjects research oversight, better ways of supporting intramural research and training NIH scientists, new approaches of doing clinical research, better care and use of research animals, improved ways of transferring technology to the benefit the public, etc. I have recounted here some personal career highlights and stories that may provide underpinnings and inform our scientific efforts and administrative activities. Mainly, I want to make clear that have enjoyed working with many wonderful, indeed remarkable, individuals, and I am grateful for my fifty-two years as a public servant at NIH!

Harden: Thank you very much, Dr. Wyatt, for an excellent oral history.