

*This is an oral history interview with Dr. David K. Henderson, Deputy Director of the Warren Grant Magnuson Clinical Center, on the history of the NIH response to AIDS. The interview took place on 13 June 1996 in Dr. Henderson's office in the Clinical Center. The interviewers are Dr. Victoria A. Harden, Director, NIH Historical Office, Mr. Dennis Rodrigues Program Analyst, NIH Historical Office, and Dr. Caroline Hannaway, NIH Historical Contractor.*

Harden: Dr. Henderson, would you begin by describing your background and education, and the positions you held before you came to the National Institutes of Health (NIH)?

Henderson: I went to undergraduate school at Hanover College in Hanover, Indiana. It is a small, liberal arts college in southern Indiana. I went from there to the University of Chicago Pritzker School of Medicine, where I got an M.D. degree in 1973. From there I went to Harbor UCLA [University of California Los Angeles] Medical Center, which is one of the UCLA teaching hospitals, where I did an internship and a residency in internal medicine. I then stayed on for a two-year fellowship in infectious diseases and, finally, the last year I was there, joined the UCLA faculty for a year.

Harden: What made you decide to go into medicine, and why did you decide to specialize in infectious diseases?

Henderson: The first question is much harder than the second. I have always been interested in science, and in my four years in undergraduate school I was a biology major and I took a lot of chemistry as well. I always seemed to be interested in the practical applications of basic science knowledge in medicine. But I also liked teaching. So I compromised in my own mind by choosing a career in academic medicine where, hopefully, I would never have to “leave the womb,” so to speak, but would be able to stay in touch with medicine and the practical applications of scientific findings as they came from the basic science laboratories into clinical medicine.

Infectious diseases, as a choice, was something of an accident. I had, for a long time, thought that I would be a hematologist and, in fact, went so far in my next to last year at Harbor [UCLA] as to accept a fellowship in hematology. But the infectious disease team at Harbor were the doctor's doctors. If you needed a “doctor consult,” that is, if you had a particularly problematic patient, the team that was always called was the infectious disease team. They were a cerebral group that was fun, and I was, I think, seduced by that. I am really glad that I was, because infectious diseases is, as it turns out, a very academic subspecialty.

Harden: So the intellectual rewards were a very strong pull for you, as they were for many people?

Henderson: Yes.

Harden: Could you describe your research interests before you became involved with AIDS? For example, talk about some of the papers that you were publishing and what you were interested in.

Henderson: Just to pick up, from my time at Harbor, in the last year of my fellowship, I became interested in fungal infections of man. Clinical mycology is, in some respects, a subspecialty in infectious diseases. The major reason I came to the Clinical Center at NIH was to work with [Dr.] John Bennett in NIAID [National Institute of Allergy and Infectious Diseases], who was at that time, and I suspect still is, the world's greatest living clinical mycologist. The opportunity to come back here to work and train with Dr. Bennett was a terrific one. Dr. Bennett was at that time doing the job of hospital epidemiologist *gratis* for the Clinical Center. There had never been a formal position for a hospital epidemiologist at the NIH. I actually called Dr. Bennett and said, "I have two job offers. I am looking toward a career in clinical mycology. Would you help me decide which of these two job offers is likely to be the best stepping stone to a career in academic medicine?"

He said, "Before you tell me about those two, let me tell you about a third option." He said, "Why don't you think about coming back to the NIH and being the hospital epidemiologist?" I said, "That would be terrific, if I had any relevant training." He said, "No one has any relevant training to be a hospital epidemiologist. Come back here and you can cover hospital epidemiology with a small fraction of your time and have the rest of the time to work in the laboratory."

So I came to NIH, the first year on an IPA [interagency personnel agreement], maintained my UCLA faculty position, was here for a year or a year and a half, and was working for the Clinical Center. I came down and spoke with Dr. Mortimer Lipsett, who was then the director of the Clinical Center, and told him that I needed to take time off to try to find a permanent position. He told me—actually he did not ask me, he told me—that I would not be leaving, that I would be staying, and that he would offer me a job as the full-time hospital epidemiologist.

Harden: Could you tell us more about Dr. Bennett and the Clinical

## Mycology Section?

Henderson: Certainly. Dr. Bennett, at the time I came, had active research interests in several fungal diseases. He had active investigations in aspergillosis, some in candidiasis, and some in cryptococcosis. I had been interested in *Candida* when I came to the NIH, coming from UCLA, but he encouraged me to become interested in cryptococcosis. He has a cohort of patients that he had treated over the years; he probably has more patients who have survived systemic cryptococcal infection than any living investigator. The disease in many people's hands has a very high mortality. Dr. Bennett is quite skilled at taking care of cryptococcosis patients and has a loyal following among them. His patients are willing, almost on a moment's notice, to come back and be studied. So we had several ideas of things we might do. I became interested in the role of the humoral immune system in host defense against cryptococcosis. We worked hard on that cohort of patients immunizing them with cryptococcal polysaccharide and also immunizing normal volunteers here and comparing their responses. We wanted to see if we could determine why patients who get this overwhelming systemic fungal infection, who have literally grams of cryptococcal polysaccharide circulating in their bloodstream, never make an antibody response to the polysaccharide. Interestingly, when we immunized normal volunteers they made a brisk antibody response. That was where my work with Dr. Bennett began.

Harden: This work on both of those infections set you up for work on AIDS?

Henderson: Certainly for learning a lot about both the humoral and cellular immunity. One of the things that Jack Bennett wanted me to do when I first came to NIH was to learn a little more about immunology. So, my old mentor from Harbor, [Dr. John] Jack Edwards, and I took two-and-one-half weeks off and went to Frederick, Maryland, to the American Association of Immunologists' Intensive Course in Immunology. That course was a real baptism by fire for me. It took me two or three days to gain an appreciation for precisely which language the lecturers were speaking. The course was a wonderful experience. Thus, really the combination of all of those experiences, I think, in retrospect—that set me up to be able to think about how we ought to manage this problem [AIDS] when it came to our hospital.

Rodrigues: You have already touched upon the role of the hospital epidemiologist and the fact that you were the first person formally

to occupy that position at the NIH. Dr. Bennett, you say, was doing this *gratis* before you came. But could you tell us more about this concept of the hospital epidemiologist? Was this something new emerging in hospitals?

Henderson:

The importance of hospital infections, per se, became apparent in the late 1950s and early 1960s when the staphylococcus became resistant to penicillin. There were epidemics of staphylococcal infections in hospitals around the country, with seemingly no way for physicians or the hospital staff to fight them. People did not understand the epidemiology of these infections, how the organism was being transmitted, or what one might do to prevent transmission. The problem of antibiotic resistance continued to accelerate, and the Centers for Disease Control [CDC] became interested. The first conference on nosocomial infections in the U.S. was held in 1970. It was called "The First International Conference on Nosocomial Infections," and it was essentially at this conference that the concept of a hospital infection control program was developed and discussed.

Such programs had been in existence in England for years, but in the United States, hospital epidemiology or infection control, as a discipline, really arose out of the CDC's interest in trying to control hospital-associated infections. Following that initial conference in 1970, there have been decennial conferences in 1980, 1990, and there will be another one in 2000, evaluating the progress of hospital epidemiology as a discipline in the U.S.

Initially, in most institutions, infection control was a nursing function, and most hospitals did not have physician hospital epidemiologists. Most hospitals had a nurse or two who did surveillance, collected surveillance information, and tracked down nosocomial infections. The nurse often had the assistance of a physician who volunteered his time to support the program. That is how most programs got by in the early days.

In the 1970s this new discipline of hospital epidemiology really sprang out of the Infectious Disease Society of America. Several individuals in that organization began to see a need for a full-time physician in academic hospitals to deal with the problems of the transmission of bacteria, viruses, and fungi in the hospital, in great measure because so little was known about the epidemiology and risks for transmission of nosocomial or hospital-associated infections. In a way, it redefined hospital epidemiology. As I said earlier, I had no formal training in either hospital epidemiology or in the formal discipline of epidemiology; what I have learned, I

have learned on the job. I would point out, however that I have not relinquished that job [of hospital epidemiologist] either, and the last hour before this interview I spent with my staff going over epidemiologic principles and some problems specific to the Clinical Center. Despite my increased responsibilities, I still enjoy working in hospital epidemiology and feel to some extent that the Clinical Center is "my laboratory."

Rodrigues: Was it Dr. Lipsett's decision then actually to formalize this position?

Henderson: What happened, as I recall--and again this is my best recollection--was that Dr. Bennett, in his own inimitable way, had gone to Dr. Lipsett and said, "You have me running the hospital infection control program, and I am also supervising the infection control nurses. It is getting to be more than I want to do and it is taking too much of my time. I do not want to have to do that all the time," (knowing all along that he had somebody ready to come and do it for him).

Rodrigues I see.

Henderson: When Dr. Lipsett said, "What should we do about this?" Dr. Bennett said, "I know this young lad who would be just perfect for the job." This was a classic NIH maneuver where he got Dr. Lipsett to ante up the salary for the first year and then the FTE [full time equivalent position] for the job. Dr. Bennett got another pair of hands to work in his laboratory and someone to do the hospital epidemiology function as well. It was a win/win situation for him and, I must say, for me as well.

Rodrigues: Let us shift gears now to focus on our project and ask you when you first became aware of AIDS. In a sense the way we have framed our question is not quite correct. You probably first became aware of patients with abnormal immune systems and a collection of bizarre opportunistic infections.

Henderson: The first I learned about the disease in detail must have been at the Infectious Disease Meetings in the fall of 1981. The meetings are scheduled long in advance, and this problem was beginning to surface by then. It had been known since June, when the first report was published, and there were several reports by the fall. There was an impromptu meeting held at night. No one likes to go to evening meetings; the only time you ever have any fun as an academic doctor is to go off to some convention and you have the evenings to go out to dinner with your colleagues. But the hall at

this impromptu meeting was jammed, literally, with people fascinated by this new disease.

I remember looking at the first patient at the NIH Clinical Center, not knowing what the patient had. The patient had been admitted to [Dr. Thomas] Tom Waldmann's immunodeficiency service and I went as a consultant and stood around the bed of a man whose name I used to be able to remember. I remember standing around with several of the world's most eminent immunologists looking at this young man.

Harden: Could you elaborate on that? Who was there? What did you see?

Henderson: I believe that Dr. Bennett was there. I believe that Tom Waldmann was there. I also think Dr. Michael Blaese was present. I think that Dr. [Anthony ] Fauci may have been there. I remember being in the patient's room and I remember standing around with the other consultants scratching our heads, not having any idea what this young fellow had. But I just remember being in the room. The patient was admitted to the Metabolism Branch of the NCI [National Cancer Institute]. That I remember for certain. And I remember going up to 3B-South in the Clinical Center to see the patient, and then he went to the ICU [intensive care unit].

My memory of that visit to the patient is extremely faint. But I can remember being in the room and, not having been here very long, being relatively junior, and being incredibly intimidated by the other people who were in the room.

Hannaway: Could I just interject, the society meeting that you went to where you heard about AIDS, was that the Infectious Disease Society?

Henderson: Yes. I am not sure whether the meeting was at night at the Inter-Science Conference on Antimicrobial Agents and Chemotherapy or at the Infectious Disease Society. Their meetings are held together, or were historically, until this year. But whether it was in the first part of the meeting or the second part of the meeting, I cannot remember. I believe it may have been in the Inter-Science Conference on Antimicrobial Agents and Chemotherapy.

Hannaway: A couple of other people who have been interviewed have mentioned that meeting.

Henderson: Anyone who had any interest in infectious diseases and who was at that meeting was in that hall.

Harden: AIDS was defined as an infectious disease by the time you were at that meeting? You were thinking of it as infectious?

Henderson: The syndrome clearly had infectious connotations. It appeared to be transmissible, and these patients had many opportunistic infections. The disease presented a clear conundrum—as I said, this is a very academic subspecialty—and it literally packed the room.

The next meeting I went to was an epidemiology meeting that was held up at the Holiday Inn Crown Plaza on Rockville Pike in Rockville. I remember that meeting as the first time I thought about the magnitude of this epidemic and how it was almost certainly going to impact on our hospital. I went to the meeting with [Dr.] Alfred Saah, who was then working in the NIAID Epidemiology and Biometry Program with [Dr. Richard] Dick Kaslow. Saah then went to Hopkins and was in their Multicenter AIDS Cohort Studies group. But he and I made notes about how we might be able to study some aspect of this syndrome in patients who would almost certainly be coming to the Clinical Center. I do not remember exactly when that meeting was, but I think it was maybe in the spring of 1982.

Rodrigues: The spring of 1982? When, in your recollection, did patients start arriving at the Clinical Center?

Henderson: The first one, I think, was in 1981.

Rodrigues: But in terms of more patients coming in?

Henderson: I think that [Dr.] Henry Masur had the first series of patients who came to the Clinical Center, because I remember working with him to try to develop a standard set of guidelines to be used for these patients when they were coming to the hospital. Those were the first patients I remember coming as what must have been some part of a beginning protocol.

Rodrigues: I see.

Henderson: The initial patient, Dr. Tom Waldmann's patient, was actually brought in, I think, before this disease was even described. That might have been in April or May of 1981. Was it June? It was?

Harden: We have the date pinned down as to when he came. But you are absolutely right in what you are describing, that he came to Tom Waldmann's service. No one knew what to do, and Tom

Waldmann's group was knowledgeable about immunology in general.

Henderson: Yes, Dr. Waldmann and his colleagues have one of the finest immunodeficiency groups in the world.

Rodrigues: You mentioned the meeting that you went to at the Holiday Inn Crown Plaza. You were thinking about the impact that these patients would have on the Clinical Center. Can you elaborate on what your concerns were about this new kind of patient population coming in?

Henderson: By the time that meeting occurred, it was becoming clear that we were dealing with an epidemic and that AIDS was very likely to be a transmissible disease. The epidemiology of the disease was, for all intents and purposes, identical to the epidemiology of hepatitis B in the community. We knew that much by then.

I knew already, as the hospital epidemiologist, even though I had only been one for just a little while, that health care workers are at extraordinary risk for acquiring hepatitis B in the workplace, and I began to develop serious anxieties about the risk our health care workers might be taking by providing care for these patients. No one had any idea what that risk might be at the time. We were beginning to think about that.

The AIDS Epidemiology Group had this meeting at Crown Plaza basically to discuss many aspects of AIDS epidemiology and to develop strategies to study it with more intensity in a variety of settings. It was from that meeting, I think, that the concept of the Multicenter AIDS Cohort Study (MACS) sprang. Dr. Saah and I attended a session at that meeting that addressed the hospital as a place to study the epidemiology of AIDS in a very general way. We had already had several discussions about the potential to study this new disease in the Clinical Center and had some ideas about how, in fact, to approach that subject.

Hannaway: What you are saying is very interesting to us because we would like to talk now about the process by which guidelines were developed in the Clinical Center for infection control relating to AIDS. We have some records of the Medical Board minutes dating back to 1982. This relates specifically to a meeting of 8 June 1982. You had been asked, or you and some others, perhaps, to develop a memo about safety guidelines for patients who were coming in on an NIAID protocol primarily for investigation of Kaposi's sarcoma. Then, in July, as we understand it, the Medical



Board voted to adopt your guidelines. Could you comment on those developments?

Henderson: Much of hospital epidemiology is based on common sense. Many of the standard practices and guidelines that we use in hospital epidemiology do not have their basis in solid science. Often we follow principles that have not been proven, but have grown out of reasonable practice over the years. A good example is in the operating room. Many of the practices in the operating room are used because some great surgeon used them, as opposed to there having been a study that showed that you actually ought to wear gloves in the operating room, or that you ought to wear a mask, or a gown.

We based those initial guidelines for HIV infection on the guidelines that the CDC had issued many years ago, in the early 1970s, to prevent transmission of hepatitis B and other bloodborne infections in the hospital. We were really in a difficult position at that time because we were very concerned about preserving the confidentiality and the medical privacy of our patients, but we were also working with an absolutely unknown magnitude of risk. On the one hand, you wanted to try to protect patient confidentiality and, to the greatest extent possible, preserve the privacy of the individual patients, but you did not want to... I wanted the health care providers to be as knowledgeable as we could make them and also to be aware that they were taking some risks that we could not measure. We tried to develop a strategy to identify the patients who had bloodborne infections in the hospital for the health care providers so they would be able to know when they were dealing with one of these patients yet, at the same time, tried to preserve the confidentiality and medical privacy of the patient.

Hannaway: What roles would you assign to, say, clinicians—you have said these recommendations often comes out of practical considerations—nurses, and other Clinical Center staff in formulating the guidelines? The reason that we ask this question is that we had the opportunity to talk with Barbara Baird, who was one of the nurses involved in caring for early AIDS patients at the Clinical Center. She remembers you consulting with nurses in your quest to determine what precautions should be taken.

Henderson: Nurses were our customers, as were the doctors in those days, and still are. So I think that if you are a constructor of guidelines, you do not do that in isolation—please excuse the play on words—and if you do not seek the advice or counsel of the people for whom you are designing these guidelines, you are not very insightful.

Every time a new piece of information became available, we immediately went to our nursing staff. Interestingly, our “open door” approach was not very popular with the leadership of the nursing staff in the Clinical Center. In the early days they were frightened and wanted to manage things their way. I did not feel that I could allow the information to be filtered by anyone. I was most comfortable going directly to the nurses who were providing care. That did not follow the traditional nursing hierarchy in the Clinical Center, and that was not very popular, but I did it anyway. What Barbara Baird remembers, I am sure, is that we sought everyone's advice and then tried to make sentient guidelines to which health care workers *could* adhere and to which hopefully they *would* adhere.

Harden: Following up on that, Barbara Baird actually went further and talked about the changing role of nurses in the AIDS epidemic, how they became technicians, and so on. I mean that nurses were doing more than they usually had done anyway. Would you say that AIDS produced a situation that caused everybody involved in caring for patients to have to adapt a little?

Henderson: I think I am going to air a commercial here. I think that AIDS did that across the board in medicine, but there is no place like the Clinical Center to make studying this kind of disease easy. One of the impressive things about the Clinical Center is that it is a huge ship in a small harbor; nonetheless, for a problem of this magnitude, it turned dramatically and rapidly. People pitched in because this clearly was a problem of expanding significance and everyone understood, I think, that we needed to learn about it. The nurses were spectacular. I agree with Barbara entirely. The nurses did all kinds of things that traditionally nurses did not do.

We have an extraordinary nursing staff here in the Clinical Center. For example, a substantial fraction of the nurses are master's level prepared. It is a very academic, very skilled nursing staff. If you have a good reason for doing something, and they can do it, they will help you. Nonetheless, I think one of the things that made many of those early studies “doable” was the fact that the Clinical Center, despite its huge size and bureaucracy, is extraordinarily malleable and allows that kind of change to take place quickly when it is needed. We have been able to do that for HIV in particular. When the multiply drug-resistant tuberculosis problem surfaced, we were able to modify part of the hospital to make the study of those patients possible. When taxol became available as a therapy for ovarian cancer, we turned our ship very quickly to get those patients treated here and to study that new compound. That

is part of the magic of working here.

Harden: Why can it be done here and not at other places?

Henderson: Most other institutions have patient care as their primary mission. The Clinical Center's primary mission is science. So we can turn our ship much faster. Patients who are admitted here are admitted electively. We provide patient care of the highest quality, but that is not our primary mission. The very best academic hospitals in the country have huge patient care responsibilities, but it is primary patient care, patients admitted with pneumonia, patients admitted who have had falls or automobile accidents or whatever, and most places are not able to turn so quickly to address a unique scientific agenda.

The institutes [at NIH] can change their research agendas very rapidly, laboratories close overnight, a service disappears. It is the magic of this place, and it is one of the things that makes it a national treasure in my view.

Hannaway: We have just discussed the changes in personnel interaction and also how it is possible for the whole institution to change and focus attention on new things. But reading the Medical Board minutes, one also gets the impression that there were certain points of controversy within the Clinical Center amongst various groups—you have alluded already to the nursing staff—about having the AIDS patients, about how many AIDS patients should be admitted, about what effect this was going to have on the Clinical Center and so forth. I wonder if you could comment on this? What do you see as the chief points of controversy about admitting AIDS patients to the Clinical Center?

Henderson: I think my recollection is that the chief point of controversy was the unknown risk. In part—I will digress just for a moment—almost all of us learned about this disease from the lay press, and one of the things I learned from dealing with this problem very early on is that the lay press is not necessarily in the business of providing you with perspective, or appropriate perspective. The lay press reports only the numerator, or only "the" risk, not the magnitude of risk. It does not say what the chances are that something will happen, just that it happens, and it is the headlines.

A friend of mine who worked for the *Washington Post* and I used to argue about this weekly, because he would explain to me that it is not the job of the newspaper to educate the public. But, on an issue such as AIDS, I think that—in the early days especially—the

lay press did us a great disservice. Our investigators, physicians, and nurses were no different from anyone else in the population, and most of their early learning came from the *Washington Post*. People were frightened, and I think that most healthcare workers had no concept of the magnitude of risk. When we did not know what the risk factors were for transmission and did not know what the risk might be to a health care worker, there were even some physicians who were thinking that it might be better to study this disease someplace else.

Rodrigues: That ties in with another question we had about cases where you could see how people in different roles dealing with the public overreacted. This, I believe, occurred with police, ambulance drivers, firemen, particularly in situations where they would go into a known gay bar or a place where there were many gay people and they would wear masks or gloves. Did you encounter any of these sorts of problems in the Clinical Center? Was it the role of the hospital epidemiologist to try and resolve some of these cases where people were kind of going over the edge in terms of having irrational fears about these patients?

Henderson: Exactly. I think that problem was very common. One could see something like that at least once a week on television. For a period of time on the nightly news, one could almost always find an issue similar to those you described that really fanned the flames of hysteria—even among our staff. It was not just the nursing staff; it was everyone in the institution—the physicians, nurses, technicians—everyone was concerned, and justifiably so. This new disease presented a risk that we did not understand. But I also saw it as our job.

Early on it became clear to me, as I said earlier, that there was not a better place than the Clinical Center to study this problem. It became very clear to me that we had several investigators who were going to be studying these patients, and I thought it was our job to try to manage the institution to make their studies possible. So the way we did that was to go out and tell everyone everything we could find out whenever we could find it out. That is how I made friends with this fellow from the *Washington Post*. I was always trying to find out what the news media were doing, or what was coming. When I heard some bad news from the CDC that we were going to get a Dan Rather hit, I tried to learn what it was, and whether we could get that information to the nursing staff before it actually appeared in the *Washington Post*. We used to do that regularly. What Barbara is remembering is that we would go and tell them, "Here is what it is, here is what it means. This does not

change what we already know, or it does change it, or, here is how it changes what we know." I think fighting the hysteria with fact in the early days was nearly a full-time job for me, but it was really fun because it kept us on the inside with respect to new information, and it forced us to maintain perspective constantly. We always had to be sampling the rest of the world for some comparison, because when the news media said something like, "Baseball catcher gets AIDS from pitcher's spitball," you had to retreat and say, "There is a chance of that happening. How does that risk compare to other things that happen in life?" That makes it possible for someone who wants to do the right thing, which is almost everyone who works in the Clinical Center, to look at those risks and manage them in their lives. But you have to fight that kind of ignorance with fact, and the reason why we began a study trying to assess the magnitude of risk for transmission of AIDS is that we felt as if we were obligated to do that. If we were going to have the patients in the hospital and the health care providers were going to be taking care of them, we needed to try to understand what that risk was at some level.

Rodrigues: I have heard many times before that the original model in trying to assess the risk of AIDS was hepatitis B.

Henderson: Absolutely.

Rodrigues: But we have also heard a different perspective. For instance, we have heard other people say, "In actuality hepatitis B is far more infectious than HIV." But, on the other hand, HIV has a longer incubation period, I think, than hepatitis B, so there are differences between...

Henderson: But, epidemiologically—that is how the disease is transmitted and how it gets transmitted in the hospital—those two diseases are very similar. The risk for transmission of the two diseases is quite different. For example, if a health care worker sticks her- or himself with a needle contaminated with blood from someone who is known to be HIV-infected, the risk that she or he will get infected is about 3 for every 1,000 such exposures. For a health care provider who has a similar exposure to someone who is 'e' antigen-positive for hepatitis B, you would anticipate someplace in the neighborhood of 35 percent of them getting infected. So, out of 1,000 health care worker exposures, 350 or so, might become infected with hepatitis B, and a substantial fraction of those will go on to develop sequelae of hepatitis B infection. It is not a benign disease. It is preventable. But now we have hepatitis C, in which it looks like there is about a 2-4 percent risk per exposure, and

there are other bloodborne infections as well. That helped us, I think, to put AIDS in perspective. Although a 1 in 325-330 risk is not a risk I would want to be taking every day, it does tell you that infection is not so likely to occur if exposure actually does take place. If every time you rolled down the runway at National Airport you thought that there was a 1 in 330 chance that you would not make it back to the ground safely, you probably would opt to take the train. Making those sorts of comparisons with those types of data helped us frame the occupational risks for HIV infection.

Rodrigues: Yes. I guess another complication is that you not only have the risk of HIV, but then there were other risks associated with the opportunistic infections.

Henderson: Really not very many. Most of the diseases that the patients had early on, setting tuberculosis aside for the time being, and again in the Clinical Center...although in the United States HIV and tuberculosis have become fast bedfellows, in the Clinical Center that has not been the case. To date the risk for tuberculosis is actually very small in our HIV-infected patient population, and that was especially true early on. Now we are reaching more aggressively into the inner cities to try to recruit HIV-infected patients, and so I think the tuberculosis risk may be rising.

But, setting aside tuberculosis, most of the other opportunistic infections that the patients have are not highly contagious, are not contagious for health care providers, and are not even easily transmitted among immunosuppressed patients.

The major risk that we were dealing with was a transmission risk for HIV, which was not a trivial risk. That 1 in 300 risk, if you stick yourself, is a terrifying problem and, as you probably know, we have had someone who got infected. So it was managing that problem, explaining that this risk was always there but that this was important work that had to get done, trying to develop guidelines, procedures, and processes that made it possible to do the work, and making certain that the staff got all the bad news from us, so when they read it in the *Washington Post* or the *New York Times*, they already knew about it. It gave us a great opportunity, I think, to keep the ship afloat.

Hannaway: How did you actually organize this transmission of information? Did you call meetings, or did you circulate materials?

Henderson: Initially care was provided for these patients in only a few places in

the institution, on the 11th Floor, on the 13th Floor, in [Dr. Philip] Phil Pizzo's unit, and in the MICU [medical intensive care unit]. Whenever there was bad news, I would call each of those places and say, "I need to come up and talk to you for just a while this afternoon." They would assemble the forces and we would work our way through it.

Hannaway: So it was very much person-to-person communication?

Henderson: Absolutely. It was my job. I felt that it was, at that time, as important a contribution as I could make. I think it was very important, or else it would have been very difficult to provide care for these patients.

I could not estimate how many presentations, as more information became available, we made about these risks to the Clinical Pathology Service, the Rehabilitation Medicine Service, or all of the services around the Clinical Center, because we wanted to get that information out. The best parts of those discussions come from participant's questions, where people's real anxieties surface and you have a chance to deal with them. You can do that either publicly—sometimes that is easier for people—or sometimes one-on-one, people would come in with questions. I would hate to estimate the fraction of my staff's time that was spent in those kinds of counseling activities, addressing those risks over and over and over again.

Hannaway: How many people did you have on your staff for this sort of activity?

Henderson: At the time I think there were four nurses, myself, and a secretary. Three of the four nurses were quite skilled in knowing about these risks and how to get the information out and how to talk with people.

Harden: Can you give us an estimate of how many of the AIDS patients were inpatients and how many were outpatients? How has the balance changed since the early 1980s to today?

Henderson: I do not know the precise numbers. I could get those numbers for you. We have those numbers carefully preserved and I can get them if you need precise numbers. All of the early studies were inpatient studies, and most of the patients were very sick when they first came, and the reason for that was that meeting the AIDS surveillance case definition was how patients became identifiable as AIDS patients. We did not know what the agent was that caused

this disease. The only way you could detect someone as having the disease was if she or he had symptoms and, as you know, that is pretty far along in the course of the illness. So the likelihood for someone who was admitted, given the fact that they were probably 10 years into the disease and already had a marker of disease, such as Kaposi's sarcoma or an opportunistic infection, that something else bad was going to happen to those patients, was pretty high. They were acutely ill, requiring a lot of care, and were difficult to manage in the hospital with many, many ICU days for some of those patients. They were very, very ill patients.

Obviously, over the years we have shifted to studying patients earlier in the evolution of the disease, and I suspect the overwhelming majority of our studies now are outpatient studies, trials of therapeutic interventions of one type or another.

Harden: I would like to have those figures at some point when you can get them.

Henderson: Okay. I can get them for you.

Harden: As you said, for the early patients who were very sick, it took many resources just to keep them alive, let alone to study them. What effects did this have on the allocation of resources here in the Clinical Center, and did it have an impact on other studies that were being done?

Henderson: That is a fascinating question. I doubt that you will ever be able to determine the answer. My suspicion is that you could get "polar" answers depending on whom you ask. To retreat to an earlier question, when we were talking about why we might, or might not, be studying HIV or AIDS at the Clinical Center, one of the early concerns was that if we used up a substantial amount, a substantial fraction, of Clinical Center resources, that might actually interfere with some of the institutes' existing research agendas. They, after all, have very important disease interests and research agendas as well. I think that concern was expressed. Whether we actually got that far is hard to pin down. My own view is that we did not. I think that the work of the institutes progressed at a reasonable pace in the Clinical Center with AIDS superimposed, to some extent. The exceptions might be in the Allergy and Infectious Diseases Institute service where they turned many of their resources over to AIDS because it was a fascinating infectious disease problem. If it were any other infectious disease, they would have done the same thing, I think, just because of that. That was probably appropriate. Also in the Cancer Institute, where they had Dr. Gallo's investment,



a huge epidemiology program, which was terrific, and then the early therapy trials as well, a large portion of the institute's resources was committed to AIDS research.

Other than those two institutes, I think the work of the institutes actually progressed normally, but you might be able to find institute clinical or scientific directors who would tell you that they felt as though it ate up too much of the budget and that it kept them from doing important things.

Harden: It certainly became a political discussion.

Henderson: Oh, we have not even touched on that. We focused on care, or the hospital, but certainly there were political aspects. It was fascinating from the start because it had everything all tumbled together.

Harden: Would you elaborate a little on that? Being in the hospital epidemiologist's position, I imagine that you were constantly on the hot seat about this.

Henderson: Yes, ma'am.

Harden: You have already talked a little about the press. Would you comment more on the press?

Henderson: I never felt as if the press were my ally. I always felt that the press, given the choice of being first and scooping the competition, or of being correct, would choose to be first."

AIDS was an incredibly political disease from the start. We had people from both sides of the aisle in Congress come out here, some lauding what we were doing, others, such as Congressman [William] Dannemyer from California, lambasting us for wasting the government's money on projects like this. We were always, I think, at the NIH level, very cautious about how the disease was managed, and I think the NIH scientists did a great job. The science of this disease has always been our pursuit and they just kept their eye on the ball. The quality of the work that has been done here really speaks eloquently to the whole approach that the NIH took. As I know you know, there were huge political issues, but every time we needed money to study the disease the politics just bubbled right to the top.

Harden: Would you explain a little more about your study on the risk of transmission in the hospital? Can you give us some details?

Henderson:

Following the AIDS epidemiology meeting that was sponsored by NIAID, Al Saah and I sat in my office, which was down in the B1 unit in the A-wing of the Clinical Center, down by the telephone operators. We drew up a plan for a study, which was based on the premise that eventually someone would figure out what caused this disease and that whatever it was that was causing this disease was likely to be something to which humans would make an antibody response of some kind. If we had serum in a refrigerator, or in a freezer, then we ought to be able to figure out whether any of our health care workers had gotten infected or not.

What we did was to design an elaborate, fifty-some page questionnaire asking health care workers exactly what sorts of procedures they were doing with AIDS patients. We thought that ultimately, since we would have collected serum samples over time, that someone would develop a test that we could use to measure them, and then we could go back and look to see what procedures in the hospital were associated with a risk for transmission. That was the overall design of the study. The questionnaire itself was overwhelming and, although it is a credit to the anxiety that was prevalent among the staff, virtually everyone completed those questionnaires dutifully, nearly 100 percent. For years they did that while we were collecting the data. Of course, it turned out that the questionnaires were absolutely useless because the risk is associated with parenteral exposures, and it is a 3 in 1,000 risk; the other things that people do with patients in the institution really present very little risk at all. There is always some risk, but we have this huge file of epidemiologic data about what health care workers did. Someday I will figure out something to do with the data.

The other part of that project, and sort of my hidden agenda, was that I was worried about our staff. I was incredibly appreciative of the people, like Barbara Baird, who were down in the trenches doing this work. If something happened to them, I wanted to be sure that we could make certain that we could show that it was a direct result of working with patients, or that it arose as a result of an on-the-job exposure. We wanted to be sure that we were protecting them as best we could. That is part of the job of the hospital epidemiologist. The agenda in starting that study had two parts.

It also offered the people working in the trenches some support. The fact that we would be out there looking for this with the expressed intent of protecting them as best we could I think sent

the right message. That message of health care worker advocacy is a very important part of a good program in hospital epidemiology. Those were our goals in starting that study. We also wanted to determine what the risk was.

Rodrigues: I think you have already touched upon the question that we planned on asking next. This has to do with looking back at what was being written about NIH, what was being said about NIH at meetings, on Capitol Hill, and by advocacy groups. Probably the most prevalent position was that NIH was not doing enough, not making the resources available, and so on. But I think the story you are telling us is somewhat at odds with that. Looking back, what is your view on all that? Do you think that the criticisms were valid?

Henderson: Let me just say that I have a very narrow and parochial view based on what was going on in the Clinical Center and the Intramural Research Program. I am not sure that you can throw any more successful money at the programs that were ongoing in the Clinical Center and have anything else come out of it. I think that we learned an important lesson from the "War on Cancer." It was that sometimes it does not help to put more money into a program. If you go back and look at the "War on Cancer," there were grants funded that were just a waste of money. I think that in this building, at least, we had the right people working in the right way with the right intensity on AIDS. You could have made these programs a small bit larger perhaps, but I do not think we could have done much more in this building than we were doing.

Now I cannot speak from a broader NIH perspective about whether the research agenda ought to have been tailored to fund more outside grants in the Extramural Program. I do not know what goes on in all of the other 48 or so buildings on campus, whether more money should have been funneled into that. Certainly at that time I was not privy to any of those numbers. But looking at the Clinical Center, I think that we were working hard and probably could not have either safely or efficiently worked a whole lot harder on the issue, especially understanding that all of the institutes had their own work that is ongoing. Admittedly we had an HIV epidemic, but we still had patients with rheumatoid arthritis and patients with all of these other diseases. The NIH is not the "National Institute of HIV Infection," it is the National Institutes of Health. We were, I think, obligated to commit substantial resources to HIV but, at the same time, we had to keep the other research agendas moving along. I think that we did that.

My own view is that, on balance, the leadership at NIH did a great job, and that we had a lot of money funneled into AIDS and most of it went to good solid investigators who knew what they were doing.

Rodrigues: One of the things that we have been looking at is the fact that the larger NIH—the Extramural Program, in particular—was not set up to be a rapid deployment system for investigating new diseases. It is designed to very carefully and thoughtfully evaluate proposals.

Henderson: Absolutely.

Rodrigues: We were essentially being criticized for not having some kind of rapid deployment system when it was never designed that we would have one from the beginning.

Henderson: That is correct.

Hannaway: You have said that you have a parochial viewpoint from the Clinical Center and as a hospital epidemiologist but, in fact, your publications suggest that you have had a much larger role. You have written articles for dentists, nurses, and a variety of other health care personnel which have appeared in general journals, like *Clinical Topics*, in which you try to discuss the general issues of the possibilities of infection and so on for such workers. Could you comment on your more public role as a spokesman upon these topics?

Henderson: Sure. I think the way that happened was because of what I said earlier, that we were primarily driven by science. We started systematically collecting information at a time when most people were not collecting that kind of information, but were thinking about it. We began to get some concept of risk very early on, in fact, very shortly after Dr. Gallo developed his first serologic test. We had 531 samples assessed by that serology, and so we had some idea early on about what that risk might be. But that is a story in itself.

Hannaway: Please tell us. We would like to know.

Henderson: Just after Dr. Gallo was confident that he had the serology, we persuaded his laboratory to run our samples. I got the samples and the results back, and I cannot remember what day of the week it was, but it was late in the afternoon. Out of our five hundred and some samples, we had 50 or 60 positive samples. I did a quick analysis of the data with what information we had about exposures

from the questionnaires that I told you about. There were several people on the list who had exposures and it looked as if there might be an association. We were very frightened.

Now, at that time, that serology was the very first generation ELISA[enzyme-linked immunosorbent assay]. We were doing a Western blot test as well, and our samples were very far down the waiting list for Western blots. In 1985, Dr. John Decker was the director of the Clinical Center, and I came to Dr. Decker with this paper with the results and said, "You have got to help me. I am very worried about our staff. These results are what we got from Dr. Gallo's laboratory, and it looks like we have got 50 or 60 people infected and we cannot get the Western blots done." So that was my first trip ever to Building 1. I went to Building 1 with Dr. Decker and sat with Dr. [James] Wyngaarden. [Dr.] Vida Beaven was there, and I cannot remember who else was in the room, talking about trying to get the Western blots. [Dr.] Ed Rall was also there. They ended up calling Dr. [Vincent] DeVita, the director of NCI, who was in Ocean City. They were able to track him down from Wyngaarden's office. Dr. DeVita called Dr. Gallo, and Dr. Gallo called the fellow who was doing the serologies, and we got our Western blots. Happily, they were all negative.

Harden: How much time had elapsed between when you received the data back and when you got the results of the Western blots? Are we talking about 24 hours or three weeks?

Henderson: A week maybe.

Harden: Because it raises the question of your ethical quandary. Should you call these people who have positive results and tell them, or do you wait?

Henderson: I learned some important lessons from this. One was that we had not sent controls; we just sent our samples off, because we were so excited to have the serology available. There were some samples that were split in the samples we sent. In addition, we had some workers for whom we had an early sample, a middle sample, and then a later sample. In some instances, the earlier sample was positive and the later one was negative, which did not make any intellectual sense. It was at a time when that laboratory, as you might imagine, was working three shifts to do HIV serologies, and the test was not very good. We got better tests and we got the results all cleaned up. We actually ran all the samples again two or three years later just to go back and make sure. But that was a frightening time, that is for sure, and I remember that vividly.

Hannaway: That was in 1985?

Henderson: Yes, ma'am.

Harden: Would you elaborate on how the Clinical Center has handled people who have become infected? How do you tell people, what happens to their jobs, and what kind of support does NIH give them?

Henderson: The Clinical Center has taken the position that people working in health care do not present a risk to patients in the process of delivering health care. We only have had, to my knowledge, just one person working in the Clinical Center who has acquired infection on the job. I think that we worked hard to try to take care of that person. That infection is a terrible thing and it has been very hard for that person, I am sure, but it is an occupational infection, and we are obligated.

By serendipity we learned that other people might be infected, but we do not have a policy that says that they should not be providing care. Then the issues get trickier. When the next provider to patient transmission case occurs—we have had the one case from Florida and a second suspected in Europe, and there will be another one sometime because it is bound to happen. As AIDS is a blood-borne disease, there is some risk for it to happen. We will have another such case and then the issue will become political again. But, as it stands currently, the Clinical Center has no policy against an infected practitioner providing care because, as best as we have determined, the one potential for transmission would be a procedure that a provider might be doing during which she or he would shed blood into a patient. The operating theater is the one place where you would worry most about that.

Harden: What about confidentiality? Do the co-workers of the person who was infected know?

Henderson: If they know it is only because the worker herself, or himself, chose to make that public in some venue. I think that, at least from the management in our Employee Health Service, I know the extent to which they went to preserve the person's privacy and confidentiality. In truth, it is my view that the co-workers do not have a need to know. But a single worker may choose to have his or her co-workers know because sometimes it is better to fight with a team than it is to fight by yourself.

Harden: This brings me to a broader question. I have personally heard from individuals who have had great differences of opinion about the entire way that AIDS was approached as an infectious disease. The traditional approach to STDs [sexually transmitted diseases] is to do contact tracing, to isolate the person if the disease is contagious, and so on. With AIDS, both the CDC and the NIH have bent over backwards to protect confidentiality and civil rights. There are people who think that the epidemic itself would have been stopped, or slowed, if more traditional, coercive measures had been employed. Would you comment on this?

Henderson: Let us go back to our mission at NIH. Our mission here is science. The people who come to the Clinical Center are not patients in a sense; they are our partners in research. I think that we make a very different contract with those people than other physicians make with their patients. If we cannot preserve their privacy and confidentiality in such an arrangement we are not going to get much work done. I would surely never come here to participate in some research project which offered me no benefit except to push back the frontiers of science if I thought that the physicians were going to tell my insurance company or my employer.

Let me tell you another true story. I was on a call-in television program that was sponsored by the National Chamber of Commerce. I went downtown Washington to do it—it was a cable satellite video hook-up—and I am sure that probably fewer than 100 people in the United States were watching, but some fellow was asking me questions and then they had a time for people to call in. A person called from some place in Kentucky and told the following story:

The person said, "I went to a doctor, as I had swollen lymph nodes, and the doctor examined me and said he did not know what was wrong with me. I did not feel too bad. But he sent off a bunch of tests. He called me up the next day—or a week later—to say the test for AIDS was positive. He wanted to know about my lifestyle and stuff like that. I said I did not think I had any risks for that disease, but I did not know anything about it because who knows. So he said he was going to send my blood off to somebody in Atlanta, but in the course of that he put down on my form that he filled out to my insurance company that this test was positive. I got a call two days later from my insurance company and they canceled my insurance. My insurance came from my boss, my job, and they called my boss to tell him that they canceled my insurance, and he fired me. Then my doctor called me up a week later to say that he had gotten the results of the blood test back

from Atlanta, and they said the test was not positive anyway."

So I thought the question that he was going to ask me—this was all prologue to his question—was if I knew the name of a good lawyer, but it turned out that he wanted to know how one test could be positive and another test could be negative.

It is an instructive point. We were evolving at that time. We needed to learn about this disease desperately. We needed to work as partners with the people who were at risk for the disease, we needed to work as partners with the people who had the disease, and we needed to protect them in every way we could because they were our partners. I think that the good epidemiology that was done in the early and late 1980s would never have been done without that partnership.

Rodrigues: Let us shift to your role as an NIH representative working outside of the NIH with other agencies like the CDC. We know that you participated in some of these activities. I wonder if you could comment about your role in the NIH's collaboration with the CDC on this?

Henderson: When I was asked to go to the CDC in Atlanta initially, I think that the CDC had called Dr. Fauci, noted that they were designing these guidelines, and asked if Dr. Fauci wanted to send somebody to participate in the discussions. I was elected. But when I got there they just asked me questions. They asked me to present our data and talk about what we thought was right. I went, I thought, to learn and ended up being one of the experts. This series of meetings was my first experience in trying to find my way through a maze to come up with meaningful guidelines for the country, for the health care workers of the U.S., with all of these special interest groups represented at the table: people from the unions, people from the firefighters, everyone with a single axe to grind, with the CDC being given the charge of coming up with meaningful guidelines, ones that actually prevented transmission of the disease. It was a fascinating process. I learned much more from that process than the CDC got from me. But I enjoyed contributing and I still do that. I still go regularly as the NIH representative to the CDC to tune up the guidelines. It is a great process.

You asked about the NIH and its relationship to Public Health Service guideline development. NIH almost always is asked to contribute and almost always does participate.

In fact, an interesting part of the hospital epidemiology story is that



almost all of the original hospital epidemiologists in the United States came from the CDC as Epidemic Intelligence Service [EIS] Officers.

Hannaway: You added onto your career as hospital epidemiologist by becoming coordinator of AIDS activities for the Clinical Center in 1985. How did this appointment come about and how did it change, if it did, your responsibilities?

Henderson: It came about because Dr. Decker wanted someone to do that who had a broad-based view of what was happening in the Clinical Center. We had some people, [Dr.] Henry Masur, for example, who were aggressively investigating specific topics related to AIDS and HIV infection. John Decker, I think, did not want to send someone who had narrow interests to this NIH-wide committee to talk about how resources were going to be managed and so on; he wanted someone who could take the perspective of the Board. Participation in this committee provided my introduction to an NIH-wide administrative point of view, painful though it was.

Harden: But you were also a part of an expert team that was convened by Dr. [James] Wyngaarden after two workers in two different laboratories were infected. Would you talk a little about that team, who was on it, and how it worked to investigate those problems?

Henderson: [Dr. Robert] Bob McKinney, who was, by then, I think, the director of the Division of Safety, was asked, or maybe it was right at the time that [Dr.] Emmitt Barkley was leaving, when this investigation came up where, as you say, people had been infected. They put together a group of people who were expert primarily in laboratory safety. I was asked to participate because I had developed an interest in the epidemiology of HIV infection in the hospital. We went to places, listened to presentations, evaluated practices, and talked about what was going on in those laboratories and how one might tune up procedures to decrease the risk of transmission. I played only a very small role in those discussions. The bulk of the work was done by people expert in laboratory safety.

Harden: Would you elaborate on your earlier comments about the uniqueness of the NIH and especially the Clinical Center as a place to investigate any new disease? What are the pluses and the minuses?

Henderson: The pluses, I think, I have really underscored. We have a

remarkable assemblage of basic scientists who understand almost every basic science principle that exists. We have "translational" investigators who are able to take basic science ideas and move them from the laboratory into clinical medicine. We have a wonderful infrastructure of equipment, of cutting edge technologies, and we have a clinical support staff that is second to none. So the pluses are the remarkable resources that are here to do wonderful clinical science.

I think that there are very few minuses that you will find from talking to investigators around the campus. Some minuses might be related to the fact that the Clinical Center is not a full-service hospital. If a person has an orthopedics problem we have to get an orthopedics consultant to come out here and that is uncomfortable, or, at best, it is cumbersome in some instances. I cannot think of too many other minuses.

Harden: What always strikes me, as we talk to people about this, is that even though the NIH is a huge place it becomes a very personal kind of operation. It is a very small village in that sense. For example, if somebody has an eye problem, you know whom to call, and in calling a person, you are relying on your personal knowledge about a person's skill, as opposed to a set of credentials on paper.

Henderson: Right. Over the years, the people who have been assembled here are stellar. So if you have an eye problem and you call the [Dr. Robert] Bob Nussenblatts of the world, you know you will get a stellar eye exam and insight not only into just what the lesions are, but how they might fit into the overall picture. The institution works remarkably well. When we had our external review last year, when the Secretary of Health and Human Services sent Dr. Helen Smits and a team in to look at us, one of the first things that we tried to explain to them is the extent to which the staff of the Clinical Center and the Institute/Center investigators are woven together like a piece of fabric. To take out part of that staff or to contract out for part of that staff we would do irreparable harm to the Clinical Center and to the Intramural Program, because it is all put together just as you say—you know whom to call and how that works—and it makes for very high quality care.

Harden: What is your sense about how well known this is among physicians and scientists? What about among the politicians, the general public, or the press?

Henderson: I think that the Clinical Center is not well-recognized by the

general public, medical professionals or the press. I think people have been surprised to find out all the things that have happened in this building and all the wonderful things that have come out of here. It is really quite a remarkable place and has been, in my view, which is admittedly quite a narrow view, a wonderful investment of tax dollars, but I think very few people actually know what goes on here.

Hannaway: Another position, or something that you have been involved in, was the Physician's Advisory Committee on the Watkins Commission on the HIV Epidemic in 1987. Were there any interesting aspects of that? I am sure there were many, but could you tell us about some of them?

Henderson: This is one of my favorite stories because it shows how foolish I can be. Dr. Decker got me into that as well. I cannot recall exactly how my name first got thrown in the hat. But there were only four of us in Admiral Watkins' "kitchen cabinet." What happened is that Dr. Eugene Mayberry was the first commissioner and he quit; the second commissioner was Admiral [James] Watkins. All we knew about Admiral Watkins was that he had come from the Navy, that he was not a physician, that he had no medical background and did not know anything about AIDS.

So, the next thing I knew, I got a call from someone downtown wanting me to be one of four physician advisors to Dr. Watkins. They wanted a neurologist, Al Saah, who is an epidemiologist, and I cannot remember who the fourth person was.

Hannaway: We have a copy of the report so we can look it up.

Henderson: We were asked to meet with Admiral Watkins. So Dr. Saah and I took the Metro down to some building where they had offices and we went in and sat down with Admiral Watkins. He came in, sat down, and said basically, "Look, I do not know anything about this disease. The President has asked me to do this job and I am going to do it, and I am going to do a great job, but I have to learn and you all have to teach me." He said, "By the next time you see me," which was, I forget now when we were supposed to meet again, in six or eight weeks, or something like that, "I will much more knowledgeable about this disease."

After the meeting, the physician advisors walked around the corner to the Old Ebbetts Grill and had a beer, and I said, "I am not going to do this." "I do not want to have anything to do with this project."

So I came back to the Clinical Center and tried to convince Dr. Decker that at some level, it was a conflict of interest for me to be involved in this commission because they were going to be evaluating us and so how could I be advising Admiral Watkins. He said to me sternly, "This man needs help. You go down there and help him."

Well, I will tell you, I have never been so wrong as I was about Admiral Watkins. He is a brilliant man. By the time we went back six weeks later he could speak the language of AIDS as well as anybody. In fact, we finally invited him out here to give Grand Rounds. He actually gave medical Grand Rounds here and talked about AIDS. I can still remember him answering questions. People were asking him medical questions about articles that had been written about cognition. He said "You are talking about that paper that was in the *Archives of Neurology*, and there is a much better paper in the *Annals of Internal Medicine*." Participation in the Physician Advisory Group to Admiral Watkins was quite an experience. We spent hours with him and his staff basically going through issues just to try to give him our scientific reading of where the issues were at that time. He had two wonderful special assistants and the way his mind worked was he would take on an issue and we would talk about the issue in paragraphs, literally, just spinning off paragraphs for him. He would look at one of his assistants and he would say, "Do you have that?" She said, "Yes," and we would move on to something completely different. We just spoonfed him about HIV infection as rapidly as we could and gave him a reading list and things to look at. He got up to speed faster than you would ever dream. He did not need an M.D. degree to do that. He was wonderful, and quite an impressive man.

Hannaway: None of this is apparent in the report, is it?

Henderson: He was a very smart man, and you get that from him very quickly.  
Rodrigues: Yes. I think he gave a speech at the First World AIDS Day that was held at NIH and it was probably one of the best talks I have ever heard.

Henderson: He was astounding. I tend to get very quiet around him, not wanting to show my own ignorance. He was very smart, a very hard worker, true to his task, and did a great job, I think.

Hannaway: Did you have any involvement with Dr. June Osborn's commission?

Henderson: Yes. I testified before that commission once or twice, maybe

twice.

Hannaway: She is rather impressive herself.

Henderson: She is a major star, and was also a wonderful choice. She was more of an academician than Admiral Watkins. He was just going to get the job done. He had a military, "We are just moving through this," approach.

Rodrigues: We are coming to the end of our questions here and one of the questions that we ask all of the people that we have been interviewing is a two-part question. It has to do with how your involvement with AIDS has affected your professional and personal life. Some people seem as though they are very capable of keeping the two things separate. AIDS has not really created stress or problems for them in terms of overloading them with the immensity of the problems with which they are dealing. Some people see themselves as separating their professional lives from their personal lives; other people seem to have a harder time doing that. I was wondering if AIDS has created any problems in your life. Also one of the things that we have found intriguing is how some people said that, when they looked at their professional lives, how surprised they were at how far AIDS pushed them in directions that they did not think they might have gone.

Henderson: Absolutely. The AIDS epidemic certainly changed my professional life. I was going to be a clinical mycologist. I was doing hospital epidemiology just because it was a way to get a position to work in a basic science laboratory. There came a time when I had to choose between mycology and hospital epidemiology. I was in somewhat of a schizophrenic position. I was being paid a full salary by the Clinical Center to be the hospital epidemiologist. Dr. Bennett thought that I would only need to take 10 or 15 percent of my time to do that and that I would have 85 percent of my time to work in the laboratory. When I got here there was not any infrastructure for hospital epidemiology so we had to create it. It took me a year to get to the laboratory, but I finally did get there and worked successfully with him. But I came to a crossroads in my career where either I was going to have to do hospital epidemiology full-time or go back and work in the laboratory. I actually went down and had a long talk with the person who ran the Microbiology Laboratory, a man named [Dr. James] Jim McLowry, whom you may or may not know—he is retired now—and just sort of laid out my options as I was offered what to do. I said, "I have to make a decision because I am not getting enough done in the laboratory to justify my space with Dr.

Bennett and because of the pressures of the Clinical Center and of trying to get this work done." He said, "You have to do what you think is best." I felt obligated, because the Clinical Center was paying my salary, so I became a full-time hospital epidemiologist. In some respects AIDS made hospital epidemiology a full-time job, not only at the Clinical Center, but at many places. There was so much angst about the risks and what was going on that you needed to invest substantial resources into it. So it clearly changed the course of my career.

In terms of how that affects your life, because I had the opportunity to do some of the things we have talked about, I always felt as though I was an insider who had wonderful inside information about this disease and that the country was ill informed about it. I had the opportunity early in the epidemic to return to my hometown, a town of 13,000 people, and talk about this disease to my undergraduate school, which has a student population of about 1,000. I have seen this as an opportunity for me to pay back the NIH at some level for what it has invested in me and also, I hope, to help people by doing that.

Harden: What about any effects on your personal life? I believe that your wife has been involved with AIDS as well.

Henderson: That is right.

Harden: Do you have children?

Henderson: We have children.

Harden: How old are they now?

Henderson: We have two sons, one is 19 and one is 16, and one daughter, soon to be 6.

Harden: We have heard from some people, especially those who had teenagers, that in 1985-1986, when the hysteria about AIDS was at its peak, that they felt some pressure. Their kids did not want their friends to know that their parents worked on AIDS.

Henderson: That is fascinating, because it is my impression that my kids liked it that I worked on AIDS. They asked me to come to their schools to talk about the disease. Because I talked about risk, that provided a reasonable approach for teenagers. At one time I had much of the information that people really wanted to hear and I think I learned over time to be able to present the information in a way that both

scientific and lay audiences could relate to it. So I went to both my kids' schools and to church and talked to the church groups, and I have done a lot of community service presentations. To my knowledge, my children never ever expressed any reservations about that.

Now, they were younger then, but they were perfectly happy in having me do that, I mean as happy as a kid ever is to have his parents show up and be doing something like that. At some level they are sort of proud of you and at some level they really wish you were someone else's father. I do not think I felt inhibited about that at all.

Harden: I also recall that Dr. Fauci told us at one point that he and his wife had decided that this was what their life was going to be dedicated to. It sort of took over their lives. I just wondered if you and your wife had had similar personal discussions of this kind of thing?

Henderson: I think there were times that the frustrations of it, the sort of bad news, when bad news surfaced, took away more time. But, as is the case with Dr. Fauci's wife, my wife is a nurse and actually worked with him for a while, when she was his special assistant for AIDS. So she was very knowledgeable about the disease. I think it was a pain in the neck sometimes, when we ended up here on the weekends or had to come in and those times when really bad pieces of news surfaced. When we had our infection, for example, we went systematically to every department, to every shift, talking about what it meant and making certain that people had a chance to talk about it because it was one of us. That took a huge investment of time, but I felt it was just part of the job. This is not a 9:00 to 5:00 job, so I do not think I ever felt it. It changed the course of my career, but it did not change the course of my personal life. But periodically it was oppressive.

Harden: Did you worry about becoming infected yourself or about your wife's possibly being infected?

Henderson: Not too much. I tried to be both sensible and careful. During this time, I also worked as a moonlighter at one of the community hospitals and took care of lots of HIV-infected patients. I did not worry too much about it, I think, in part, because I understood the risks pretty well and knew the risks I was taking.

Hannaway: As far as your personal career, I wonder if you would comment on the effect of AIDS on the infectious diseases field. You obviously have reflected on the field of infectious diseases. I remember when

I was on the faculty at Hopkins in History of Medicine and I went to a Medical School Council meeting. The Council had a representative from every department, and the man from infectious diseases came running in and he was all excited—this is very early on in the 1980s—about this new problem. He was excited because of the research possibilities and so forth. But he was also excited because infectious diseases was going to gain more status in the medical hierarchy. Would you comment?

Henderson: Infectious diseases physicians do not routinely perform procedures such as bronchoscopy or endoscopy. The only ‘scope’ for infectious diseases physicians is the microscope (and you really can’t bill for its use!). We do not catheterize anybody, or squirt dye into anything; in great measure, infectious diseases is an intellectual subspecialty, and it is a little dusty for some people. But a higher profile certainly has resulted from the HIV epidemic. I think under Dr. Fauci's leadership, the National Institute of Allergy and Infectious Diseases has really blossomed in this epidemic by doing wonderful work and he has led them unbelievably well through that time.

Rodrigues: I noticed this morning in the paper that the CDC finally seems to have a reasonable budget proposal for funding for their infectious disease surveillance system.

Henderson: Right. The article I read, it must have been the *New York Times* or one of the other big papers—I cannot remember where I saw it—quoted [Dr. James] Hughes, who was one of the people that I sat with developing those first guidelines at the CDC. He is now the director of the Center for Infectious Diseases in the CDC. He came from the Hospital Infections Branch, did HIV, and then has stepped up there. In effect, we have kind of gotten gray together.

Harden: We are hoping to go to Atlanta to talk with [Dr. James] Jim Curran. The CDC does not have such a program of interviewing people about their contributions to AIDS research. Dr. Curran has moved to Emory University now.

Henderson: Yes. He is the dean of the School of Public Health. He is a terrific guy. he has a wonderful sense of humor and he will be a great interview for you.

Harden: He seems to have been in the middle of everything. Everybody talks about having run into him.

Henderson: He was the lightning rod for AIDS and HIV infection at the CDC.



He and [Dr.] Harold Jaffe. Harold Jaffe is the other person you might want to speak with at CDC.

Harden: People have talked about AIDS turning into a "normal" disease instead of being something that people do not understand. We have now lived with it for over a decade and we are dealing with it as we deal with most diseases. Do you want to speculate on the future course of AIDS? Is it a harbinger for other emerging infections, and for what is coming in the world of infectious diseases?

Henderson: The major problem, I think, that we are dealing with poorly currently has to do more with how we are managing the infectious diseases that we already think we know how to manage. The problem of antibiotic resistance is going to be a huge hurdle. There was a wonderful paper in *Science* about two years ago that said that we were entering the post-antibiotic era, and I think that is significant. Vancomycin resistance in enterococci is a harbinger of things to come, and if that glycopeptide resistance finds its way into *Staphylococcus aureus*, we will have retreated successfully to 1950. We will be back to where hospitals may have to close, operating theaters may have to close, despite all of our intelligence and all that we have learned.

AIDS is slowly—even though I thought it would never happen—being better accepted in society. From my own view, the biggest problem will be to control the epidemic in Africa. The dimensions of the epidemic on the African continent are horrific, at best.

Initially, everything that one might imagine that was horrible seemed to be associated with this disease and really, I think, that is what polarized people. It has taken a long time for it to be accepted, and it still is not accepted perfectly in society.

Harden: Thank you so much for talking with us, Dr. Henderson.

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