

Curran, James W. 1998

Dr. James W. Curran Oral History 1998

This is an interview with Dr. James W. Curran, Dean of the Rollins School of Public Health at Emory University in Atlanta, Georgia, on 19 May 1998, about his involvement in the AIDS epidemic. The interviewers are Dr. Victoria Harden, Historian at the National Institutes of Health (NIH), and Dr. Caroline Hannaway, historical consultant.

Harden: Dr. Curran, we would like to begin with your background. Could you tell us a little about where you grew up, where you went to college, and where you went to medical school?

Curran: I was born in Michigan, went to the University of Notre Dame, and graduated in 1966 in pre-professional studies. Then I went to medical school at the University of Michigan and graduated in 1970. After internship, I joined the U.S. Public Health Service in the Center for Disease Control (CDC) as a commissioned officer to fulfill my military obligation. I was assigned to the University of Tennessee to study the complications of gonorrhea, pelvic infections, and other diseases in women. I spent two years there. Then I went on a career development program with the CDC to the Harvard School of Public Health and Harvard Medical School, where I got an MPH and finished a preventive medicine residency as a fellow in health services research.

Harden: Let us go back a little and ask you to expand on what influenced you to go into medicine. Was it your family?

Curran: No, my father was a small businessman. I was the only child. It was a combination of an interest in both science and the humanities. I was interested in public health, without really calling it public health, from the beginning. I was interested in issues related to reproductive health and issues related to world health. So I had a combination of interests in science and the humanities, which I thought was related to health.

Harden: Did you have any special professors or anybody else who guided you?

Curran: I went to a Jesuit high school, the University of Detroit High School. I think I would say that nobody in particular guided me, but in high school I had the same kind of dual interests nurtured in both humanities and logic and so on, and also science. So I entered Notre Dame interested in pre-med and was successful enough at it that I just stayed in that area. My family was very supportive of my going into this area, but not to the point of not wanting me to do something else. [Dr. Samuel] Sam Broder was a classmate of mine in medical school.

Hannaway: I was wondering about that when you mentioned the school.

Harden: I was going to observe that [Dr. Anthony] Tony Fauci also went to a Jesuit school and both of you speak very well "on your feet" in public. Can that be attributed to your education?

Curran: In my high school, we had speech for all four years. We had speech classes the first, second, third, and fourth year. In high school, that is pretty unusual.

Harden: Originally, you did a residency in obstetrics and gynecology. What made you shift towards public health from clinical practice?

Curran: I was interested in maternal and child health, and I was in a four-year residency program. The Vietnam War was going on, and all male physicians who graduated either had lottery deferment to finish their residency or could go in the military right away, and then you had the lottery, or a mechanism selecting which service. Or you could have what is called a corps deferment, or "Berry Plan" deferment, for the Public Health Service, NIH, or the CDC. Or you could go into the Public Health Service right away, depending upon certain selection criteria. I had missed my "Berry Plan" deferment. I had applied for a corps deferment for the Family Planning Service; but at the time the Family Planning Service was authorized but not appropriated. I did not know what that meant. I had this letter back from them saying, "We are not going to have any commissioned officers because we do not have any money."

Another colleague was named [Dr.] Jan Schneider—he is the chairman of Ob-Gyn at the Medical College of Pennsylvania, I think. He was a Brit. I had written a paper when I was a senior in high school on midwifery with him for something called the Victor Barnes Society at the University of Michigan, which is the historical medical society a few of us belonged to. We used to hang out at professors' houses. And I wrote this paper on the history of midwifery. So I was lamenting to him that I was going to go in the Air Force, which was okay, because my colleague William Ledger's brother-in-law or some other relative controlled who got to do Ob-Gyn and who did not. I thought, I will get some experience, but I really want to go into public health. Schneider suggested that I come down to the CDC, that there was a family planning unit there, very, very small at the time, headed by a man named [Dr.] Carl Tyler, who was the only Ob-Gyn person at the CDC.

I was not interested in infectious diseases in particular. But I came down; I was single and I had a week off. I came down but found that the EIS [Epidemic Intelligence Service] program was filled up. This did not bother me particularly, because I had never heard of it, anyway. When I interviewed, I gave them my resumé, which was pretty short—it was written on the back of an envelope. They sent it to what was then the VD [Venereal Disease] Control Division [of the CDC], and at the time the VD Control Division was a very small group. I think it was the VD Branch. It was a small group that dealt only with syphilis. But they were interested in getting into gonorrhea control, and, of course, all they had on their staff were dermatologists. Here was somebody who actually knew how to do a pelvic exam, and I knew a little about pelvic infections and I was inclined to think about things beyond syphilis. So I had some appeal for them, I guess.

They offered me an option of going into the Public Health Service instead of going into the Air Force. I was to go with the winner of an RFP [Request for Proposals] to study the medical and economic complications of gonorrhea. The University of Tennessee in Memphis got the award, so I found out that that was where I was going. It was either UCSF [University of California San Francisco] or Tennessee or Indiana or a couple of other places. Tennessee got the award, and I stuffed everything into the back of my car and came to Atlanta for six weeks of training to learn about some of these things. Then I went to Tennessee to set up shop and started—I think I was 26—to study the complications of gonorrhea, to figure out what the economic impact of these was, how common they were, and set up a laboratory. I saw lots and lots of patients, and I wrote 11 papers.

After about a year I decided I liked this and I did not need to be an Ob-Gyn specialist. I was more interested in studying health economics and epidemiology and things like that.

Harden: But you were still focused only on gonorrhea then. You were not seeing other sexually transmitted diseases?

Curran: I was focused on gonorrhea for the first two years.

Harden: When did the name change occur at the CDC from "venereal disease" branch to "sexually transmitted disease"?

Curran: Maybe 10 years later.

Harden: Was that because you were expanding the number of diseases you were investigating by that time?

Curran: The list was always larger than the "five traditional venereal diseases." But the branch, and then the division, went through several leadership changes. Through the time of [Dr.] Paul Weisner, who was the head of the division when AIDS came along, it was still called the Venereal Disease Control Division.

I think it was not until [Dr.] Ward Cates became the director that they changed the name to STD Control Division. As to why they did or did not do that earlier, I think that almost all of the money was for gonorrhea and syphilis, except for a small amount of research money. I believe Paul felt that there ought to be some federal resources for other STDs first before they changed the name. But certainly the STD name is more representative of the problem.

Hannaway: After you had been to Harvard and had done your work in the Department of Health Services Administration there, who were you working with then? Who were you working with then?

Curran: [Dr. Alan] Al Yerby. Do you know Al Yerby?

Hannaway: No, I must admit I do not.

Curran: He died, I think, last year. He was the chairman of the Department of Health Services Administration. He was an African-American who was a very prominent man. He had been Commissioner of Health someplace. I am not sure if it was Massachusetts. [Dr.] Julius Richmond was in his department.

Hannaway: Right. That is a name that I know.

Curran: And quite a few other people. But he was my advisor. I was actually a fellow at a place called the Harvard Center for Community Health and Medical Care, with [Drs.] Paul Denson and Rashi Fein and Isadore Altman. There were five of us who were fellows, which allowed us to do what we wanted to do, and [for me] to take additional classes after I got my MPH.

Hannaway: It must have been quite a transition from Tennessee to the Harvard School of Public Health?

Curran: Well, I also met my wife, and today is our 25th anniversary.

Harden: Congratulations!

Curran: That is how long ago this was. I met my wife in Tennessee. She had been born in Alabama and raised in Florida. She moved to Memphis. We got married in Tennessee just before we left, and moved to Boston for two years. She went back to school, finished college at Simmons [College], so we were both youngish students then. I was a lot younger. I did not feel that young. I have never felt young. I never thought of myself as young. Nor old!

Hannaway: When you made the transition back to working for the CDC, what did you do next?

Curran: When I came back, I took a job with the STD Control Division—it may have been called the VD Control Division. I do not remember what the title was then—in Columbus, Ohio. Why Columbus? I was wondering where my career was going to go. Now I had this additional training and had developed these great perspectives, and I was not sure if I wanted to go into academia, stay with the CDC, or go into state and local public health.

The thing that attracted me to this job was that the CDC--the STD control people in their research branch--had had commissioned officers in Columbus, Ohio, I think, for five years previously. Two of the people were colleagues of mine and friends, and the last one had set up a registry for gonorrhea. This was still in the early gonorrhea control days, and he had a huge gonorrhea screening program. But the CDC had the idea at that time of setting up a computerized VD-control registry. This was 20 years before Bill Gates.

It was called the VDCSPS, the "VD Control Strategic Planning System." There were two sites: Denver, Colorado, and Columbus, Ohio. Now, Denver had wonderful clinical facilities, but not much public health outreach. Columbus had terrible clinical facilities, and clinical care was pretty bad, but it had tremendous outreach programs: screening, health education. It was also in the state capital and had a friendly health department, but it did have bad clinical facilities.

My assessment of the situation was that I had an opportunity to make a difference. I had also noticed that at Columbus they did not have an assistant health commissioner for medical services and that the job had been vacant for 10 years since the current health commissioner moved up. The problem was that my predecessor, as a research assignee, was unable to get the administration to get things done. So, although this job was a research job with the CDC, I offered to be the assistant health commissioner, as a CDC assignee, which gave me control over the clinic, the laboratories, and also the neighborhood health centers. Then, of course, the swine flu program came along, so I had major involvement with the swine flu program. I had rabies outbreaks and all kinds of things to deal with, which was good experience. But I also was able to oversee the Hill-Burton Act remodeling of the clinic and to remove some of the old staff, bring in some new staff, modernize the facilities, and really make it an outstanding clinic. I was also an assistant professor at Ohio State University, so I had a chance to teach as well.

Hannaway: You were trying things on every front?

Curran: I stayed there for three years, and I really liked it. Then in 1978, I was asked by the CDC to come and be the chief of the research branch of the VD Division, or STD Division, whatever it was then called. So my wife and I moved from Columbus to Atlanta. At the time the research branch of the VD Control or STD Control Division had a portfolio of about \$3 million worth of studies. They were a combination of intramural-extramural studies. It is a little more blurred at the CDC than it is at the NIH. We had sites in Atlanta at the Grady Hospital and STD clinics, and a lot of our physicians worked in these clinics and did clinical studies. We also had contracts to do studies elsewhere.

Shortly after I arrived, our branch was debating what type of role it should play in hepatitis B vaccine studies. Actually, what was the Hepatitis Branch at the time was in Phoenix, Arizona, and a man named [Dr. James] Jim Maynard was the head of that, and [Dr. Donald] Don Francis was the assistant director. They were doing studies with [Dr. Stephen] Steve Hadler there, with a man named [Dr. William] Bill Darrow and with [Dr. Samuel] Sam Thompson, both of whom were at the CDC in Atlanta. Sam eventually came to Emory. They were doing studies on the incidence and prevalence of hepatitis B in five communities in preparation for a hepatitis B vaccine trial that was to follow close on the heels of [Dr.] Wolf Szmuness and [Dr.] Cladd Stevens' trial in New York. As those studies were wrapping up, since the studies were being done in STD clinics, and since our division had money and hepatitis really did not have any money, it was decided that the CDC needed to do this study, but it should not be done with Merck money. So my first decision, or maybe it was an acquiescence—I remember this decision—was to use our branch's extramural funds, \$2 million of it, to support the hepatitis B vaccine trials in these five STD clinics. I was not closely involved with the studies, but a couple of our staff were involved with the hepatitis people, particularly Bill Darrow and Sam Thompson, and I was at many meetings and was involved in a lot of the problem solving and administration of it.

That started in 1978-1979. [Dr.] Harold Jaffe then rejoined the CDC, and Harold was involved in this. It got us in a lot of conversations with many physicians in the gay community in each of these five cities, so that when the first cases of *Pneumocystis pneumonia* were reported to the Parasitic Disease Division—the Parasitic Disease Drug Distribution Group was the actual name of that group, I think. A woman named Sandra Ford was a clerk at the time who distributed the drug. The first *MMWR* [*Morbidity and Mortality Weekly Report*] report describing AIDS came across my desk as we worked with gay men.

Harden: Would you expand a little on when you first learned about these cases that came to be called AIDS? Was it when this *MMWR* article was published, or had you heard about it from other sources, and can you describe your initial reaction?

Curran: I first heard about it when the draft of the *MMWR* came out. Now, that article was published on June 5, 1981. In 1996, the CDC did a 15-year update on AIDS. They republished the *MMWR* article, and I wrote an editorial note for it. It was fun to write it. Whether it adds anything, I do not know. But I think the document itself has some historical value.

Hannaway: So you saw a draft of the original *MMWR* article on AIDS.

Curran: Yes, I saw the draft in May of 1981. These articles get out pretty quickly. I cannot remember whether there were four or five cases discussed in the draft. There were five that were published. I saw the draft, and it was at the time we used to have annual STD conferences.

That year we were having ones in San Diego and Atlanta. Between the time I saw the draft, and when it came out on June 5, we had our first conference, which I think was around May 30, June 1 or June 3 or something like that. Harold Jaffe and I went to the conference. We had both seen this draft. And we talked to some of our West Coast colleagues in the gay community and people who were at the meeting. I do not remember whether [Dr.] David Ostrow was there; but [Dr. Robert] Bob Bolan, who was a physician in San Francisco in the gay community, and some other people from the studies in San Francisco and L.A. were there. A few weeks later we had a meeting in Atlanta and we did the same thing. But, in our discussions in California, Harold and I learned that there were some other similar cases that had been seen by some of the physicians. They were not fatal. There were some unusual things going on in the gay community. It was more than the rampant enteric diseases and hepatitis and STDs in the gay community. This was something more than that. There actually was a case of Kaposi's sarcoma that somebody had seen in San Francisco, but nobody knew really what that was.

So, after the article came out on June 5, a task force was formed at the CDC. Now, the way this worked was, I am not sure who convened the meeting, but it was in the director's conference room. People from all parts of the CDC got together, and this was just a day or two after the *MMWR* was published. We had people from the fairly new cancer group; people from parasitic diseases; people from STDs; people from viral diseases; from the laboratory, senior scientists who were administrators, and [Dr.] John Bennett, [Dr. Walter] Walt Dowdle, [Dr. Michael] Mike Lane, people who were around then—[Dr.] Clark Heath was a cancer epidemiologist—they were the senior people. Then a lot of us were about a decade younger. Then there were even a few younger people. We talked about what this meant and what we thought it was.

Harden: Expand on that a little, would you?

Curran: The CDC had dealt with two other major epidemics in the preceding five years. One was Legionnaire's disease, and there was quite an experience around Legionnaire's disease in the CDC. Then, the other one was toxic shock syndrome. They both turned out to be bacterial disease problems. They started off not looking like bacterial disease problems, and they both involved mobilizing resources to act on a problem and trying to coordinate resources. The CDC was interested in how to do that better. The CDC loves this kind of problem as an institution. It is one of the things the CDC does best, this kind of rapid mobilization and response to public health emergencies, and that is what the CDC is known for. It does not necessarily do the long-term study best, but it does the short-term response well. So, initially, it was thought that these unusual cases might be the next toxic shock syndrome or Legionnaire's disease. Everybody got together to talk about what was going on from different perspectives and to speculate about what the cause could be. There was not a lot of unique insight. It was suggested that we continue to investigate. We needed to find out more. We had a number of calls from different people, and we had to follow up on those calls; find out what was going on, call some other people we knew, and see if this is a common problem elsewhere, and just do a lot of phone work initially.

Harden: So all your personal contacts across the country became very important.

Curran: Yes. We took a lot of different approaches. We agreed to form a task force, and all of the elder generation stepped out and said, "Call us if you need us. Keep us informed." Different centers in the CDC were involved. We formed a Task Force on Kaposi's Sarcoma and Opportunistic Infections, initially comprised of people my age and of those a couple of years younger.

At the first meeting, the group elected me as the moderator or chairman of the task force. First of all, at the very first meeting, there had been a call about some cases in New York. The man who was in charge of the research in the Parasitic Disease Division, who had supervised the people putting together the first article on *Pneumocystis*, was scheduled to go to New York.

The group decided that I should go with him because he was a veterinarian. They thought, "You ought to send a physician, too." I did not know anything about *Pneumocystis*, but it did not matter. There were also patients with Kaposi's sarcoma, which, of course, I knew nothing about either. So three days after the *MMWR* article came out, [Dr.] Dennis Juranek and I flew to New York to visit [Dr.] Alvin Friedman-Kien and [Dr.] Linda Laubenstein at New York University. They had a patient who had Kaposi's sarcoma, an actor from Detroit. He graduated from a Catholic high school, Royal Oak, in 1962, not too far from my high school. I went to Notre Dame and he went to Yale. The differences between him and me were not great, but he was gay and I was straight. He was in New York, he was an actor, and he had these funny purple lesions all over. None of us knew what the hell they were. They did not look very serious. We laughed about it and talked about it. We also tried to act intelligent, because none of us had ever seen a case of this. And Friedman-Kien had only seen a couple of cases. He was a professor of dermatology. Laubenstein and one of her fellows, a man named [Dr.] Kenneth Hymes, had written a paper on *Pneumocystis* and they submitted it to *Lancet* at the time, and they shared a draft of that with us.

Some of these people with *Pneumocystis* also had Kaposi's sarcoma. That was the first time that I really felt that these things must be related, because the patients were the same, but the diseases were quite different. That gave us, I think, one of the most important fundamental clues that this was a similar epidemic with an underlying problem.

Subsequently, over the next year I made about 40 more trips to New York, and I saw this man deteriorate over time. He was treated with chemotherapy and things that probably did not help him any, and I eventually watched him die. This was all before the virus was discovered. I realized that there was a certain unfairness to this, because our paths in life were not separated by much, but he died while I lived. But I am getting ahead of myself.

Hannaway: One of the questions that we were wanted to ask was about the CDC's experience with swine flu—some people have complained that it had an effect on the way the CDC responded to AIDS. What are your thoughts on that?

Curran: Well, swine flu and Legionnaire's disease came at the same time.

Hannaway: The claim basically is that the CDC overreacted to swine flu, and reacted more appropriately, let us say—and this was in some of the literature—to Legionnaire's disease. The literature says that the CDC overreacted in advocating that a more dramatic vaccine program be developed and so on. Did this ever come up in your discussions relating to AIDS? Did anybody ever say, "Oh dear, we overdid it with swine flu. Maybe we shouldn't be so vigorous"?

Curran: I mentioned the two things that I thought were more relevant, and that was Legionnaire's disease and toxic shock. We were in the mode of investigating, not implementing. The swine flu vaccine initiative was a program of protection, which is more like the national immunization program or polio eradication or smallpox eradication. When swine flu came along, it was a calculated-risk issue. Gerald Ford was president, [Dr.] David Sencer was head of the CDC, and Jimmy Carter became president shortly afterwards. Carter and [Joseph] Califano fired David Sencer as director of the CDC and appointed [Dr. William] Bill Foege, currently one of our faculty members at Emory, to be director of the CDC. Foege was director when AIDS came along.

More importantly from a political standpoint, I think, was the advent of the Reagan Administration and the reduction in force in the Public Health Service; the closing of Public Health Service hospitals; and the initial threat both to the NIH and the CDC, probably more to the CDC, of a reduction in budget and reduction in force. A sort of a pall settled over the government when Reagan first came in. It affected many of my initial interactions, which were with the National Cancer Institute and [Dr. William] Bill DeWys and his boss. Was that [Dr.] Bruce Chabner?

Hannaway: Bruce Chabner, yes.

Harden: We are not as familiar with the administrative structure of the CDC as we are with that of the NIH, and so you may have to set us straight on this from time to time.

Curran: The center is like an institute.

Harden: But you then have preventative programs that the swine flu episode would have fallen under, and that is a different kind of administrative initiative from the response to diseases like Legionnaire's.

Curran: A lot of the parts of the CDC are scientific parts, but there are also preventative parts. A center may have both. For example, the National Center of Chronic Disease Prevention and Health Promotion has cancer prevention programs, but it also has nutrition research groups, cancer research groups. In general, the CDC is more an institution of applied research groups.

For the infectious diseases, at that time there were really two centers. There was the Center for Prevention Services and also a Center for Infectious Diseases, where laboratory research and a lot of the epidemiological research was done. It was not all cut and dried, though, because STD research and tuberculosis research and immunization research were in the Center for Prevention Services. But the laboratories were in the other center. So it does not all make a lot of sense, and it still does not.

Every July 1, a new bolus of young people come into the CDC as epidemic intelligence officers, and they are really anxious to go and solve the world's problems. They are very smart young people. They are like the new people that come to the NIH. They are usually physicians, but they are young physicians who come in for two years for training, and then they leave. Some of them stay.

Hannaway: Idealistic and dedicated.

Curran: Yes. They wanted to go jump on AIDS. Everybody wanted to work on this problem, but the Reagan administration was cutting back support. We could not get any staff support or secretaries or computers. But every EIS officer wanted to work with us, so we had that resource available.

Now some of them had actually seen cases. There was one who had been a fellow with [Dr.] Henry Masur and others in New York. And it was said that the people in New York said, "If you go to the CDC, do not tell them about this problem because we do not want the CDC to scoop us on this." So we did not learn about the New York cases, even though requests from New York for pentamidine had come in earlier. Apparently, this clerk could not get the people in New York to tell her any details about it. So she went to California instead, and people in California provided the information. The first cases were from New York, though.

The first thing we did after we organized the task force was to gather some EIS officers to work on AIDS. Some of the original EIS officers were [Dr.] Martha Rogers, who is still at the CDC, and [Dr.] Harry Haverkos, who had since been at the NIH and the FDA places. He had been a fellow in Pittsburgh and had seen a patient with this syndrome, with *Pneumocystis*, and he came to the CDC because he wanted to study *Pneumocystis* because he had run into this patient. When he came to Atlanta, it happened that the patient was transferred to Emory.

There was also Ida [?], who is now with TB, but at that time she was the fellow with Masur in New York. So we had a few people who had actually seen patients as new EIS officers, and they joined our task force. We had a cancer EIS officer join the task force. Martha was in Viral Diseases and Harry was in Parasitic Diseases. They were assigned to me as task force coordinator.

Also, over the next few months, a few of my branch people were reassigned to the task force. I was assigned permanently for three months to head this.

Hannaway: For three months. That was optimistic.

Curran: Yes, three months became fifteen years! The task force initially operated out of the Center director's office, and then it operated out of the CDC director's office; so we would get more visibility, and hopefully more resources.

The first thing we did was to go back to the source of the information; that is, go back to the pentamidine requests. Pentamidine isethionate was an orphan drug before they used the term. It was a drug that was made by a group called May & Baker of England. I used to picture them stirring a big vat, because they never wanted to get licensed in the United States. It was too much of a hassle, and the drug was only used maybe 100 times a year at most, maybe not even that. As a matter of fact, there was a professor at the University of Cincinnati named [Dr.] Peter Walzer, who, when he was at the CDC, had conveniently written a paper reviewing the pentamidine requests from 1969 to 1978, and he found only one case in which pentamidine was used in somebody without an underlying cancer or severe immunosuppression. That gave us a background of nothing happening for nine years.

So we started following up systematically every one of the recent cases in which pentamidine had been requested. We found a few more in New York, a couple more in California, and four in Atlanta. That was it for the whole country. And they were all gay men. Except for a couple of additional cases in New York among men, supposedly drug users, but we were not sure they were gay.

Then we sent EIS officers who had finished their training to each of the states where there were cases, in the 18 largest metropolitan areas in the country.

We had prepared a case definition of this new syndrome. It was defined as these life-threatening opportunistic infections or Kaposi's sarcoma in people with no case of underlying immunosuppression. That was the bottom line. We had the EIS officers go into the university hospitals and academic centers in each of these 18 metropolitan areas and review all of the pathology records—at the time, *Pneumocystis* was diagnosed with a lung biopsy—or medical records or tumor specimens for any of these cancers or opportunistic infections back to 1975, and there were none except for the ones that we already knew about, and maybe a couple of others in California and New York. Then we kept on doing active surveillance of all the calls that were coming in. Friedman-Kien called in and said that he had 20 cases of Kaposi's sarcoma, and we tracked down all of those around the country. There did not turn out to be that many new calls, but there were a few, and a lot of them had been seen at Sloan Kettering by a famous doctor named [Dr.] Bijan Safai, who was a skin cancer doctor there.

We then needed to verify these diagnoses. We sent a lot of Kaposi's sarcoma specimens to a dermatologic pathologist in New York, Dr. Bernard Ackerman, to make sure that the diagnosis was accurate. We were trying to piece together answers to the questions: is this new, is it increasing, who has it, who does not? The most important thing for our case definition was that it was specific. It was not so important that it be sensitive; it was important that it be specific, because when you are determining a new problem, you have to make sure that you do not over diagnose it. If you think about some of the obscure syndromes that we have trouble with nowadays, like chronic fatigue syndrome, part of the trouble is that it sounds like everybody has it. Then you try to define it and it is very difficult.

The first connections with the National Institutes of Health came through the National Cancer Institute. Alvin Friedman-Kien was a dermatologist who had discharged his military obligation as a Clinical Associate at the National Cancer Institute in the Cancer Virology Lab, and he had some friends there whom he contacted.

Now, at the time, the NIH was not the only place that was having trouble with the concept of an epidemic of cancer and an epidemic of infectious diseases that was the same epidemic. But the NIH was included in that group. NCI had a little more insight than the other institutes did—I do not know why they did, but they did. It may have been that Bill DeWys and Bruce Chabner had experience in the Uganda Cancer Institute with [Dr.] Greg Donaldson, and that people that were still around from the old Kaposi's sarcoma study days, when they studied epidemic Kaposi's sarcoma in Uganda and were very interested in the viral etiology of Kaposi's sarcoma.

They were aware of the literature that showed that this was a cancer that also occurred in transplant recipients. As a matter of fact, it was hundreds of times more common in people with kidney transplants than others; and if you took away the immunosuppressants from the kidney transplant patients, the cancer would go away. So it was a pretty perfect setup. Here you have a cancer associated with immunosuppression and you have opportunistic infections that potentially occurred because of immunosuppression.

NCI also had a lot of money invested and a lot of interest in the viral etiology of cancer, and it was the biggest institute. So, in retrospect, NCI had several reasons why they were interested in the strange outbreak. Also, Alvin Friedman-Kien was driving them crazy because he was calling them. So NCI called me—I am trying to remember—it was sometime in the fall when I met with Bill DeWys. Bill DeWys had been a senior resident when I was a medical student at Ann Arbor. I have notes on all this.

I did not have any money to travel to D.C., but they wanted me to come up and talk with them. Eventually, NCI planned a workshop. The CDC was also a sponsor, but I do not think we paid any money. We did not have any money. But we helped them get it together and organize it. I spoke at it. It was a small workshop, but it was a start. They got recommendations from that to take to the Division of Cancer Treatment's Board of Scientific Counselors to request seed money to do some studies.

Harden: Would you talk some more about the workshop itself? Whom do you remember being there? What do you remember? What were the major points in your talk?

Curran: I do not remember exactly. I am sure I presented whatever data we had under surveillance, some fairly crude makeshift slides, with the latest numbers that we had, because we were keeping active surveillance of the problems here in Atlanta. I would have described the cases as a single problem. And it was intentional that we called our task force, as awkward as it sounds, Kaposi's Sarcoma and Opportunistic Infections, because we wanted people to understand that this was one problem. We were committed to that concept; and we would present our data by the number of cases of Kaposi's sarcoma, the number of cases of opportunistic infections, and the number of cases of both. In every case, they paralleled each other. They were all going up; they were all of the same age groups; they were all in the same places. So that this was really one epidemic. I would have presented that.

I would have presented some preliminary information and said that our next step was to find out what we could do. We were still in the Legionnaire's disease/toxic shock mode, thinking like the CDC group that found the Legionnaire's organism and found a way to treat it. Or the group that found that super-absorbent tampons can lead to toxic shock syndrome. Once the cause had been identified, we could demonstrate how to intervene. What we like to do, of course, is stop the problem.

So we were into trying to find out if there was something easy to do. In order to do that, we wanted to talk to as many living patients as we could. We devised a questionnaire, 30 pages or something, and flew out to talk to every living patient that we could find.

Harden: Do you have a copy of that questionnaire somewhere?

Curran: Harold Jaffe might.

Harden: We would like to have a copy of that original questionnaire.

Curran: We flew all around the country, and when we were finished, all we could really say was that the average age was 35, so they were not spring chickens. Also all of the patients were gay men, and they were living in areas of either high opportunity or exile, depending on your point of view. They were living in strong gay communities: San Francisco, New York, and Los Angeles. The other thing was that the men were all openly gay. That was one of the things that struck me. There were no closeted gay men. My previous experience with sexually transmitted diseases in the gay population was different from that. Here was a spectrum not only of bisexuality and homosexuality, but even more important, of openness. Most of these people were living in openly gay communities, in an openly gay lifestyle. They seemed to have large numbers of partners, and they were getting a rare infection. There were also other things associated. They all went to the same clubs. Most of them used poppers or isobutyl nitrite or amyl nitrite, and they may have been exposed to other things in their environment.

But, from the very beginning, since we came from an STD background and had been studying hepatitis B, this really looked like a hepatitis B pattern. I think that our top hypothesis was always that. But that was not going to be an easy one. I mean, we wanted to make sure that it was not something easier. Had it been something like isobutyl nitrite, for example, it would not have taken us very long to get rid of that as a risk. That would have been a much more pleasant outcome.

Harden: By the time of the symposium that the Cancer Institute sponsored—in September 1981—had you seen any cases in women or children or any other groups at that point?

Curran: The cases of drug users were not well documented in terms of risk. I think it was December when the articles in the *New England Journal of Medicine* came out, by Masur and Siegel and Gottlieb. Masur's patients were minority men. I think that they were all men. They were drug users, but they were all dead by the time of the report. People were saying, "Well, maybe these are really gay men that are selling themselves to get drugs or maybe they had similar drug exposures." That did not seem too plausible, but Siegel, from the same city, had cases in gay men, so maybe they were. It was hard to prove that it was occurring in other groups initially. That was by late fall.

Harden: So it was after that September symposium that you were moving out with your questionnaire and getting more information.

Curran: Yes. We did a case-control study in October. In that study, we restricted it to gay men; so there were patients by then, I think, that were not gay men, in October. The controls were matched for city of residence at the time of the onset of illness; and they came from STD clinics or private physicians' offices.

Harden: Were you in charge of this study?

Curran: I was in charge of the New York City group, and Jaffe was in charge of the California group. We had maybe one or two cases in Atlanta. We interviewed 50 cases, 203 controls or something. Then we used the standardized questionnaire and the standardized specimens, and the papers from those results were eventually published in *Annals of Internal Medicine* in about 1983. We started the study in October of 1981. And we interviewed about 90 percent of the living cases in the country.

Hannaway: Wow!

Curran: We did another study—it was a cluster analysis—that began in 1982, started with some gay men who reported to [Dr.] David Auerbach. Do you know who I am talking about?

Hannaway: Yes.

Curran: They reported that there was a gay fundraiser in the late 1970s at which a man had reported that his lover had died. From his table at the fundraiser—there were maybe about 1,000 people at the party—three or four of the people had all died. It turned out, to make a short story long, Auerbach contacted Bill Darrow, who interviewed all the living cases in the L.A. mega-metro area, of which there were nine. Nine of the first 13 cases, whether living or dead—I am sorry—were linked to each other. Eventually they linked up to 90 other people around the country through sexual contact, which was 40 percent of the first 220 gay men reported with AIDS in the United States. This is where this patient zero fits into this epidemic. By doing personal interviews around the country, they were able to link together the network of sexual contacts. This was in 1982.

The other most important thing was the cases in hemophiliacs. We were sitting at a little task force meeting at one of our animal facilities converted to offices at the CDC, and a man named [Dr.] Bruce Evatt, who was the head of our hematology group then, was in the task force. He said, "There's an old man from Florida who had *Pneumocystis* that they called me about, who died." Maybe he died in New York, but he was from Florida, the Florida-New York connection. The man died of *Pneumocystis*. I said, "What?" We were all thinking blood—it was going to be in the blood supply because it was hepatitis-like, when we were thinking it was an infectious disease. But not everybody wanted to believe this. There were a lot of other hypotheses. So we said, "We really have to watch out for this."

Bruce was the head of the hemophilia branch from Georgia; he was hooked up to the hemophilia community. Sure enough, a couple of weeks later, we had the report of a case in a person with hemophilia from Colorado of *Pneumocystis*. We sent a man named [Dr.] Dale Lawrence, who is currently an NIH scientist, to investigate. I think he is in the Division of AIDS, Vaccine Group.

Dale was a post-EIS officer, an extremely compulsive young guy. He went out and spent two weeks with this family and with this man and reviewed everything he could possibly ever know about his personal and medical history, almost until he died. He took down every lot number of concentrate the man had ever received and concluded that there was no way this man was gay. I mean, there were no other cases in Colorado. This must be related. Maybe he had an underlying tumor we did not know about.

This man was the second hemophiliac to die of *pneumocystis*. So we went to Bill Foege and said, "We've got these two cases. It's getting real suspicious." We all agreed that if it happened twice, it was going to happen three times, and there would have to be two living cases before we could prove to people there was a pattern, because the first man who died was an old man. Who knows? He might have been gay, even though he was a grandfather.

Then another case was reported in a young boy from Ohio, and he had never been anywhere. I think he had never been out of the state. So Dale Lawrence went up there and spent two weeks with him.

In the summer of 1982, at a symposium at Mt. Sinai Hospital, [Dr. Herbert] Herb Selikoff invited [Dr.] David Rall, the director of NIEHS at the time, to discuss AIDS. NIEHS, true to its own perspective, believed that AIDS was somehow related to the environment. Everyone was touting his own special hypothesis. The director of the CDC, Bill Foege, was going to talk about the infectious disease hypothesis. Somebody else was going to talk about poppers, somebody was going to talk about autologous sperm, somebody was going to talk about the environment.

Foege went first, and he announced, "In this week's *MMWR*, there will be an article describing three AIDS patients with hemophilia, and we think this makes it look very much like it is a virus that is transmitted in the blood, in concentrates, because hepatitis is transmitted in these." All of the other speakers changed their talks to co-factors.

One of the things that I was doing for the meeting, in addition to writing Foege's talk, was trying to get people together for the next meeting and trying to make sure that the gay community and the blood bankers and others were all on the same page, that we did not end up with a lot of cacophony. We did not want the gay community coming out and denying this or volunteering to donate blood or anything like that. So I was trying to get leaders in the gay community, nationally and in New York, to have representatives come to the AIDS meeting—it was not called AIDS then, but it would soon be called AIDS—where they could meet with the scientific community and the blood bankers to make a concerted effort to deal with the problem.

Hannaway: The Hemophilia Foundation people and so on.

Harden: This must have taken a lot of diplomatic skills, given everybody's particular interests.

Curran: Yes.

Harden: Do you want to talk more about that?

Curran: I think that probably the most historically important stuff that CDC did were things up to the March 1983 recommendations. I mean, I think the period from June of 1981 to March of 1983 was when the CDC made its most difficult, but also most productive and important contributions.

I am particularly proud that we were able to find things out as well as we could. I think we set an international example for surveillance, for moving the envelope forward, and for getting everybody on the same page. The scientists really started to believe this was a virus when the hemophiliac cases became known, and they started to be interested in searching for a virus at that point.

Hannaway: So it was the CDC's skills in epidemiology and...

Curran: Yes, but also in convincing and communicating to people and getting people together, especially scientists, blood bankers, and the gay community. These were a lot of unnatural partners. It was not just cancer and infectious disease. AIDS for a time was identified with the gay community, which did not have a lot of reason to trust many people in government, especially as this was still a relatively small problem in the larger picture. There were a total of 1,000 cases by early 1983. That is not a lot when you think about it. There were 15 or 20 states that did not have any cases yet.

Harden: The problems in the gay community have been fairly well documented and talked about in a lot of literature. On the blood-banking issue, the question of the safety of the blood supply, there has been a lot of criticism of the blood bankers. But we have talked with people like [Dr.] Harvey Klein—who heads the NIH Blood Bank—about what happens when blood bankers inform the public that the blood supply may be in danger. Dr. Klein said that the blood bankers did not want to be irresponsible and panic people unnecessarily. You, on the other hand, were sitting here believing, after seeing the data on the hemophiliac cases, that there possibly was a virus in the blood supply. How do you decide at what point it is worth panicking people if necessary, to safeguard the blood supply?

Curran: The answer to how fast to do it is somewhere between how fast we do it now and how fast we did it then. In the pre-AIDS era, we accepted enormously high rates of hepatitis in the blood supply. For hemophiliacs, it was universal that they would get hepatitis. The concentrates themselves were not heat-treated or inactivated, and the products were pooled from thousands of donors. It was just expected that one of the benefits of getting these concentrates, which were saving a lot of lives, was balanced by the risk of hepatitis. So there were people going along with the safety paradigm as not being as important as the benefit paradigm. And a concern that what is a few more new infections when, after all, we have hundreds of thousands of cases of hepatitis. I mean, let us put this in some perspective. Now, they would not get up and say that, but 5 percent of the people who got transfusions get hepatitis. In those millions of transfusions, you were talking about two or three cases of *Pneumocystis*.

The other part of it was that the people who were from blood banks were pathologists and clinical pathologists for the most part. They were not infectious-disease people. They did not receive funding from the CDC. They received more funding from the NIH, and even then, the funding came not from NIAID or NCI, it was from NHLBI [National Heart, Lung, and Blood Institute], whose concern was not focused on infectious diseases.

Bruce Evatt and, occasionally, I would go to blood-banking meetings. We would give our presentation, and then we would leave. Then the blood bankers would talk to each other about how the suggestion that the blood supply might be infected could not possibly be true. They would bring in NHLBI people, and they would talk to them, and, of course, they did not believe it either. They said, "Oh, we've seen this forever, this *Pneumocystis* stuff." So that was just the way it was. Now, were they wrong? They were wrong, in retrospect. Of course, they were wrong. Is it understandable? Yes, sure. They are portrayed, 10 years later, as being self-protective, evil people who were killing other people, and I think that is overstated.

Harden: Could you understand where they were coming from when you were trying to persuade them about the possibility of transmitting AIDS in the blood supply?

Curran: Yes, but they would say, "Who are these people from CDC? They have STD backgrounds. Why are they talking about blood banking issues?" There were, of course, some people who had backgrounds in both infectious diseases and blood banking. [Dr.] Lou Barker in the Red Cross had an infectious-disease background from his time in the FDA, before he went to the Red Cross. They tended to believe in the infectious hypothesis more than some of the people in some of the other blood banks. There were certainly people in other NIH institutes who were believers and who were trying to convince NHLBI people initially. And, eventually, NHLBI really got very much involved in AIDS. [Dr.] June Osborne became head of their advisory panel and it became more infectious-disease oriented. But AIDS forced a difficult marriage among all of these different groups of people who had previously not worked together. And, naturally, there was no particular reason why they should have.

Certainly, if you look at the blood supply now, just like if you look at the dental office now, AIDS has made a revolutionary change in how things are done. At some cost. But the paradigms changed completely.

We were dealing with this issue of the safety of healthcare workers. We talked about dentists and dental operators wearing gloves, watching out for needle sticks, and wearing masks. They said, "Well, how can they do that? Thirty percent of the dental school professors do not wear gloves."

Harden: They do now.

Curran: Everybody does. You have to. So it was just a different way of doing things that had to be changed. It was among some of the revolutionary things that happened as a result of the AIDS epidemic.

Harden: One of the things that I am interested in here is any cooperation that may have gone on between the CDC and the NIH in setting some of these standards. I know the Dental Institute made recommendations early on for how dentists could protect themselves. And we talked to [Dr.] David Henderson about the hospital epidemiology at the Clinical Center; and everybody was looking to the CDC for case definitions and so on. What kind of interactions were there?

Curran: The first recommendations that the CDC came out with were in, I think, November 1982, *Guidelines for Healthcare Workers*. This was before there was a virus. I do not remember—I could find them if you do not have them—what the NIH involvement was in that. I would be surprised if it was not shared with NIH, but I am not sure whether it was co-authored by NIH or what. But this is the kind of thing the CDC had done a lot of before. It was like the hepatitis guidelines, things that fit that paradigm.

Hannaway: You were very experienced at developing guidelines.

Curran: Yes, particularly hepatitis, viral disease people. Then, in March of 1983, the interagency guidelines that were true guidelines came from CDC, FDA, and NIH, and maybe another agency, but I think just those three. Those guidelines were developed following a meeting that we had here in January. It was a Public Health Service meeting, but it was at the CDC with all the blood bankers and a variety of people. It was a very open public meeting. There are a lot of portrayals of that meeting in the literature.

But following that meeting, we drafted guidelines that were co-authored by all of the agencies. Actually, some of them came out after the blood-banking guidelines, the Red Cross guidelines, and the gay community guidelines, but they were all pretty much the same. We were all openly sharing drafts with each other because we were not trying to be secretive, and we thought consensus was pretty important.

You know that the period of time between the identification of hemophiliacs as people who could get AIDS and those guidelines was a period of nine months. During that time there were the first cases of transfusion AIDS. We knew of a few cases of AIDS who had received transfusions; it looked like transfusion-associated *Pneumocystis* in New York City. But we could not get permission from the New York blood center or others to investigate the donors, so we did not know if they were linked to a donor. But we knew there were men, for example, who had bypass surgery, who had *Pneumocystis*. And we went to interview them. We got their blood, but we did not find any connection. Men who had bypass surgery got lots of units of blood, they were users of large numbers of units of blood. So it was a rare problem. You expected people who received a lot of blood to get it. Interestingly, a lot of them had acute retroviral syndrome, which we noticed right after the transfusion. But we could not get permission to investigate the donors in New York.

So the first case was a case in San Francisco—[Dr. Arthur] Art Ammann reported it to us—a baby who had AIDS, had *Pneumocystis*, had had a blood donor who was a closeted gay man, who had subsequently died of AIDS. So that was a direct link, and that was the first transfusion case reported. It came out, I think, in January 1983 maybe, in *MMWR*. That kind of blew things open for us to do the other investigations. It turned out when we did investigations of some additional cases—an article would be published in the *New England Journal of Medicine* in 1984—that we found, not too surprisingly, that there was usually one gay man as a blood donor with a reverse T4-T8 ratio in each case. Then, subsequently, after the virus was isolated, we actually isolated the virus from each of those donors, and, I guess, now we could genetically link them if we had both isolates.

Harden: I have two questions here. You could not find out about the donors in New York. Was that due to state law, or the Red Cross?

Curran: They were hiding behind the privacy-of-the-donor issue.

Harden: Yes, and that is another other sort of issue.

Curran: It was not the Red Cross in New York. It was the New York Blood Center. The question then became, what could we do? I mean, should we try to invoke powers to do it? At the time, we still had more cases of hemophiliacs being reported. We were kind of looking for a softer niche to do the investigation because we were trying to cooperate with people at the same time, and there were other cases getting reported in other places. Once you investigate the donors, what do you find? You did not find a virus, there was still no virus. So you find a gay man who is a donor. But, I mean, we were trying to establish the pattern. And these cases were remarkable, I will tell you that. Do you like anecdotes?

Harden: Yes.

Curran: The second case of AIDS in Tennessee was reported. We had EIS officers always poring into this thing. I was motivating them and getting out in the field with these officers. There was a guy named [Dr. Steven] Steve Solomon, who was not entirely convinced that this could be transmitted through transfusion. See, we still did not have a virus. And a lot of us were zealots, believing this. There was this case of *Pneumocystis* reported in a young college student from Memphis State in Memphis, Tennessee. And that, of course, was where I got my start. Right?

Hannaway: Yes.

Curran: Here we were 12 years later, so I knew Memphis. And we got to do something there. There was a man, the state health epidemiologist, who wanted to help us with this. This was the second case of AIDS in Tennessee.

The student died almost immediately of *Pneumocystis*. To be exact, we went and interviewed him and got his blood. Then he died. He was in the ICU. Steve Solomon went to look into it. This young man had received blood from two donors. We interviewed the young man's family and people he went to college with. He was a shy young man, and he had been to New York once, and they could not say for certain that he did not have homosexual contact. And he came from a small farm town in Tennessee. So we found those two donors who had given him blood. One of the donors was in this major blood bank, and he was a prison guard. We rushed people to get his blood. He was a married prison guard, and it was a prison in Tennessee. There were two cases in Tennessee anyway, but, still, it was a prison. So we got his blood, and his T-cell subsets were normal, and he was healthy. He was married and he had three kids, and it looked like he was not the source of the student's infection. There was one other donor, but that blood bank was a paid blood bank and had gone out of business—and the records were gone. It turned out, however, that somebody found out we were looking for the records, and eventually we were introduced to the man who had bought the blood bank. He was mad at the man who sold it to him because he considered the business a "pig in a poke," but he had all the records in his basement. So Steve Solomon went down in this guy's basement and found the records of the blood donor. The man was supposedly from a military base in northern Tennessee. Well, Solomon could not find him. So he went up to northern Tennessee just south of the Kentucky border looking for him. It turned out that a woman, who had been a preventative medicine resident of mine when I was at Ohio, was running the health services for this little military base. This blood donor had been discharged from this base, given an honorable discharge, because of his health. He had health problems. What he had was extensive lymphadenopathy syndrome, which had been diagnosed in L.A., or San Diego or someplace, when he was assigned there in California back in the late 1970s. He was a single man. Where the hell was the guy?

We tried to find him. Solomon tracked all over Tennessee looking for this man. He found his mother. His mother said, "If you find him, let me know where he is. He's been drinking a lot, and we do not know where he is, and we're worried about his health. He is somewhere in this small town." Steve said, "Do you have any gay bars?" They said, "Not really, but sometimes we think there are some gay people that hang out outside of Bill's or Joe's." Solomon went and hung out there, and found this man. He did a physical examination of him; and he had all kinds of lymphadenopathy. Solomon drew his blood, and results showed an inverted T-cell T4/T8 ratio.

Solomon came back as a total believer: "This thing is transmitted through the blood. This is too much of a coincidence. This man was a gay man who was discharged from the military for lymphadenopathy syndrome, started in California, comes back, donates blood to a blood bank that goes broke in Memphis. The blood goes to this college student who gets *Pneumocystis* and dies."

I worked a case myself in Huntsville, Alabama, at about the same time. There were no reported cases of AIDS in northern Alabama. It was a case of AIDS in an old man. After doing a few of these kinds of investigations yourself, tracking the cases down, you understand how people became convinced of the infectious hypothesis.

The blood-banking community was concerned about the integrity of the blood supply. They were concerned about putting it in perspective. They did not realize that the problem itself would change the perspective, and that it was much more common than it was.

Harden: Let me go back to Dr. Robert Gallo and ask you to talk a little more about your relationship with him. Here you had all these cases, and you were thinking it sounded very much like an infectious disease, and you are looking at these T4/T8 ratios. You know Bob Gallo, and you know he has been working on T4 cells. Would you go through it for me? How well did you know him?

Curran: I did not know him at all until, I think, it was the National Cancer Advisory Board meeting.

Harden: This was in December of 1982?

Curran: It might have been before that. One time I met him was at the Board of Scientific Counselors for the Division of Cancer Treatment. The next time was the National Cancer Advisory Board.

Harden: Right.

Curran: I think it was the National Cancer Advisory Board, because it would be the latter meeting that [Dr. Vincent] DeVita was chairing. The two presentations for the afternoon were going to be one by me and a presentation by Bob Gallo about his work on retroviruses. This meetings, as they often do, ran late. Since I cannot remember which of the meetings it was, I do not know if it was DeVita or Chabner chairing, but let us say it was DeVita. Gallo was waiting and I was waiting, and they got to the point where they were saying, "We only have time for one more presentation." Now, NCI had paid my way up there. The NCI people probably thought, "Well, Gallo can present next quarter or next meeting." So they said, "Bob, we will ask you to wait. Let this other guy get up and talk."

So I got up and I dedicated my talk to him. I said, "You know, since you have worked on T4-trophic viruses, what you need to do is find this one." I said that in front of the National Cancer Advisory Board. And I presented this data, which I am sure was obscure to many people on the National Cancer Advisory Board. But Gallo listened, and he is not a patient man. He was not one then when he was younger. And he listened, and that is probably why he occasionally attributes his involvement to me. I became one of his group of advisors and was communicating with him over time. He pulled a group of people together which included [Dr. Dani] Bolognesi and people like that, but also Dr. Luc Montagnier and others to get together to brainstorm over what to do, how to find the cause.

Harden: Were you traveling back and forth to Washington a lot? Were you in and out of the NIH and presenting seminars?

Curran: I would say that the first year I spent more time in New York than I did in Washington. Then, starting with the reports about hemophiliacs, I began spending more time in Washington. After the virus was discovered, by 1984-85, I was spending much more time in Washington than anywhere else. But by then we had a larger team of people, and we had funding and divisions.

Harden: I was just trying to pin down whether your interactions with NIH people were more formal or informal in 1982.

Curran: I would say more informal. The formal mechanisms did not start until there were PHS task forces and things like that.

Harden: Nineteen eighty-four, I think.

Curran: Yes, or even later

Hannaway: Was Bob Gallo interacting with CDC virologists as well, or was that a separate enterprise?

Curran: There was a lot of friction between Gallo and Don Francis and many of the people at CDC, and it involved a lot of issues. We were doing everything we could to discover the virus. And we were interested in retroviruses. At NIAID at the time, although [Dr. Malcolm] Mal Martin was interested in retroviruses, the leadership of NIAID was [Dr. Richard] Dick Krause and Dr. Kenneth] Ken Sell, and they were interested in anything other than retroviruses.

Harden: Why were they not interested in retroviruses? Because that was the Cancer Institute's investigation?

Curran: That is speculation. I think that NIAID figured that those were not their viruses, except for Mal Martin. We would go to meetings--I remember [Dr.] Albert Sabin chaired a meeting in 1983 on the various causes of AIDS, and the CDC and the NCI representatives were the only people who believed AIDS was caused by a retrovirus. NIAID was presenting other ideas as potential causes of AIDS that made much less sense to us than a retrovirus did.

Hannaway: Well, I just was wondering about...

Curran: Oh, the CDC and Gallo. A lot of things happened that were tough. Don Francis came from Phoenix to coordinate our laboratory efforts. We did not have any retrovirus people. He had done a doctorate with [Dr.] Max Essex at the Harvard School of Public Health on feline leukemia virus and was a strong believer that a retrovirus was causing AIDS. He was not himself a laboratory scientist, but he was trying to pull together laboratory activities. The CDC did not have a lot of horses, and Don was very aggressive, but CDC people had never worked on retroviruses in the laboratory.

Gallo was the dominant American scientist in the field. The French discovered the virus. I mean, they published it in 1983. And the paper was not convincing to everybody. But it was HIV; that is what it was. Don was working with the French, too, and we had connections at the Pasteur Institute. So Don was going over there, and we were getting specimens from them. Meanwhile, Gallo's principal protein chemist was a man named V. S. Kalyanaraman. He was having career-change thoughts, for whatever reason; and Don Francis hired him away from Gallo. Now, Kalyanaraman did not want to work for Gallo at the time. So somebody would have hired him. But Don did, of course, which did not make Gallo happy, because he needed him. Kalyanaraman came here with a couple of other people whom Don had also hired who were trying to beat Gallo to the punch. Meanwhile, we were getting specimens from France, as did Gallo. .

Hannaway: Mal Martin did too.

Curran: Mal Martin did. That is right. We were all getting specimens. We took some of our specimens and we put them into chimpanzees and we put them in a lot of other areas. And we developed protein probes. But we could not get the virus to grow. Gallo got the virus to grow, which is the main first thing that he did. Then he accumulated additional evidence to show, to convince people, that it caused AIDS.

When Gallo then published his papers, I mean, we had co-authored a paper with Max Essex, and related it to HTLV-1, in the same issue that Gallo published his paper about HTLV-1. And the French published their paper with LAV. The CDC was in the same kind of situation that Gallo was in in 1983. We were immortalizing the chaff and throwing away the grain. We all know that the French were right; but they could not get it to grow. We had inoculated the French virus isolate in animals already.

Hannaway: Yes.

Curran: Then U.S. politics took over. Now, how much of that is Gallo, how much of that is NIH, how much of that is HHS [Health and Human Services], how much of that is American nationalism, I mean.

Hannaway: Or all of the above.

Curran: I was the one trying to work together with the NIH. I was just being pragmatic. We needed reagents from NIH to do our studies. This was not a personality contest; we had a lot of things we needed to do. We needed to protect the blood supply; we needed to find out if the blood test we were going to be using at the blood banks worked; we needed to do a lot of things. So we needed reagents to do it.

So [Dr.] Peter Fischinger and I were always negotiating with each other to get things done. Don Francis was fighting with Gallo on the phone in the middle of the night. But also, the government was saying, "Okay, we're going to call this virus HTLV-3." We could not do that because we had been doing studies with LAV, and we could not call it HTLV-3. For example, we were doing studies with chimps that were infected with LAV, because that was what we put in them, before Gallo published his paper. So we could not call it something it was not. It was LAV. So until you could prove that LAV and HTLV-3 were the same virus, and we had to say they were, I mean, we knew they were the same. We did not know they were identical, but we knew they were the same. They had to be the same. Otherwise, neither one would cause AIDS.

Then, there was a meeting in Park City, Utah, that Gallo and [Dr. Jacques] Chermann and other people went to, and I was there. Gallo was talking about the isolates that he had. He called me shortly thereafter and asked me for a panel of sera from CDC, which we sent to him to do serologic tests, before he published the papers. He and I met in a restaurant called La Mìche in Bethesda, and went over and broke the code and looked at the results. I was quite convinced that what he had was the discovery. I told him that his results were nearly identical to those of the French.

Hannaway: But this was before he had published. He told you this in La Mìche?

Curran: Well, he showed me the code.

Harden: But you are talking about a meeting before the papers in *Science* came out in 1984, correct?

Curran: Yes. But it did not mean that his virus was the same as the French exactly, or that the French could really#SYMBOL ¶f "Symbol"188 I mean, his test was better.

Hannaway: He amassed the data that demonstrated what the French had not been able to demonstrate.

Curran: The thing is that they were all publishing in peer-reviewed journals; they can come back and reconstruct the history. I mean, the way it stands now is that the credit is viewed to be somewhat shared.

Hannaway: Yes, as far as the credit goes. I would like to ask if you would talk about the international scene. First of all, there was an investigation in Haiti in which Dr. Richard Krause and Dr. Clifford Lane were involved. This is in the very early period. We were under the impression that someone from CDC went with them. I just wonder if you recalled this.

Curran: Some people who went to Haiti.

Hannaway: Yes.

Curran: I do not remember whether they went with Krause and Lane or not. When did Cliff Lane start work on this problem?

Hannaway: He was Tony Fauci's postdoc, so in 1982, when the first AIDS patient showed up, the first one that they treated, he was the primary care physician for that patient. He and Fauci had worked on the patient to study AIDS pathogenesis.

Curran: So he was with Fauci from the very beginning?

Harden: Yes, from the very beginning. He has a wonderful story about going to Haiti and telling a cab driver he needed to find some brothels to do a scientific study. And the cab driver said, "Sure, doc." It was a charming story.

Curran: He is a charming guy.

Hannaway: It was a gay brothel he wanted.

Harden: Yes, he needed a gay brothel, the cab driver thought. And that was mostly because he looked like a choirboy.

Curran: For us, the Haiti story has a lot of different dimensions to it. There were cases of Kaposi's sarcoma diagnosed by a man named [Dr.] Bernard Liautaud, who was a dermatologist from Haiti, and they were diagnosed in the late 1970s and early 1980s. So it was suspicious that something was going on there.

The second thing is that among the gay men that we interviewed and met in the first year, there were about a dozen or so who had been to Haiti. I mean, I have never been to Haiti; not very many people have been to Haiti. But we knew that there were gay windjammer cruises to Haiti. We knew that there were gay brothels in Haiti. That was not the most common place gay men went. I mean, probably a lot of gay men have been to the Bahamas, they have been other places. But Haiti stuck out for that reason, in part, because there was clearly an epidemic of very similar conditions in people from Haiti who lived in Florida. This created a huge political problem.

Hannaway: Right.

Curran: KS was first diagnosed by pathologists in Florida. Two-thirds of the cases of AIDS in Florida in 1982 were among Haitians who were recent Haitian migrants, illegal migrants who came in the boat-lift era. As a matter of fact, there were a half a million or more Haitian-Americans who had been migrating over to the country legally since Papa Doc had been in Haiti in the 1960s, at the rate of about 8,000 to 10,000 per year. Most of them lived in the Northeast, the New York and New Jersey area. Almost all the Haitians were in that area. Very few were in Florida, actually. And there were no cases of AIDS in those Haitians. So, whatever it was, this was something that was a recent problem in Haiti. It was a problem that had recently migrated over among the boat-lift people. And it looked very much like AIDS. It was occurring in the sex ratio that one would predict from the sex ratio of the migrants, of about maybe 3/4 men and 1/4 women, which was equivalent to what the migrants were. So it looked to us like some type of heterosexual transmission issue, although we could not prove that.

First of all, for some, the politically correct view would be to say that this really was not AIDS at all. The Haitian physicians abroad, the Haitian tourist industry, did not want it. This was at a time when Haitian migrants were very unpopular in Florida. Baby Doc was a tyrant in Haiti. From a human rights point of view, it was a horrible thing to implicate the Haitian migrants as having AIDS. But the truth was, they had AIDS.

Harden: Were you not hearing anything from Africa, if I might just ask?

Curran: Not quite yet. That was coming soon, and, of course, it would fit together. This was in 1982. And that was when the cases in hemophiliacs also appeared.

Hannaway: Yes, right.

Curran: So we had these Haitians with AIDS. Then we did studies of Haitians, but they were not gay. But they looked very much like AIDS. They had toxoplasmosis, which is a different opportunistic infection, but that was predictable based upon their exposures. They also had *Pneumocystis* and other things, Kaposi's sarcoma. So it looked very much like we had Haitians with AIDS. Then, the story in Africa came along.

Hannaway: Yes, we would like to know about that.

Curran: One of the men who was on our staff at the CDC was a guy named [Dr. Joseph] Joe McCormick, who was an Ebola virus, hemorrhagic fever virus expert, and who is now at the Pasteur Institute. He had worked in Africa a lot. As a matter of fact, when he got out of college, he was in the Peace Corps. I think it was the Peace Corps. But, anyway, he was a high school teacher, and he was teaching in the Congo at the time it was Zaire.

His experience was in the late 1960s, before he went to medical school, that when the Belgians were removed from power in Zaire, there were very few educated Zairians. They had to bring in all kinds of expatriates, including Haitians to teach in Zaire. When Joe was over there teaching in the Peace Corps, he was teaching with many Haitians, who then subsequently returned to Port-au-Prince.

Now, in Haiti, unlike Africa, unlike Zaire, the cases were present in Port-au-Prince way before they were present in the rural areas, which would go along with the migration back of people from that country, and be compatible with the hypothesis that the virus moved from Zaire to Haiti. In Zaire, on the other hand, there were cases in the rural area in the 1960s and 1970s, which would go along with the thesis that urban migration increased the transmission in that country.

So Joe, coming back and thinking the way he thinks, believed that the most plausible idea was that this problem comes from Central Africa, is transmitted to Haiti, and then gets transmitted to the North American continent somehow through a combination of factors. Now, that kind of speculation back in the 1980s would be enough to get you severely criticized, but it was the most plausible hypothesis. Then Americans transmitted it through sexual contact to injecting drug users.

But the pattern in Haiti looked very much like the pattern in Central Africa. Ultimately, we were able, after the virus was discovered, to call it that way, that this was simply another heterosexual transmission pattern. And it became epidemiologically less relevant. But, initially, the cases in Haitians were a new phenomenon and it was a fairly prominent part of the cases, and it fit the specific case definition, and it was most of the cases in Florida. So we could not really deny its existence. But also, the uniqueness of the pattern among Haitians resulted in a lot of concern and discrimination in a population that was already discriminated against because they were migrants.

Hannaway: Can you tell us a little more about the cooperative projects between the CDC, the NIH, and the Institute of Tropical Medicine (ITM) of Antwerp in Projet SIDA in Zaire, because I know that [Dr. Thomas] Tom Quinn told us that you and he were both coordinators.

Curran: We were, yes. The most important aspect of starting the project was the trip to Zaire where Tom Quinn, and I think a man named [Dr. Frederick] Fred Feinsod or something like that...

Hannaway: That is his name, yes.

Curran: Joe McCormick from the CDC; Sheila Mitchell, who was a technician who worked on AIDS, and now she is with FHI [Family Health International], they were working with Joe; and [Dr.] Peter Piot, who was with the Institute of Tropical Medicine [ITM], visited Zaire. The initiation of that visit had been a combination of Dick Krause talking to the head of ITM at the time and Joe McCormick talking to some of his colleagues in Zaire. It looked as though, for a while, that the NIH and the CDC would be making separate visits, so we got together and made a cooperative visit.

During that visit—it was a remarkable visit—Tom and Peter and Joe were the principals, and Sheila did all the laboratory work. They were able to determine, with a man named Dr. Kapita, who was the head of cardiology and the head of medicine at Mama Yemo Hospital, that there were a lot of cases of this illness and wasting syndrome occurring, and that it was a fairly new phenomenon that he had been seeing more and more of since the late 1970s. They were able to get quite a few blood specimens, interview a few people, and they came back and actually published a paper in the *Lancet*. But after they came back, the CDC and the NIH were absolutely convinced that there was an epidemic in that city, Kinshasa, and that there should be some additional scientific commitment to a project there. And we agreed to do it together.

That created all kinds of benefits and problems. I think that, overall, there were an enormous number of good things that happened from that project. The worst thing that happened for the project was the downfall of the Zairian government, and our leaving. So whatever the mini-stories about conflicts and things that went on; occasionally Tom and I would fly over there to solve problems.

Hannaway: That is what he says. He says he talked to you all the time.

Curran: But there were hundreds of publications and some very important work. And the people who worked on the project were some outstanding people. Obviously, Peter Piot is now the head of the U.N. AIDS Program. Tom Quinn is ever productive and more famous all the time. And [Dr.] Jonathan Mann was our first project director.

Now, Jonathan had never been to Africa. But he was an enormously productive guy who spoke fluent French. His wife was French, and he studied in France. He had been a state epidemiologist in New Mexico. I met him and was very captivated with his abilities and his fluency in French. And his desire to do something like this was very important. So he went over along with [Dr. Henry] Skip Francis, who was the NIAID person.

Hannaway: Right.

Curran: The idea was, Jon was the project director. The CDC was putting up about two thirds of the money, the NIH was putting up one third of the money, and the Belgians put in a little bit of money. The Belgians had a man named [Dr. Robert] Bob Colebunders, who was a clinician, and then Skip Francis was the laboratory director. They started a project which, before it ended, had 220 staff and maybe a \$2 or \$3 million annual budget, an enormous number of extremely competent Zairian professionals, and an awful lot of good work was done there.

Jonathan left after two years, against my advice, and became the director of AIDS programs at the World Health Organization, with \$500,000 and two staff. His boss died immediately, and he built up the program to about 200 people and \$100 million a year. Then he resigned and went to Harvard, which he also resigned from, and now he is the dean at Allegheny School of Public Health.

He was replaced in Zaire by [Dr.] Robin Ryder, who was an NIH-funded investigator from Boston University, who had been at the CDC at one time. Robin, and then Skip Francis was replaced by [Dr. Christopher] Chris Brown. Is Chris still at intramural NIAID?

Harden: Yes, he is.

Curran: Chris Brown was an immunologist. He had worked in Tony Fauci's laboratory or [Dr.] John Gallin's laboratory at the NIH. Chris then came over to Zaire with his family. Then there was a guy named [Dr. Donald] Don Feyer who came. We were joined by [Dr.] Gerry Keusch's group from Tufts. So we had an extra partner. We also had the Armed Forces Institute of Pathology involved. They were a partner, and they had a woman who was assigned there for several years, [Dr.] Mary Duzarian. Who else did we have? We had a whole slew of people over there eventually. Robin Ryder was then replaced by [Dr. William] Bill Heyward and [Dr. Michael] Mike St. Louis from the CDC, and then the walls came tumbling down.

Hannaway: That was 1991, was it? The early 1990s, anyway. But since then you have cooperated with the NIH in other African countries.

Curran: We started a project in Côte d'Ivoire, and then we started another project in Thailand. And we cooperated with the NIH in Thailand. I think the CDC has collaborated with the NIH in Côte d'Ivoire too.

Harden: We are moving into our final questions. You have been very good about going into detail.

Curran: Not too much, I hope.

Hannaway: You are telling us all sorts of new things.

Harden: I wanted you to know that this interview was not going to go on all night. But we want to ask one or two more general kinds of broad, speculative questions. Has the AIDS epidemic changed the way that the CDC and the NIH interact, or the way federal science agencies interact in general?

Curran: Well, I was not around before the 1970s. I think that there is always going to be a certain amount of both brotherly collaboration and competition between the agencies. Of course, I am very interested in the NIH now that I am in academia, and I actually spend at least as much time with the NIH. I am very involved with NIAID and OAR [Office of AIDS research], and NIDA [National Institute on Drug Abuse] and other agencies. I love the NIH and I love the CDC. But I think that I describe the NIH as the world's premier research agency. The CDC is really primarily the world's premier public health agency.

Now, that does not mean that the NIH does not do any service, and it does not mean that the CDC does not do any research. A lot of people at the CDC may think that the CDC is a research agency; and there are some people who are in the service aspects of the NIH that would say, "We serve." But it helps fundamentally to say that the NIH is a research agency, and that the CDC is a service agency; and, therefore, derivative from the CDC's mission comes a lot of applied research and a need to respond directly to problems. With the NIH, derivative of its mission is the need to find relevance for research. So there is a need for the two agencies to cooperate and collaborate together.

The other thing is that the constituencies for the NIH are, at one level, research institutions, which are very good at harnessing to get what needs to be done, done. To the extent that anybody can do it, the NIH can do it. The CDC's constituencies are more action agencies, and the CDC has a broader network of action agencies around the world than the NIH does, is also more involved with health departments and regulatory issues, and has more of the public health authority. So we understand the differences between the agencies as a natural reason to cooperate and collaborate on certain problems.

The other parts of it involved the questions of how the Public Health Service works, and how the HHS works, and how individual personalities interact.

Harden: And will there be a Public Health Service?

Curran: My concern now is that it has been diminished so much in the current administration. I am not aware that there is the same kind of intense communication that there used to be. There used to be agency heads' meetings at least every couple of weeks or so.

Harden: Not anymore?

Curran: I do not know. And they all work for the Secretary now. In AIDS, we would have meetings every couple of weeks that were chaired by the assistant secretary for health. I do not think that is necessary anymore, but whenever there was so much communication between the groups and a need to collaborate, even if there were issues of competition, there was somebody who was able to rise above that because of the kind of relationships and communication that we had.

So I think that, in general, the NIH and the CDC would usually would pull together before there was a major, huge problem. One of the things I found most rewarding during the AIDS epidemic was the work I had with my colleagues at the NIH. I grew to respect deeply many of the talented people there who worked so hard.

Harden: Do you think the AIDS epidemic changed the way the CDC does business in terms of its funding and its standing in the government? The CDC has been the one agency that people in the United States have known about, because it has been the agency that has always there when some disease crisis happened. But, of course, it was really suffering from the funding cuts in the early 1980s. Has it recovered?

Curran: The CDC got a lot of AIDS funding. That does not necessarily translate into fiscal health for an agency. I think that the concern about emerging infectious diseases and the concern about AIDS has led the country to appreciate the need for that type of response. There was antibiotic resistance, tuberculosis resurgence, the AIDS epidemic, Ebola. These are the kinds of things that I think legitimize that concern. The CDC has a problem in that it got involved in disease prevention interventions, and prevention is something that everybody is for in principle, but they do not like the details.

Hannaway: Yes. They do not want to be told not to do things.

Curran: For example, "Do not smoke." Also register and control firearms to eliminate firearms-related deaths, practice family planning.

Hannaway: Wear seat belts to improve car safety

Curran: And use contraceptives; exert more control in the meat industry to help prevent the appearance of lethal *E. coli* bacteria in Jack in the Box hamburgers. Because of these unpopular policy positions, the CDC gets itself involved in controversies. The NIH is a safer agency, in terms of political interference, because it sticks to research. I think that if there is a generic jealousy between the NIH and the CDC, the NIH people would say that the CDC gets too much credit, too much visibility, too much press, and is a little too saintly. The CDC people would say that the NIH has all the money, has direct contact with the Congress, has all of the academics behind them--every party loves them. The CDC has trouble sometimes with some of the different parties and groups. But I think the country should be proud to have two such agencies; and I worry about the health of both of them, because I think they do go in parallel.

Hannaway: Would you just comment briefly on how AIDS has changed your life. Did you or your family experience any negative fallout about your involvement in AIDS?

Curran: My daughter had her seventeenth birthday last night. She was born two weeks before the first case of AIDS was reported, just about a week before I got the *MMWR* article. My son was two at the time, and so, I have really not had a family life without AIDS. And I constantly think that I am the last generation to go through life without AIDS.

Harden: Yes, that is an excellent point.

Curran: My kids have always had AIDS. Their sexual lives occur in the context of AIDS. That may be true for the rest of history. The world is different.

Hannaway: Yes.

Curran: I am constantly asked, usually not by experts like yourself, however, for my "perspective" as I get older and the epidemic gets older. I think that, on the one hand, it is a horrible epidemic, and it is now in a kind of a slow phase, not scientifically, but in a slow phase with respect to public concern, as the horizons become certain in the United States. But throughout the world, it is getting worse all the time. There is an enormous amount of uncertainty in the long run, say, for the next 50 years with AIDS, in terms of adequacy of therapy and the availability of vaccines. It is not an epidemic that we have a lot of confidence in controlling. We have millions of human incubators wandering around the planet, immuno-compromised people, carrying other organisms around with them, much like they did multi-drug resistant tuberculosis; and there are a whole lot of things we do not know yet about AIDS. There are a whole lot of acts to the play still to follow.

I do not know what would have happened if there had been no AIDS and what I would have done in my career. I guess my life in public health would have reflected other experiences. AIDS is an epidemic that I am humbled by, but I feel grateful to have learned from. Does that make sense?

Hannaway: Yes.

Curran: It is pretty humbling when you think about it.

Harden: I am thinking of the term that David Henderson used. It was "lightning rod," in the sense that you and Bob Gallo and Tony Fauci were very public figures who spoke about AIDS, frequently on the television. Did this have any impact on your personal life?

Curran: At first I was the most visible. Then Gallo became the focus of attention. And then Fauci became the central spokesperson and stayed there. My visibility was dimming by that time.

Harden: You never had any friends who refused to have dinner with you because you worked with AIDS?

Curran: I have had a lot of personal experiences, but my wife has never worried about it. I had staff who died of AIDS, and I also had staff who, before the virus was discovered, got needle sticks during the investigations. They were really panicked, and their spouses were panicked.

(Actually, I had fairly severe sinusitis when the epidemic first started, and I had a brutal travel schedule, and I did not take care of myself. "Take care of yourself." What does that mean? I was coughing all the time. I would get up and I would have sinus headache. I would get up and show these X-rays of people with *Pneumocystis*. And I would be coughing and coughing. I would go from meeting to meeting around the country coughing. I knew it was sinusitis, but I think sometimes people thought that I might have had *Pneumocystis*.)

No, I did not feel personally threatened. An ironic thing was that my phone number was listed in the directory all those years. I never had an unlisted phone number. I never had people come to my house and break my windows or anything. In fact, I received a couple of awards from the gay community, in 1982 and 1983, when they were really upset with the government.

During the time of the women's issues case definition, Tony Fauci and I, but especially me, were the enemies of ACT UP. I got 20,000 postcards, with my picture with a target on it. And a lot of them came to my house. I went to conferences and got spit on a few times.

Harden: Did the CDC have pickets?

Curran: Yes, we had pickets, people in our offices. At that time, my son was an adolescent. He did not understand why those postcards did not bother me that much. I never really took it personally. I used to hang the target up in my office. I had an award from the Atlanta Business and Professional Guild, which was a euphemism for the Atlanta gay business community. They gave me that award in 1982, and then my staff, when I moved from one job to another, gave me a blowup of this target with my picture on it. And I put that underneath that award in my office.

I did that to show that the award and the target post card came from essentially the same community, and I was the same person. But it really was not personal. I was getting the award for what the CDC did early in the epidemic. And I was getting blamed for what the Reagan Administration would not do, that is, provide Social Security benefits to people with AIDS. The gay community wanted the case definition of AIDS changed so that the Social Security Administration would be forced to provide those benefits. It had nothing to do with surveillance. Of course, people in the gay community, even in ACT UP, did not distinguish my surveillance responsibilities from the Reagan Administration's policy decisions. They just decided that I was the person they saw dealing with AIDS, so I was the target. My son got the postcards in the mailbox, and he was an adolescent and did not like this, so he wrote "F-uck You" on a couple of them in the back, just to see if he could get me upset. Sometimes my wife would get them from the mailbox and would say, "This is really getting out of hand."

Harden: It is hard if people spit on you.

Hannaway: Yes, I would be pretty upset.

Curran: At a meeting that Tony Fauci and I went to in Washington, a Women and AIDS Conference, I was the designated target. I got up and gave what I thought was a very heartwarming, pleasant, informative speech. And, of course, they were all screaming and yelling.

Hannaway: Yes.

Curran: Then I had a big public affairs bodyguard, a huge guy. A few women followed me out. They were not big women, but they were angry, and the cameras were all around. They were spitting at my face. The idea was that I was supposed to react in some way so it would look like I was hitting them or doing something. But the real problem was I could not go to the meeting. They kept me in a Washington hotel room for my own safety.

In Amsterdam, I had ACT UP Europe angry at me for the same reason, and that was because of the case definition of AIDS. The case definition was really related to Social Security benefits.

Harden: Yes, I wanted you to continue on about that.

Curran: It had nothing to do with anything in developing countries, but they said that our case definition was killing women in Africa, which, of course, made no sense. But that was what they were programmed to do by the activists. I gave several talks, and after I gave one talk, when they were all standing and screaming at me, I agreed to meet with them afterwards in the ACT UP conference room. Now, the interesting thing was that the CDC did not have a conference room at the meeting, but ACT UP had this beautiful conference room. They had become "corporatized" activists. Some of them did not speak English. Maybe it is a new form of activism, but fundamentally it is not personal.

Harden: Tony Fauci was telling us that Larry Kramer insulted Tony's wife at some point, and Fauci said, "He has spent the last 10 years trying to make up for that insult." Kramer said that it was not personal either but it was what he had to do as an ACT UP person.

Curran: Remember Ellen Cooper, who is now at OAR? At the time I am speaking about, she was at FDA. She was in charge of approving drugs. And the activists were just horrible to her, and she decided to leave government. She did not want to do it anymore. Well, the activists went crazy and claimed that Ellen was the best person they had ever worked with. Her story also shows how different people are different. Not being in Washington, I experienced less personal activism here in Atlanta. I am sure that if I had been in Washington or New York, I would have been more likely to have people actually picketing my house. But I was always there for them to talk to. I did not go away. I would go to meetings and talk with them.

Harden: Did we do the right thing by not employing some of the traditional public health contact tracing and not publicizing the names of people who were infected? There is one recent case that is very upsetting--the case of the man in New York who was going around infecting high school girls.

Curran: There is a misconception about what constitutes traditional public health methods. First of all, we have the STD Control Division. There are 25 or 28 STDs. The only STDs which ever had any public support for contact tracing were gonorrhea and syphilis. Most recently, there has been a little funding for chlamydia infection. They are all bacterial infections that could be either prevented or treated easily with one shot of antibiotics or one pill, or sometimes a slightly longer regimen.

Contact tracing has never been done traditionally, or untraditionally, with hepatitis, with herpes, cytomegalovirus infection, anabiasis, trichomoniasis, or anything else. But it is the gonorrhea-syphilis mentality in venereal diseases that people remember. They are thinking back to the Thomas Parran era and how they treated syphilis then. They think that syphilis is *the* venereal disease. How we treat syphilis ought to be how we treat everything; and that is the standard by which everything is set. Nowadays people do not even do contact tracing for syphilis uniformly.

So that the issue, when people say to me, "Why do not we treat AIDS like other STDs," from my 10 years in STDs, I would say, "What do you mean, ignore them?" That is the way we treat other STDs. We ignore them.

Harden: That is interesting.

Curran: So the question is, "What are the most effective means to treat and prevent HIV infection?" Obviously, behavior modification. You have three major modes of transmission: sexual transmission, drug abuse transmission, and perinatal transmission. Those are the three big ones. Identify infected people as soon as possible. Make sure they know they should not transmit to others. What you have to do is get them to recognize that they are infectious for the rest of their lives. They have theoretically been infectious for as long as they have been infected. They could have had partners way, way back, they could have current steady partners, and they could potentially have future partners. So to stop the spread of the disease, you need to obtain their cooperation for the future. You have to get them tested. I do not think all that many people in general who are infected willingly and knowingly transmit to others. The trick is to find them early enough, and also to deal with people who are not infected.

There are always going to be a few exceptions from which people will try to make the rule, like that man in New York. But if you think about it, who was going to find that guy anyway? He may not know he was infected. Even if he did know he was infected, and somebody told him he should not transmit to others, it may not have stopped him. There are all kinds of people, a lot of whom you and I know, some of whom are NIH and CDC employees; who are not people who need constant surveillance by public health officials to monitor their behavior. It is also very hard to do that because you have to do it for the rest of their lives.

Harden: The analogy with Typhoid Mary, however, leaps to mind. Just put her in jail. Some people have suggested that we jail people infected with AIDS who knowingly transmit the virus. But we just do not do that.

Curran: I have reviewed the new book, *Typhoid Mary*, by Judith Leavitt.

Harden: Yes, we know the author very well.

Curran: Even Typhoid Mary was singled out. There were thousands of carriers of typhoid, and she was the one that they picked on. Now there were some reasons. She went back to work and started transmitting the disease again. But she was also a single woman who was tough, somewhat large in size, and easily selected out. Then they just forgot about her when she was imprisoned for years.

Once again, though, typhoid was transmitted in a different way. It was an example of selective transmission. I think that if you found a person like the man in New York who was knowingly transmitting AIDS that you certainly would stop him from doing what he was doing. But one of the questions is, "What do we do with prostitutes and drug users?" If we are really serious about transmission, why do we not have a serious attempt to deal with substance abuse in the United States? We cannot even use the words "needle exchange" because of their political fallout, and we know that the roots of the heterosexual epidemic are in drug users. How about all the women who are infected, for example, through drug abuse and needle sharing? How do you think they get the money to get their drugs? And how do we as a society deal with that?

In general, what we do is we lock up drug users. The last decade has shown that we have doubled the size of our prison population, and we have actually decreased the size of our drug treatment population. But that does not make sense. If people are really serious about stopping the transmission of AIDS, we must take a more epidemiologic targeted approach to HIV prevention. We must go to where the transmitters are. We need drug treatment programs in jails because a lot of people infected with AIDS are in jails. There are more of them in jails than there are in treatment now, and there are an awful lot of them on the street. So we ought to be figuring out how to deal with them, rather than being so focused on the disease in middle-class gay men.

I think we need to have programs that really encourage people to be tested and counseled. There may be some rare times when you have to use force, but when you start off with that premise, you end up with a very uncooperative population. And, for example, the NIH MACS studies and the CDC cohort studies in San Francisco and other places show that people in those studies were usually tested. They were counseled over a period of time. They eventually then became candidates for drug therapy, and they were followed, and the NIH said, "None of these people are getting infected anymore." And I said, "Isn't that wonderful. That is really great." And they said, "But they are different from everybody else, because they have adequate counseling and adequate medical care, and access to drugs and treatment, and you cannot say that everyone else is going to be like that and not transmit or not get infected anymore." Well, why can't they be? Why do we have only some people who are tested, treated, and counseled?

Instead, what we have is rhetoric on one side saying that we need to throw people in jail; when, in fact, there is still a third of the population that do not know they are infected. They die of *Pneumocystis* before they get any therapy at all, and then we blame them for transmitting.

Harden: That is very well put. Is there anything else that needs to be said about AIDS before we try to get you to talk a little about being a dean of a school of public health.

Curran: No, nothing else needs to be said about AIDS.

Harden: Could you comment here on your new career? What is your perspective now, having moved from AIDS, which clearly dominated the last 20 years or so of your life, to being a dean?

Curran: We have a wonderful School of Public Health, and it is nice to move into a combination of jobs of educating and being responsible for the training of the next generation of extremely idealistic people. It is just remarkable what these young people are like. They really want to save the world. And it is wonderful to be able to get resources to send them overseas; we have 90 people overseas this summer.

Harden: I noticed all the things on your bulletin board as we came in.

Curran: Yes, and you ought to see the international health board, on the next floor down. We just graduated 287 people last week, so there are not a lot of students around now, but it is great to be able to do that. It is great to continue my relationships with the NIH and the CDC, and to stay involved in public health issues. It is fun for me to broaden my interests and to be in an academic environment because I get a chance to do lots of other things that are interdisciplinary and inter-school. And Emory is a fun place to be. It has always been good, and it is getting even better.

Harden: Harvard is the "Emory of the north," is what we Emory alumni like to say.

Curran: Emory is a friendly place, and there is a lot of hot research going on here. So I enjoy it.

Harden: We certainly appreciate your giving us this interview. Thank you.