

This is an interview with Dr. James C. Hill, the Deputy Director of the National Institute of Allergy and Infectious Diseases in his office at the National Institutes of Health in Bethesda, Maryland, on October 4, 1988. The interviewer is Dr. Victoria A. Harden, Director of the NIH Historical Office.

Harden: Dr. Hill, could you summarize your career before 1981 for us and indicate your special areas of interest and how they helped to prepare you for thinking about a new infectious disease?

Hill: I have a Ph.D. in microbiology from the University of Arkansas, and my graduate background is in research on fungal physiology. I went into the Navy Medical Service Corps as a research microbiologist and was stationed at Naval Biological Laboratory, Naval Supply Center, Oakland. There I worked for about two-and a-half years on meningococcal disease and helped set up the *Neisseria* reference laboratory, a World Health Organization reference center, for meningococcal and gonococcal strains. I worked with Dr. Neylan Vedros. I then came back to the Naval Medical Research Institute in Bethesda and spent five years working with Dr. Emilio Weiss, who is well-known primarily for his work in rickettsia physiology but who was also working on meningococcal disease and vaccine development. I worked with him on meningococcal physiology and vaccine development.

I then was offered the job as the bacterial vaccines program officer at NIAID in 1974 in what was then the Infectious Diseases Branch run by Dr. George Galasso. That branch later became the Development Applications Branch after Dr. [Richard] Krause came to NIAID and reorganized its extramural programs. I spent nine years as the extramural program officer for the development and testing of bacterial vaccines, such as meningococcal, *Hemophilus influenzae*, pneumococcal, and pertussis vaccines. In this role, I obviously gained some experience in the development of RFPs [request for proposals] and the award of contracts for research. I mention this because it leads into how I became involved in the AIDS area.

While I was in that position, the first cases of AIDS were described, and in the spring of 1983, the then scientific director of NIAID, Dr. Kenneth Sell, asked me to work on the establishment of a contract for AIDS research. Dr. Sell had previously been the executive officer and commanding officer at the Naval Medical Research Institute at a time when I was there. He knew my experience in contract awards and development of RFPs. At that time he envisioned that the NIAID intramural program would develop a large contract to gather specimens, all kinds of specimens, from persons with AIDS to see what was causing the disease.

You remember that in the spring of 1983, the causal agent was not yet discovered. It wasn't even certain that it was an infectious disease, although a lot of people thought that it probably was. Dr. Sell wanted to mount a major intramural contract that would gather samples from persons with AIDS, including fecal samples, urine samples, blood samples, sputum samples—any sort of body fluids that you could think of. He wanted to store them for research in order to see if we could determine what causes this disease. He needed somebody to help set up this contract, and he approached me and George Galasso, who was my boss at the time, about whether I could come over and assist in setting up this contract. I did it as a favor to the intramural program to help it get started, and I wrote the RFP.

Then Dr. Sell asked me to join the intramural program as his assistant. In July 1983, I became the associate director of the [NIAID] intramural research program. My involvement from that point on in AIDS was in helping Dr. Sell administer the intramural program, which was gradually developing major research areas in AIDS. The contract that was set up was taken over by Dr. Richard Wyatt, who is now in Building 1, as the principal investigator.

A number of intramural projects were going on in AIDS. You know Dr. [Anthony] Fauci's work on immune deficiency was on going. He was involved in that from the early days when people with this kind of immune deficiency were first identified. Dr. Sell himself had a group at that time. Dr. Tom Folks, who for the last two years has been in Dr. Fauci's lab and has now gone to the Centers for Disease Control [CDC], was then in Dr. Sell's group. Dr. Sell, even though he was a scientific director, had his own research group.

Gradually the intramural program began to acquire more and more AIDS projects. My involvement with AIDS was assisting Dr. Sell in the overall administration of the intramural research program. When Dr. Krause left the institute in 1984, Dr. Fauci was selected the director. I knew Dr. Fauci as an intramural lab chief when I was the associate to Dr. Sell. Dr. Fauci asked me to take the position of special assistant to the director, a position similar to what Dr. Jack Whitescarver had under Dr. Krause. I came to him in that job and also as his AIDS coordinator for the Office of the Director of NIAID. I stayed in that role for about a year, until we hired Maggie [Marguerite] Donoghue, who then became Dr. Fauci's AIDS Coordinator. I stayed on as his assistant, handling congressional liaison and liaison with constituency groups. This is how I became involved with AIDS research.

Harden: When Maggie left the Institute, who took her place as AIDS coordinator?

Hill: After Maggie left, Debbie [Deborah] Henderson held that job for about a year. She moved on to the FDA, and now Dr. Margaret Hamburg has what is the equivalent of that job. Now there are two people who cover AIDS for Dr. Fauci. Dr. Zeda Rosenberg handles the scientific part with Dr. Fauci. She works with him closely on manuscript preparation, liaison with the lab, and on a lot of scientific aspects. Dr. Hamburg deals with scientific aspects as well, but she is much more the AIDS coordinator in this office. She is Dr. Fauci's liaison with the [NIH] Building 1 office and with downtown [Department of Health and Human Services], and she deals with his role as an AIDS spokesman.

Harden: Did the advent of a new infectious disease surprise you as it seemed to surprise a lot of people? Could you, from your own microbiological experience, put this disease in the context of the other sexually transmitted diseases?

Hill: I guess the answer is both yes and no. It surprised me to a certain extent. There were so many theories as to how this disease was caused and how unusual it was. While a lot of people thought it was an infectious agent, we thought it might be caused by some sort of drug use. There was even a proposal that it was some sort of mercury poisoning. It also seemed to be a result of a combination of activities, since initially it was mostly a gay men's disease. Even though it began to look more and more like an infectious disease, it did come as a surprise, mainly because the idea that an infectious disease existed but had not shown up before was devastating. Even microbiologists didn't understand it, and the public and the scientists had become very complacent in the last few years, in the last few decades, because antibiotics could hit pretty much everything, in spite of the fact that a lot of tolerance had developed.

In another way, it was not too surprising because there had been a couple of similar forewarning jolts—for example, Legionnaire's disease. It was quite a surprise to us to discover an infectious agent that was totally new, and you know that it was a very scary thing when it happened. Legionnaire's was very mild compared to what AIDS has been, but it was very scary when it turned out to be a new infectious disease. Toxic shock syndrome—it took a while to figure out what was causing that, and that clearly was a new infection, at least a new disease that hadn't been described before. So I guess in a way those two events made us realize that a new infectious disease could exist, but this one was so devastating that I guess it did surprise people. I think it may have surprised us more because we were all hoping that it really couldn't possibly be true.

Harden: From your experience over the last few years with AIDS, how do you evaluate its impact? Certainly it has proved fatal, and that, of course,

makes a major difference. How would you compare it with the impact of the other sexually-transmitted diseases?

Hill: Obviously, it's been a whole new ballpark to an extent. Historically not so much, because there was a time when syphilis was not curable and people lived long lives with syphilis, went through various stages, and it killed a lot of people. Gonorrhea certainly caused a lot of problems as well. But most of the sexually transmitted diseases were fairly slow in causing a disease, you lived with them for a long time, or they were not life-threatening. They caused problems that we recognize now as being severe sexually transmitted diseases. We just didn't recognize the importance of things like pelvic inflammatory disease, which causes sterility and death in a lot of women.

I think that certainly in the modern era, there are no sexually transmitted diseases that can be compared with AIDS. For the most part, they can be handled just like a lot of other diseases. Except for a little bit of a scare about the penicillin-resistant gonorrhea, and there were other drugs that promised to deal with that, most of the sexually transmitted diseases certainly can be handled. Herpes is not such a case. Before AIDS came along, there was this enormous scare over herpes. But again, not a lot of adults, and particularly young, productive, healthy adults, died of herpes. That time it seemed like a tragedy. Now it almost seems like an inconvenience, to have gonorrhea, herpes or even syphilis. And while these diseases still cause problems in other countries and in the United States, and they are serious diseases, causing infertility and death, in comparison to the sheer horror of AIDS—the debilitating, wasting, way in which very young productive people die—I don't think anything, at least in the modern era, quite compares with it. Again, this is if we look at it in the perspective of this country. If you look at the Third World countries, you see the numbers of people who die every year from malaria, and from other infectious diseases, you look at it from a different perspective. But from the standpoint of this society, in this country during the last several decades, it has been more a traumatic thing compared to the other sexually transmitted diseases and other infectious diseases.

Harden: It seems to me traumatic also because none of the other diseases in recent decades had caused the same degree of behavioral fear. People seemed to have changed their behavior since AIDS came along.

Hill: There are social aspects involved in this disease that are just not really the same as in other sexually transmitted diseases. Although the incidence of gonorrhea or syphilis may be considerably higher in gay men, for example, those diseases do not have the clear association of making an individual stand out as being a gay person simply because he has

contracted one of them. Early on, and even now to some extent, many of the persons who had AIDS were homosexual men or were intravenous drug abusers, people who are disenfranchised and discriminated against in society. Thus social ramifications in this case are far more complex, involved, and frightening to those people who have the disease than was the case with the other sexually transmitted diseases.

Harden: Could you recall when you first heard about these unusual cases of Kaposi's sarcoma and *Pneumocystis carinii* pneumonia [PCP]? How did your own thinking evolve?

Hill: It was 1981-82 when we heard about the CDC report. But you really couldn't take it as a base period. These were a few isolated incidences. It just didn't seem like something that was going to have quite the ramifications that eventually emerged. It was almost like, "Here's an interesting sort of disease anomaly." It was very slow, and I think a lot of scientists were very slow in recognizing what it was. It was interesting that it was hitting gay men only, and a couple of other groups. But there was no particular appreciation that what we were standing on was the edge of something as devastating as it turned out to be.

Harden: Were you getting any reports out of Third World countries at this point?

Hill: I don't recall, but again, you've got to realize that in the early days of this, I was not working in this area. I was the program officer for bacterial vaccines. I was neither in the intramural program working with the people who were involved in this area, nor was I involved in the sexually transmitted diseases program directly at that time. Professionally I was not really privy to any more information than anybody else at NIH working in another area would be. My appreciation for it really began to develop in 1982-83, recognizing the concern and what was happening. I was in an extramural program that was not really involved in this area until Dr. Sell asked me to get involved with his contract. From then on I've been involved, not so much as a researcher, but as a science administrator and in policy administration of the institute. So it's hard for me to remember exactly what the early thinking was. There certainly was no real comprehension of the seriousness of this. Probably not as much as the people working much closer in the field. I was really just on the periphery.

Harden: As I recall, between 1981 and 1983 there were just a few brief news accounts, and I wondered, "What does it really mean?"

Hill: I think I had just a little bit more information than that but not much more. I would see the CDC *Morbidity and Mortality Weekly Report* [MMWR], but I was looking at it from the standpoint of a meningococcal

disease or whatever. I would see occasional reports, and then a few weeks later there would be another report from CDC on something about it, and a few weeks later something else. So, I began to become aware of it, but my awareness really only picked up in the early part of 1983.

Harden: Would you describe the process through which NIAID shifted its priorities? Here we have a large institute in which the investigators are all working on their own problems, and all of a sudden this huge institution needs to redirect itself. Tell me how it's done.

Hill: During that period, I moved from an extramural to an intramural position, so I was not in the office of the NIAID director, but I could see what happened in the intramural program. The first people who began to look at it were people like Dr. Fauci, who were interested in diseases of immunoregulation and immunodeficiency. Not long after—or at about the same time—that the first cases of the Kaposi's sarcoma were described by Dr. Mike [Michael] Gottlieb, and I really don't know the history here—you might have to get that from Dr. Fauci—but not long after the first description of Kaposi's in this population, Dr. Fauci began to look at patients, who were later known as AIDS patients. I'm not sure that his looking at them predated the description of the California cases, because he was looking at people who had strange immunodeficiencies. But very soon after that, there was stepped-up activity in the intramural program. Because of Dr. Fauci's efforts, Dr. Sell, who very quickly began to see this as a concern, developed his contract, and several laboratories began working on the problem—people like Dr. Robert Purcell, who was involved in hepatitis and saw possible similarities. There were a few people who became aware that this was a new research area that needed to be moved into.

One of the good things is the strength of NIH in having an intramural program. You can immediately redirect your intramural activities, because if a scientist is interested, then he has only to get his lab chief and his scientific director to agree. It's not like the extramural program, where it could take a number of months to advertise the new idea and then almost a year to get an award granted. The intramural program can react very quickly. Gradually people within the intramural program saw AIDS as a disease that they should be working on: Dr. Fauci's lab, Dr. Sell and a couple of other people. I think that Dr. Mal [Malcom] Martin came a little bit later into this.

Harden: Was their interest in this disease in part stimulated because the CDC, which was the first line of reaction to this disease, wasn't getting anywhere in their efforts to identify an etiological agent? NIH doesn't usually look into every questionable disease event.

Hill: I'm not sure that this approach was much different from the others. You've written about serendipity in science. In researching Rocky Mountain spotted fever you have seen how research on it led to the discovery of the organism that causes Lyme disease. Something was observed that was similar to ongoing work. I have actually shown a little prejudice here, in that I have looked at AIDS strictly from an NIAID perspective. Dr. Bob [Robert] Gallo and the other [National] Cancer Institute [NCI] retrovirologists, who were looking at the viral etiology of a number of lymphomas tested HTLV. HTLV stood for "human T-cell lymphoma virus" or "leukemia virus." I think the "L" initially stood for "leukemia" virus, and then, at some point it was changed to "lymphotrophic" virus. I think it was after the AIDS retrovirus was described. So you had people who were looking at viruses that caused some sorts of tumors and immune deficiency. I think Tony Fauci looked at some of these patients and saw that they had problems with their T cells. He then immediately asked Bob Gallo, who was working with a virus that affected T cells, to look at them, also.

Now, I'm not sure of the connections. You can get this information from Dr. Fauci and Dr. Gallo. What you had were people who saw something happening that was similar or at least showed a relationship to something that they were looking at both immunologically and virologically. NIH's normal response is that if the investigators see something of interest, they continue research in that area. I don't think there was any point where any NIH policy decision was made that we were going to work on the disease. What happened is that investigators who were studying this general type of immune deficiency or a virus that caused this type of T-cell defect saw the similarities with AIDS and immediately began pursuing it because of their personal interest. I think that individual scientific interest generated the intramural program's moving into that area. There was no special policy decision until a little bit later on.

Harden: When did it become clear that there was considerable public pressure for the government to do something? Can you recall a particular discussion within the NIH about what should be done and how it should be coordinated? I know that Dr. Bob [Robert] Gordon headed a coordinating committee, and I'm sure the NIAID had one with the NCI.

Hill: I was actually not privy to a lot of that, because I worked as the associate director to Dr. Sell. In that role there was a period of time when I served as the intramural AIDS coordinator for NIAID, when I did coordinate what was going on intramurally between, for example, Mal Martin's lab and somebody else's. Occasionally I sat in on meetings that had to do with NIH, but I was really not privy to the discussions within NIH, or to that extent within the institute itself. So I can't help you much prior to about 1984, which was when I came in to this office.

Harden: Who was the NIAID person on the overall NIH committee?

Hill: Well, I don't know, but somebody like Jack Whitescarver, who was the assistant to Dr. Krause, would maybe know. I would imagine that he, because of his interest, may have been involved.

Harden: Could you outline the particular steps that NIAID has taken against AIDS since you became involved at the institute level?

Hill: From my perspective, most of the steps have been strictly in getting funding for research and involvement. I came with Dr. Fauci in 1984. Tony Fauci came into the directorship with a very strong personal involvement and interest in AIDS and with a very strong commitment that this institute should make a major and leading push. By that time—that is, in 1984—we knew that AIDS was an infectious disease of the immune system, which predisposed those infected to other infectious diseases and tumors, and that AIDS was sexually transmitted. It also held an interest for those people working on international tropical medicine. So from the standpoint of the institute's and Tony's personal interest, there was no doubt that this institute could make a major contribution to AIDS research.

In doing so, there was increased involvement in the intramural program. There was an increase in requests for money through our appropriation process. I think that the fiscal year 1984 hearings may be the first year that the word AIDS appeared in the congressional appropriations hearings report language. You may want to check that out. Now, retrospectively, we've gone back and reported funding back in 1982, because there were intramural scientists working on this disease. There were people like Bob Gallo, Tony Fauci and others who were doing work. We, in fact, started that contract in the intramural program in 1983, but we didn't know what we were looking for. We started the extramural Multicenter AIDS Cohort Studies in 1983. This study followed cohorts of homosexual men over a period of time to see what caused the disease. This was funded with money the institute chose to use for that purpose. It came, in part, from our Infectious Diseases Program and in part from our Immunology, Allergic and Immunologic Diseases Program. Both extramural and intramural programs used their current funding to support this kind of work. After Dr. Fauci became director, several opportunities arose for supplemental funding in a fiscal year or increased funding in an upcoming fiscal year. It began to move fairly quickly from that point on, but the institute needed to make a major push.

In the early days, the Cancer Institute was the major spender of money on AIDS, and we were a smaller player. It gradually moved to the point where we were receiving sixty nine to seventy percent of NIH's money [for AIDS]. Basically, this was because we saw AIDS as our mission, and we made a commitment to work on it. The single biggest factor in that link was probably in fiscal year 1986, when we got a supplement. Then we were also allowed to amend that budget, and so we got a second amended budget. But a major push occurred, I think, not very long after Rock Hudson's death in 1985. Up until that point, we had gotten signals from downtown that we should either ask for amended budgets or ask for supplements to our budget to do AIDS research, which the institute did. The problem is that we would never ask for more than we knew we could absorb without destroying our other programs. What would happen, in spite of the Congress's interest, was that the administration might say, "Tell us what you need to do AIDS research." We might provide a dollar amount, and they might say, "That's very important, it has high priority. Take it out of your other areas." It was the Congress then that would go in and cover us and give us additional money. There was one fiscal year we needed some extra money, and Senator [Alan] Cranston put in a special bill that gave us the money to bail us out because we had put money into AIDS research and fell short in other areas. If you go back and look at the history, you'll find that in the past few years the administration requested additional money for AIDS, new money. What they did was agree that it was very high priority but that we had to take it out of other areas.

For a number of years, it was the Congress who put in more each year. In fiscal year 1986, I believe, we had already put in for a modest amendment because, again, we didn't want to be told to take it out of the other areas. After Rock Hudson died, we got a signal from the Department [of Health and Human Services, DHHS] that they would be receptive to a much larger request. We made a very crucial decision in the NIAID that we would go for broke. That decision was made in this office. Dr. Fauci and his staff made the decision to go for it. Yvonne du Buy and I were very much involved in putting together a major increase in our program. The major increases were in what we called the "AIDS Treatment Evaluation Unit," modeled upon our existing vaccine evaluation units for other diseases; in epidemiology; and in the intramural program, which Dr. Fauci supported. He looked at the program and he pushed it. We decided that the only way we would make a major push in this area was by going for broke and ask for so much more money that everybody would know that if they made us take it out of our existing budget, that they would totally destroy our other programs. We were afraid to do that before then, and other institutes were afraid to do it as well. Over one weekend we put together a huge new program, I can't remember the dollar figures right now, but it was a big percentage over

anything we had ever done. It would be very interesting for you at some point if we could go back and pull out the figures to show you, and to sit down and take the budget tables from fiscal year 1984 compared to the budget table for 1986 and 1987 when we were working with a regular budget, a supplemental budget, and two amended budgets in one fiscal year. I could explain to you how those things happen, because what we did was to agree on the amount we needed and ask for it. It was so much that if they told us to, as we expected them to, to take it out of our existing program, it would wipe out the rest of the research on infectious disease and immunology in this institute. It was a gamble that we took.

In discussions with the Cancer Institute and some other institutes, we decided we would all go for this. There was a meeting in Building 1, in which we sat around the table and decided whether to go for this or not, and the other institutes backed out. They weren't willing to put it on the line, even the Cancer Institute. There may be people who will dispute this version of history, but in that meeting our position was that we were not going to back down. We would take this by ourselves if we have to. The NIAID and Dr. Fauci pushed and supported it. The NIAID really pushed for a major increase and it worked. Congress appropriated the money. That was the second amendment in the fiscal year 1986, resulting in the second amended budget which was the single biggest increase at any point. These later increases have been big, but at that time it was a major, major push and it worked. Suddenly the other institutes began to want some of that money because it came back to the Office of the Director of NIH for distribution. We had put together this big program, had come back with the money, and suddenly people were talking about re-evaluating where the needs were. It was very interesting politics at that period because we were the only institute willing to stick our necks out, and then when we got the money, everybody suddenly came along and said they needed some of it. And there were moves on the part of Building 1 to look at the "renewed request" because there were several months between the time we made the push and when we got the money. I think that if you explore some of that from an internal angle it would be very interesting.

Harden: I would like to go on with this a bit. I think it is difficult for the average American citizen to comprehend the idea of \$4 or \$5 billion dollars. Furthermore, you knew that you couldn't cure or prevent AIDS simply by throwing money at it. Obviously, when you put this program together, you had major areas of work envisioned. Could you describe some of them?

Hill: We had identified a number of areas as major foci. There was hope for a vaccine at that time, more I think, than what there is now. Epidemiology, natural history, and pathogenesis—how the disease developed—were

parts of this new program. We made a major push in all of these areas—basic research, pathogenesis of the disease, how the virus causes the disease, more studies on the virus itself. All of those were components of this new program.

What was interesting at that point is that we had no one to run it. AIDS research was scattered throughout our immunology program, extramurally, and our microbiology and infectious diseases program, as well as the intramural program. That's why, in the fall of 1985, we decided that since this was so important to the institute, and since we had received additional money, we had to create an extramural program for AIDS. This concept was developed in the fall of 1985, when we had a transition team that helped put this together. The program was established in January 1986 with two people, Dr. John La Montagne as the director, and Dr. Maureen Myers as the treatment person. You can get a lot of history out of Dr. La Montagne and Dr. Myers about the very early days. Before the AIDS extramural program was established, Dr. La Montagne was the influenza program officer, and Dr. Myers was the antibiotics substances program officer in our Microbiology and Infectious Diseases Program. We all knew that they were the two of the most capable extramural program people we had. We were desperate and needed our best people to get this program started. A number of other people were also conscripted into this program: Dr. John Nutter, who was the head of program planning in the Office of the Director, became the head of the vaccine branch. A number of other people were pulled from here or there. The program started with two people and gradually added more. Now it has fifty to sixty people, and we still need more.

Harden: Would you talk about the relationships among NIAID, NCI, Food and Drug Administration [FDA], CDC—in other words, intragovernmental coordination? There has been some talk about tension between agencies and I'd like to understand better how intergovernmental coordination works.

Hill: I'm not sure that I have a good perspective on this. I think that there are other people from whom you'll get more information because I really have not seen a great deal of tension. I was involved with Tony Fauci and with Ken Sell. The Cancer Institute and Bob Gallo did their thing and we did our thing. I know that there were individual differences between scientists such as Mal Martin and Bob Gallo, but I don't know how it affected us overall. The one point where there was some contention was in the fall of 1986, when we were deciding whether we were going to go for a bigger push. We thought the Cancer Institute was going to go with us, but they didn't. They later reconsidered and decided it was important to them as well. I think the Cancer Institute did make a decision at that point that this was not going to be a major research

commitment. It would be interesting if you could get someone from the Cancer Institute to give you their thinking through this period of time. I think there were signals that their main obligation was to cancer and not to AIDS.

I think that one of the things that permeated this period was not a friction or a rivalry between NIAID and the Cancer Institute but the fact that we think differently. There is a Cancer Institute approach and a NIAID approach. We just do things differently. I think what has evolved is that the NIAID and the NCI work together very well when each designs its own program and does its own thing. When we try to mesh things, it has not always worked well. I don't know the reasons for that, other than the fact that there just seems to be a different approach to dealing with things. It's even more interesting when you realize that we've hired a number of Cancer Institute people to take over parts of our program. Part of it comes down to something that I guess could be criticized from the outside. You'd like to think that everybody has the good of people at heart. That is so, but all the institutes see their individual roles and their missions, and there is a certain amount of self-interest and obligation to one's institute. We always look at it as from an NIAID approach and the good of this institute. I guess there might have been some appearance of conflict. I think it has been greatly overblown, however. The working relationships between individual people in NCI and NIAID have been extremely good, far better, than the public would think. Tony Fauci and Bob Gallo have worked together very closely. We have worked with NCI people on individual areas very well and also on committees.

Harden: I guess one of my concerns reading the literature about AIDS and from talking with some reporters, is this misconception that science is some sort of monolith and NIH is a monolith, and . . .

Hill: . . . that all scientists should have the right answers, that there should not be any sort of disagreement on the approach, that everybody should be working single-mindedly, and so forth. I don't care how serious the problem is; I don't care how good everyone's intentions are. I think everybody's intentions have been good. There is no way that groups of people will not run into some differences of opinion over the way something is done. That often translates into disagreeing or bickering. I don't think there is any country, any program, or any situation that can work without differences. In spite of the fact that AIDS is a major international problem, there is no way scientists can work without some disagreements. The groups that stand on the outside and criticize this—whether it be the Congress, the gay rights groups, or the local medical groups—usually disagree on the way things should be done. How do they expect us to be any different? We are criticized by many different groups, for our approach, but, believe me, they have had their own

setbacks and difficulties because of disagreements in the approaches to things. We all have the same goal. It is natural to have differences in opinion and approaches.

Harden: How would you evaluate the media's presentation of AIDS?

Hill: A number of reporters are very knowledgeable and not prone to panic and sensationalism. But occasionally it does flare up. In the early days of this epidemic, a lot of it had to do with the incredible severity of the disease. A lot of it had to do with the fascination with the fact that this was a disease of gay men. It was extremely sensational; it was a combination of attraction, repulsion, fear, and hysteria. This made any newspaper article on AIDS a front-page article. There were reporters who knew that any sort of article that had anything to do with AIDS was going to get a great deal of attention. A lot of people took advantage of that. There were several reporters, though, who very early on became real experts and did a good job. I think that, overall, the press has served more of a positive than a negative role. It has been able to educate people, but often it has depended so much upon the individual reporter and his or her knowledge. Many of the articles were written by reporters who were not science reporters and who had very little understanding of the basic principles of infectious disease transmission, immunology, and microbiology. Consequently, there was an awful lot of misinformation in their statements. In the early days, it was a matter of having people in the press who had an understanding of AIDS. They were constantly pushing for a sensational article, and we used to have to guard our clinics and labs from reporters who wanted to interview patients. They were there with their cameras. It was very difficult in the early days, when we would have to throw them out. In the long run, they have gotten a little more blase, much more sophisticated, and they have done very well. By the same token, a lot of scientists involved in this area were not very media-sophisticated.

Harden: They normally don't have to be.

Hill: No, they normally don't have to be. Scientists don't have the skills to be looking for all of those twists and turns of the question. That created some problems. There were some people early on, like Dr. Fauci, who have been excellent in using the media to get the right message across and not permitting the media to trick them into saying the wrong thing or having it come out in the wrong way. The media have, overall, exerted a positive influence. I think there have been some examples in which media people were very damaging. The majority of these people didn't know, didn't understand, or didn't take the time to make sure that their facts were straight.

Harden: Would you like to make any other comments or make a long-term prognosis about this disease?

Hill: You know I'm not a scientist any more, and I haven't been for a long time. I'm a science administrator, but by osmosis I hear a lot around here. I think that in the long run, we will have a drug to treat AIDS. I'm very confident that we will have some sort of chemotherapy or chemoprophylaxis. It is a matter of time. Though for people who are ill or about to be ill, the prospect of years of research is certainly not very encouraging, I think we will have a drug that will, if not wipe the virus out, at least keep it latent. I am far less optimistic about a vaccine. Eventually, we probably will have a vaccine that will work against a number of the strains or variations of the virus, but, clearly, that is in the future. The use of a drug as a prophylactic as well as treatment may be a practical thing within the next few years. I think that some sort of improved treatment regimen is not that far off, maybe a couple or three years. Clearly, people are already living much longer on AZT [3'-azido-2', 3'-dideoxythymidine]. We will basically control this disease, I think, but it's going to be a long time before these sorts of treatments and therapies are available for world-wide use, and in the meantime, we're going to have huge numbers of people dying. For the U.S. population that has access to an effective drug, whenever it becomes available, then fairly quickly—I think within the next two or three years—the prospect will be much better. I don't think that AIDS will automatically mean a death sentence, because I think that drugs will be found to treat the disease. I may be wrong, but I'm very optimistic.

Harden: Thank you very much, Dr. Hill.

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