This is an oral history interview with Dr. Arthur S. Levine of the National Institute of Child Health and Human Development [NICHD] on the NIH response to AIDS. Dr. Levine was formerly in the National Cancer Institute [NCI]. The date is 2 November 1989 and the interview was conducted in Dr. Levine's office at the NIH in Bethesda. The interviewer is Dennis Rodrigues, program analyst, NIH Historical Office.

Levine: Prior to the "AIDS era," my own research training and interests were in molecular virology and I had experience with retroviruses in my own laboratory. Moreover, as a clinical oncologist and oncology researcher, I was very familiar with, and I had myself researched, opportunistic infections and lymphomas. I was also knowledgeable about Kaposi's sarcoma via my close working relationship with Dr. John Ziegler who had seen many cases of Kaposi's sarcoma in Africa and had worked there as director of NCI's Lymphoma Treatment Center. When John Ziegler first told me about three gay men in San Francisco who, at a young age, had developed Kaposi's sarcoma–one of them also having *Pneumocystis carinii* pneumonia [PCP]–it was something that I was immediately fascinated by, as was he. Because I was a molecular biologist/virologist, I was able to conceptualize what might be happening in those terms. Thus developed my intellectual interest in what was yet to be defined as a syndrome.

At that point in 1981, I do not believe that there was anybody else on the NIH campus who was familiar with this new entity. I had succeeded Ziegler as chief of the Pediatric Oncology Branch [of NCI] and he was now at the University of California at San Francisco. He came to see these three patients in consultation at the Veterans' Hospital in San Francisco. As John and I began to speak over the phone about these interesting patients and as my interest developed, the idea of a workshop arose. The NCI was the logical sponsor because the presenting manifestation of this new entity was a form of cancer. A small workshop was held at the NCI in 1981 focused on Kaposi's sarcoma.

In the beginning of 1982, I left NCI's Pediatric Oncology Branch and began to work with Dr. Bruce Chabner, director of NCI's Division of Cancer Treatment, in a coordinating and advisory role. In this new position, I attempted to galvanize other people into research and administrative activities focused on the new entity. As I received updated news and became more intrigued, I began to put together what the possibilities might be, with respect to the cause of the illness, its impact on the public health, and its potential epidemic quality. I tried to interest more people on two levels–research and fiscal.

Rodrigues: One of the things I noticed was that you speculated that there might be a

viral origin. You even talked about HTLV since it was endemic to the Caribbean, a site that appeared to have a role in the new syndrome.

Levine: In early 1982, nobody had any idea as to what caused this syndrome. By January 1982, there were ten to fifteen United States patients, all gay men. At this point, I was meeting with Dr. [James] Jim Curran from the Centers for Disease Control and people from the New York City Department of Health. In the early spring of 1982, I went with Bruce Chabner to appear before an ad hoc hearing organized by Rep. [Henry] Waxman in south Los Angeles. I was still probably the most knowledgeable person at the NIH about this entity as it was developing in San Francisco and as it was just beginning to develop in New York City. I began to believe that this syndrome was occurring in epidemic fashion and that it would become a major epidemic. I tried to interest people both intellectually and administratively on that basis.

But there was some resistance because NCI had been "burned" previously by becoming involved in putative epidemics of leukemia and lymphoma. There was a famous Hodgkin's disease cluster in Albany and [Dr. Vincent] Vince DeVita had been involved in that. Out of that experience Vince had come to be suspicious of epidemics of malignant diseases. He felt that such "epidemics" were statistical artifacts and one should not use the word "epidemic" or otherwise arouse public anxiety until one could really confirm what was happening. So I felt myself a little bit out on a limb in that period of early 1982 and I had the sense that the NCI and NIH leadership was perhaps taking a more conservative position than I felt should be taken.

The next thing that happened in this saga is that one hemophiliac was reported to have the disease. There was no common denominator except that hemophiliacs get blood products, and certain gay sexual practices could introduce infectious agents into their blood. I decided at that point that this illness had nothing to do with inhalant aphrodisiacs, which were thought to be the cause up until that point [April 1982]. Before April, most investigators believed that the cause was something restricted to the gay population and that it probably was a drug or a toxicant that was depressing their helper lymphocytes. The likely candidates were thought to be inhalant aphrodisiacs or tanning salons. There was some thought that the ultraviolet radiation in the tanning salons might be suppressing the T-cells. There were actually some experiments to show that the nitrite-based inhalant aphrodisiacs could indeed suppress the circulating T4 lymphocyte count, as could UV light. But as soon as I learned that one hemophiliac patient had developed Kaposi's sarcoma, and *Pneumocystis* pneumonia, I felt it was inescapable that this was a blood-borne disease.

There was a second patient a couple of months later in June, also a hemophiliac, and that really clinched it.

So now we were dealing with a blood-borne disease that was undoubtedly infectious. What could be the organism? From the way in which anti-hemophiliac factor is prepared, it could not have been a bacterium; it had to be a virus. It became very clear in my mind that this was probably a virally-transmitted disease. I began to write and speak about this possibility.

In the fall of 1982, I was invited to give a plenary talk at the first major symposium on this syndrome which was to be held at New York University in March 1983. In preparation for this talk, during the early autumn of 1982, I went through the taxonomy of viruses and tried to eliminate, one-by-one, classes of viruses that could not be the cause. I came up with a small group of viruses that could meet the test of a transmissible agent for this new disease. At this point there was more data including observations of long latency between infection and illness. What I basically came down to as I put my thoughts together for this March 1983 talk, and for several publications, was that this was probably going to be an RNA virus as opposed to a DNA virus. This was on the basis of long latency and immunosuppression–phenomena associated with retroviruses such as FLV [Feline Leukemia Virus].

Having read papers from [Dr. Robert] Gallo's laboratory and the Japanese papers about the human T-cell leukemia virus [HTLV], I was familiar with the epidemiology, which was that adult T-cell leukemia (due to HTLV) was occurring in the Caribbean and Africa. It became evident to me, as I was scanning the CDC reports, that there seemed to be an increase in the number of young patients with Kaposi's sarcoma in Africa–Zaire–and in Haiti as well. That clicked in my mind because it was just like HTLV. HTLV was occurring in Africa and the T-cell leukemia virus was being found in patients there and in the Caribbean and in Japan. Thus, having gone through the taxonomy of viruses and having decided that this was most likely a slow-acting RNA-containing retrovirus, and aware that this virus, like HTLV, was becoming evident in Africa and in the Caribbean, it seemed to me that this must be a closely-related virus. I presented that idea in my talk at NYU. There was a lot of discussion, and I felt reinforced in my notion.

Some time before my NYU talk, in the late summer or early fall of 1982, I called Bob Gallo and asked him if he was knowledgeable about these patients with "a new, funny syndrome," who had *Pneumocystis* and Kaposi's sarcoma, and he said that he was not. He had not heard anything

about it, but was interested in my information. I told him what I knew about the syndrome so far: the transmissibility by blood, the long latency, the immunosuppression, and the presence in Haiti and in Africa. I told him that I thought the cause of this disease was a retrovirus, probably very closely related to the human T-cell leukemia virus. I assume that his laboratory then began to try to isolate such a virus, and indeed they did. I was not aware that [Dr. Luc] Montagnier was doing the same thing at the Pasteur [Institute], because my discussion was only with Gallo; I did not know Montagnier.

Rodrigues: It is a fascinating story. Everything that I have read thus far does not provide that account.

Levine: If I had not made that phone call to Bob, I am sure he would have soon learned of "AIDS" from others, since so many were becoming interested. He might not have begun to work on the virus until a little later, but I am sure he would soon have done so.

Rodrigues: I think it also reinforces a belief that there was a strong interest in this problem. I think that there was an intellectual interest that was affecting change. In some of the more popularized versions of the events that I have read, the NIH is characterized as being unconcerned about this or resistant to becoming involved in any way at all.

Levine: You are probably familiar with the report of the congressional subcommittee on "The Federal Government's Response to AIDS." Much of the NIH chronology appears there in the form of various internal memoranda.

Rodrigues: I do not have a copy of that report but I have seen it. We need to get a copy of that. I am sure that they have a copy of it in [Dr. Anthony] Tony Fauci's office, or [Dr. Samuel] Sam Broder's office.

Levine: Another good source that I might mention is Sandra Panem's book. Have you read that?

Rodrigues: Is it *The AIDS Bureaucracy*?

Levine: Yes. She interviewed me extensively for that book. I reconstructed a lot of this history for Sandra. I think a fair statement to make is that there was developing intellectual interest on this campus by the end of 1982. Nobody was uninterested but there were essentially two problems: one is that no matter how concerned and urgent people may be, it still takes time to put grant monies "out on the street." It is not that easy. There have to

be negotiations with the Congress, the Public Health Service [PHS], the OMB [Office of Management and Budget], and the grants bureaucracy. Research must be supported rationally, fairly, and competitively. You cannot accomplish that two days after you first get the idea to do so. You also have to remember that not very many people were knowledgeable about AIDS in 1982 and the first half of 1983. My thoughts on cause were just speculative. I concluded that this was a viral-transmitted disease. If that were the case, it would be in the blood supply and could cause an epidemic.

But it is not fair to assume that everybody else would come to the same conclusion as I did at the time that I did so, because little data had been published and there was still a small number of patients. I think it is quite responsible for people to be cautious and circumspect. I was a little surer of the likely cause than most were because I was a little more knowledgeable than most were. It took time for people to catch up. That is part of the history of ideas.

Rodrigues: Unfortunately, some people have characterized the NIH as a place where a large number of researchers are walking around waiting for something to happen with nothing else to do, when, in reality, everyone is engaged in their own research, and it takes a lot for people to drop what they are doing and to redirect their efforts.

Levine: Particularly until they become convinced that it is a problem. It is easy for us to be critical, having defined the magnitude of the problem now. But, in 1981-1982, until it became clear that this was a blood-borne, probably virally transmitted, disease, it was very difficult to know in which direction to go.

Rodrigues: Let me ask you another question. This goes back to the administrative side of things. Was the first thing the identification of the need for this workshop?

Levine: It was mostly people who knew about Kaposi's sarcoma. It was a rather small group of people because, again, we were struggling. We had no idea what was causing this situation.

Rodrigues: Another group was set up at the NIH, a standing body of NIH staff which Bob [Dr. Robert] Gordon was chairing.

Levine: That was afterwards. That also happened sometime late in 1982, after the NCI workshop.

Rodrigues: From what I can tell, going back through the records, those were the first two coordinating efforts, as far as some administrative action on our part.

Levine: Yes. I was trying to coordinate things for the NCI. We still thought this was mainly a cancer problem. It was not until quite a bit later that it became evident what the putative viral etiology was and all that meant for the public health. People began seeing it as a problem for the Heart Institute [National Heart, Lung, and Blood Institute] because of blood transfusions, and for NIAID [National Institute of Allergy and Infectious Diseases] because of viruses and immunosuppression, and so forth. Actually, when one thinks about it, the NCI workshop in 1981 and the formation of Bob Gordon's task force in 1982 were pretty early formal responses–very early responses.

Rodrigues: Especially considering the apparently limited magnitude of the problem at that point.

Levine: Right.

Rodrigues: There were, of course, a number of other administrative initiatives that occurred early on, too. Apparently the NCI was providing supplemental funds to set up its "AIDS-related" grants and contracts.

Levine: Right. Most of that came out of the Division of Cancer Treatment, which is where I was; Bruce Chabner was the director and I was functioning as his advisor. We tried to do as much as we could, given the usual bureaucratic constraints.

Rodrigues: I think we have probably covered a good bit of ground in this interview.

Levine: You may be interested in a confidential memorandum that I wrote to the NCI Director, Dr. DeVita, in June 1982. I wrote, "It is apparent that this epidemic is growing by the day, no longer involves one group exclusively and, in fact, is spreading to other subsets of the population. It is also becoming increasingly likely that there is a transmissible infectious factor in the syndrome and that it is mainly sexually transmitted. It seems to me that this problem should involve all of the NIH and not just the NCI and that money should be identified in excess of the one million dollars, which is the current expenditure, to facilitate a most urgent response. I believe that an NIH/CDC task force should be funded with whatever dollars are required by these agencies to pursue this effort and funds should be made available now." The congressional report, which published my memo, offers a comment about NIH's responding to this by not taking a fairly active role. But, in retrospect, I think that is unfair. I think we may want to use your report to clarify the record because, given that I had these feelings, they

really reflected my scientific intuition and judgment more than any available data. I do not believe that it is fair to allege that either De Vita and/or [Dr. James] Wyngaarden were dilatory.

Rodrigues: Thank you, Dr. Levine.