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STATEMENT OF FRANCOISE BARRÉ-SINOUSSE, PH.D.

My name is Francoise Barré-Sinoussi. I am head of the Retroviruses Biology Unit at the Institut Pasteur (IP), Paris, France. I also am a member of the IP scientific team that in 1983 discovered the virus that is the cause of AIDS. I have participated in collaborative research activities with National Institutes of Health (NIH) scientists for over twenty years, including, beginning in 1983, certain collaborative research activities on HIV/AIDS, with Dr. Robert C. Gallo and his colleagues, at the Laboratory of Tumor Cell Biology (LTCB), National Cancer Institute, NIH.

I read, comprehend, and speak English fluently. Following is my account of certain events of which I have first-hand knowledge:

I. During April 1983, with my colleagues Drs. Luc Montagnier and Jean-Claude Chermann, I wrote a paper reporting the discovery of a new human retrovirus isolated from a patient with lymphadenopathy. This is the virus we later would call LAV.

I was told by Dr. Chermann and/or Dr. Montagnier that Dr. Gallo said he would arrange for publication of our paper by the journal Science. I was also told that Dr. Gallo said he had papers ready for publication, and he wanted our paper to be published with his. I was told Dr. Gallo said that his data were "in the same direction as ours," and our understanding was that Dr. Gallo thought having our paper published in the same issue of the journal as his would make his paper stronger.

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Dr. Gallo pushed us to complete our paper as rapidly as possible and transmit it to him, with the understanding that he would submit it to Science. However, in the rush of events, we forgot to prepare the abstract necessary for our paper to be submitted to Science. When Dr. Gallo received our paper, he called Dr. Montagnier ^{AND} Dr. Gallo informed Dr. Montagnier about the missing abstract, and Dr. Gallo proposed to write the abstract himself. Because of the urgency of submitting the paper, Dr. Montagnier agreed to this plan.

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Sometime in the latter part of April 1983, Dr. Montagnier asked me to sit in on a telephone conversation he had arranged with Dr. Gallo to review the abstract Dr. Gallo had written. At the time of these events, my colleagues and I did not have a fax machine available to us. Consequently, we were not able to review a draft of the abstract. However, Dr. Gallo said he would review the abstract with us by telephone.

During the telephone call with Dr. Gallo, I sat in Dr. Montagnier's office and listened to the amplified conversation. Because of the nature of the telephone equipment,

I was not able to enter into the conversation myself, although Dr. Montagnier and I attempted to communicate by gestures. My recollection is that we generally said "okay" to the abstract. This was a mistake on our part, because, as we later discovered, the abstract contained statements that misrepresented our work. I believe the mistake occurred, in part, because Bob Gallo was speaking very fast, and, in retrospect, I'm not sure we really caught what had been written.

In addition, concerning the sentence Dr. Gallo added to the text of our paper, I do not remember Dr. Gallo saying anything to us about the addition of this sentence. An associated matter, relating to both the abstract and the added sentence, was Dr. Gallo's use of "HTLV" to stand for "human T-cell leukemia virus" instead of "human T-lymphotropic virus." To the best of my recollection, during the telephone conversation with Dr. Gallo, we said we did not agree with calling our virus "HTLV" (meaning "leukemia,") although we did agree with calling it "HTLV" (meaning "lymphotropic"). But later we learned that both in the abstract and in the added sentence, Dr. Gallo said our virus belonged to the "HTLV" family, with "HTLV" defined as "human T-cell leukemia virus."

Later, in May 1983, when I spoke at Cold Spring Harbor and then, at the LTCB with Gallo himself, I found that Dr. Gallo strongly believed our virus was an HTLV (meaning a member of the leukemia virus family). After Cold Spring Harbor, when I was visiting Dr. Gallo, we went for a meal, and we were discussing the IP data on LAV. On this occasion, Bob told me, "Francoise, if it's (the IP virus) not an HTLV, I will just forget it." I found it curious for a scientist to make such a statement. To me, it represented a closed mind. At this time, listening to Bob Gallo speak, it was clear to me he was completely convinced that the AIDS virus was, at least, an HTLV (leukemia)-like virus. It was not even possible to engage him in a conversation about any other possibility.

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II. During the week of November 13, 1983, I attended a meeting on "Manipulation of Host Defense Mechanisms," organized by Dr. T. Aoki, held in Tokyo, Japan. Also present at this meeting were Drs. Robert Gallo and Flossie Wong-Staal. During the meeting, I presented a paper, coauthored with Drs. Luc Montagnier and Jean-Claude Chermann, titled "A new human retrovirus associated with acquired immunodeficiency syndrome (AIDS) or AIDS-related symptoms." (Later, in 1984, this paper was published in a volume with the same title as that of the meeting.) We called this new retrovirus "LAV" (lymphadenopathy-associated virus) or "IDAV" (immune deficiency-associated virus).

The paper I presented reported our work isolating, propagating and characterizing the new human T-lymphotropic retrovirus that we believed could be involved in AIDS. In addition, the paper reported the results of viral antibody blood testing for the new virus; specifically, we found viral antibodies in 37.5 per cent of AIDS patients and 74.5 percent of patients with lymphadenopathy or pre-AIDS.

On my way from the Tokyo airport to the hotel in which we were staying during the scientific meeting, Dr. Gallo and I shared a taxi. During the ride to the hotel, Dr. Gallo told me, "I think we have found your virus." Dr. Gallo said he had obtained an isolate very similar to our virus. He said he had electron micrographs (EMs) showing "his" virus had similar morphology to ours. He also said he and his colleagues had confirmed the cytopathicity of the virus, and had shown the virus was recognized by AIDS patients' sera. Dr. Gallo said he and his colleagues were working on developing specific reagents for the new virus; however, he did not mention that he was growing the virus in a permanent cell line.

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III. During the second week of August 1992, I attended the annual LTCB laboratory meeting, at the Hyatt Regency Hotel in Bethesda, Maryland. On one occasion during that week, in the evening, I was waiting to board a bus that was to take meeting participants to dinner. While I was waiting for the bus, Dr. Mikulas Popovic, whom I have known as a scientific colleague for many years, was standing close to me and we started to speak together. We entered the bus and sat together through the entire trip to the restaurant.

I asked Dr. Popovic how he was. He responded with something like, "how do you think I can be, with this thing going on?" I understood this to be a reference to the still-ongoing investigation of possible scientific misconduct involving Drs. Popovic and Gallo.

Dr. Popovic began to speak about his paper (Popovic et al., Science, May 1984), which was the focus of the scientific misconduct investigation. Dr. Popovic said he had to write the paper in a great rush; he left the manuscript behind when he departed the laboratory to attend an out-of-town scientific meeting. Dr. Popovic said that when he returned, he found many notes written on the manuscript. In addition, much of the original manuscript, in which Dr. Popovic acknowledged the IP work, had been thrown out by Bob Gallo.

Dr. Popovic told me he had proof that he had tried to give credit to the IP; he said he had sent a copy of the manuscript to his sister in Czechoslovakia. He said he could show me the proof,

and he seemed very agitated about this. I tried to assure him that I believed what he was telling me. Dr. Popovic then said that he was "not responsible" for most of the things that were done. I responded that there was one thing for which he was responsible, namely, that he mixed LAV together with other AIDS viruses that he grew in his pool.

Dr. Popovic responded by telling me that he mixed LAV with the other viruses to increase the capacity of growth of the virus. He said he was successful in doing this, and I responded, "Yes, but what you got was LAV" ("HTLV-IIIb," the LTCB "pool" virus has been proven to be LAV/LAI [Chang et al., Nature, 363, 1993, pp. 466 - 469]).

Dr. Popovic's answer to this was that at the time he did not know it was LAV that would grow out of the pool. We then discussed the things we did not know about the AIDS virus, at that time, particularly the variability among isolates. Still, I told Dr. Popovic he should have been more careful in his use of LAV, i.e., he should have known LAV would grow better than his other "isolates" because it had been grown for quite some time at the Institut Pasteur, and thus was adapted to growing in culture.

At this, Dr. Popovic became angry. He told me he did not appreciate my criticism; that he is a well-known retrovirologist and previously used the pooling technique, in his work with avian retroviruses, in Czechoslovakia, to help the viruses grow. Dr. Popovic also said he had other individual virus isolates (he mentioned "RF" in particular) and thus, had no reason to use LAV (other than mixing it with the other viruses in the pool). I told Dr. Popovic I understood all this, but I still felt he should have been more careful with his use of LAV.

At this point, Dr. Popovic stopped talking to me; however, I reinitiated the conversation, because I felt sympathy for him in his current situation. I told him he should remember that the past is the past, and there are many things that are important outside of our work. I urged him to try to take off some time from work to think about other things for a while. Dr. Popovic responded by saying he agreed with me, but, he said, "I cannot." He repeated this several times.

Dr. Popovic did not say if he told Dr. Gallo about his (Popovic's) mixing of LAV and the other viruses in the pool.

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IV. In October of 1993, shortly before the scheduled beginning of the hearing of Dr. Gallo's appeal of the scientific misconduct finding made against him by the Office of Research Integrity (ORI), Dr. Gallo was in Paris, and he asked me to have lunch

with him. ~~It was not usual for Dr. Gallo to ask me to lunch,~~
and when he began to speak, I realized that he wanted to talk
about the upcoming Appeals Board hearing.

Dr. Gallo asked if it was true that I would testify at the
hearing as a witness for ORI. I said it was true, and he asked
why I would do this. I told Dr. Gallo that both the French
Embassy and IP officials had encouraged me to testify. I also
told him that both Drs. Montagnier and Chermann had agreed to
testify for ORI, and with those two testifying, I felt I could
not myself refuse.

Dr. Gallo told me that if I testified it might not turn out
well for me. He said that some of the ORI witnesses in the
earlier appeal hearing of Dr. Popovic had been made to look
ridiculous. I told Dr. Gallo I am not an aggressive person,
that I wanted this matter to be over, and that while I intended
to answer truthfully any questions that were put to me, I did
not intend to go beyond those questions in making my responses.

Dr. Gallo said I might be contacted by his attorney, and I said
"fine." However, I was not contacted by Dr. Gallo's attorney,
and within a few weeks of my conversation with Dr. Gallo, ORI
decided to withdraw its misconduct finding against him, so no
appeal hearing was held.

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I swear (under penalty of perjury pursuant to the laws of the
United States of America) that the foregoing account is true
and correct, to the best of my knowledge and belief.



Françoise Barré-Sinoussi, Ph.D.

December 14, 1993