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Paris, July 14th, 1984

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Building 37, Room 6A09
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Dear Bob,

I acknowledge your letter of July 25th. I am writing to complete the list of material exchanged between our labs in 1983 and 1984, partly mentioned in your letter, and to reestablish the truth with regard to some of your statements published in interviews given to the press.

I. Exchange of material

A. From NCI to Pasteur :

1. T-cell growth factor : received in 1980 (document enclosed). Not used for retrovirus isolation from AIDS patients, but used in 1982 in search for another retrovirus possibly associated with breast cancer (enclosed copy of our publication in BBRC, acknowledging your gift of TCGF). TCGF for LAV isolation was obtained from Dr. FRADELLI, Hôpital Saint Louis, Paris, as it is acknowledged in our Science paper of May 1983.
2. HTLV1 reagents received in March 1983. Acknowledged in our Science paper, May 1983.
3. HTLVII cell line taken by F. Barré-Sinoussi in June 1983. Cells were not alive upon arrival.
HTLVII clone : received in April 1984.
4. HTLVIII cell line brought by Dr. SARNADARAN, May 15th, 1984 for comparison with LAV. Patient sera (40 μ l each) and rabbit antisera.

B. From Pasteur to NCI:

1. Virus LAV : July 1983 (you were not able to grow the virus)
Virus LAV : September 1984 (you were able to grow the virus and confirm EM morphology.

Not acknowledged in your publications.

2. Anti human α interferon serum requested by you and sent to you four times. (July, September, December 1983, March 1984).
(Enclosed a copy of a letter in December 1983)

Not acknowledged in your publications, although your collaborator MARKHAM showed in a meeting recently held in Montreal a dramatic effect of antiinterferon antibodies on HTLVIII propagation in normal T-Lymphocytes, as we found earlier for LAV.

3. Serum of AIDS patients (BRU) from which LAVI was isolated : July 1983, September 1983.
4. DNAs : - from HRU lymph node
BRU cultured T-Lymphocytes, March 1983
- from lymphocytes in vitro infected with LAV, July 1983
- c-DNA (crude endogenous reaction of LAV), May 1984
(includes a majority of cellular DNA sequences, which is expected in a crude endogenous reaction).
5. ELISA plates containing antigens.

II. Your attitude as scientist and responsible of the NCI task force on AIDS :

1. Having LAV in your Lab since September 1983, the normal way would have been, when you had made your own isolates, to compare them to LAV, as we did in March 83 with HTLVII.

The thesis that you did not use our LAV because you did not want to be accused of contaminating your own isolates by ours, seem to me very weak : your lab is sufficient large to accomodate independent isolates growing in several independent rooms. And, I believe, you are confident in the experience of your collaborators. Even in March 1984 you refused to answer my question on the relatedness of the proteins of HTLVIII to those of LAV.

2. Until February 1984 (UCLA meeting) your attitude toward our findings was to minimize their importance or to demolish them, *be very critical.*

After I presented the first data on LAV morphology and new isolates from AIDS patients in July 18, 1983, at the NCI meeting organized by you, the three pages-long report made by you only mentioned my presentation by a single sentence : "HTLV type 3 is a single isolate by the French group from an AIDS patient".

At the Cold Spring Harbor meeting on September 15, 1983, organized by you, you questioned the retroviral nature of my isolates rather than stressing the interest of the data I had just presented and which are now, at last, published. The original publication schedule established by the Editorial Office of Cold Spring Harbor Laboratories was end of January 1984. The data showed for the first time the T4 tropism of the virus, its cytopathic effect (giant cells) the first serological data and described 3 more isolates (Kaposi sarcoma, hemophilic, Zairian) suggesting that the virus was universally present in the major groups at risk of AIDS. You also told me after my presentation at this meeting that you will spent 6 month

to check whether or not we were right, and that if we were right, you would say so. Eight months later, appeared your Science papers in which our work is mentioned by a single sentence at the end of one paper, suggesting that our virus was insufficiently characterized. It is not true that our virus was poorly characterized. Since September 1983 you had already in hands our manuscript for the CSH Book and two papers (Lancet April 1984, Annals of Virology, March 84) were published before yours and from what I have read in your Science papers, the viral proteins of HTLV III were not better characterized than those of LAV.

At the Seillac's meeting and the ARC meeting in Paris, October 1983, you questioned the viral nature of the main antigen (p25) recognized by the patient's sera and the interest for the Pasteur Institute to use it for a blood test.

3. Newspapers and magazines :

In several interviews, you have said that some of our people were trained in your laboratory to grow T cells and make HTLV isolation. This is not true. Françoise Barré-Sinoussi stayed in your lab for 6 weeks in 1979 but did not work at all on T cells and HTLV. Her stay was part of a collaborative work between our labs to study the effect of antiinterferon serum (our earlier work on murine retroviruses) on production of Primate retroviruses in B lymphoblastoid cell lines;

I have a 25 years experience in cell culture and virology and I did not need training for culturing T cells. The isolation of the first four L.V. viruses was made by myself, and was a logical, carefully planned, achievement. At any rate, I was not "started off" by you.

Worst : your interview to the Medical Post, which includes statements on my group which are unbelievable from a responsible scientist talking about other scientists and ... "friends", "friends". This is too sad and does not deserve any more comments.