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Paris, June 4th, 1991.

Our ref. : LM/em 91.1194

Robert C. GALL
National Institutes of Health
National Cancer Institute
Building 37
Room 6A11
Bethesda, Maryland 20892
U. S. A.

Dear Bob,

I received today your invitation to give a special lecture at your 1991 lab meeting.

I appreciate that you make this invitation, given the difficult relationship existing between us.

However, I regret to decline it to express my discontent about the multiple untrue or uncorrect accounts of our past relationship in your book "*Virus hunting AIDS, Cancer and the Human retroviruses*". Among them :

1/ You say page 146 that "I know little about retroviruses and that at the NCI Task Force meeting on AIDS in July 1983, you had to explain to me what was a lentivirus.

You should know that I started working and publishing on avian retroviruses in 1965 and that I have published between 1965 and 1979 32 publications and review articles on animal retroviruses. Moreover, since 1971, I have been teaching retrovirology at the Pasteur Institute and I was the first to show homology (EM pictures and antigenic cross-reactivity) between LAV and equine infectious anemia virus. I remember having shown to you in your home in July 1983 my recent data on the ELAV homology and remember that you did not pay much attention to it.

In your report of the Task Force meeting of my presentation which involved typical lentivirus pictures, you simply stated "that HTLV-III is a single virus isolate from the French group" and I sent you a letter of protest against this misreporting.

2/ You say page 147 that thanks to you the LAVBRU virus could be propagated and saved on chord lymphocytes. This is untrue, and I already told you that before.

I did myself the first cultures of BRU lymphocytes (as well as LAI, DL, ELI, etc...) and then gave the supernatants to Françoise Barré for RT detection. When the first culture of BRU was exhausted, I passed the virus to PHA-activated lymphocytes of an adult blood donor. The virus grew well again. Some following passages were made on chord blood lymphocytes but this was just an alternative to the choice of cells from adult

June 4

3/ You say page 157 that "I broke off our collaboration in July 1983" because "it all must be done in the hexagon". This is untrue. I never said that.

On the contrary, we sent you again virus samples in September 83 and anti-interferon serum in December 83 at your request, and were eager to receive any news about the use of these reagents in your laboratory.

I just received an ambiguous sentence from M. Popovic in December 1983: "We know how to grow your virus". And no support from you of our conclusion that LAV was the best candidate to be the cause of AIDS before your 1984 publication.

4/ You say that in our phone conversation of June 84, I did not clearly answer your suggestion that there was a contamination.

This is untrue. I replied to you that if there were to be a contamination, it had to be one way: III-B contaminated by LAV.

This proved later to be real.

5/ You say page 209 that in the dinner at Claudine Escoffier-Lambiotte home, François JACOB proposed to you for the Pasteur-NIH agreement, a split of 1/3 for Pasteur, 2/3 for NIH. This is untrue and François JACOB expressly denies it.

Unless you retract publically these false assertions, I do not feel like resuming any kind of good relationship with you.

Of course I still place the interest of research on AIDS above these considerations. However, since there are several scientific meetings in which we both participate each year, I do not think it is vital that I attend your September meeting.

Sincerely yours,



Luc Montagnier

P.S. : My first name is *Luc*, not *Jean-Luc*.