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Paris, January 21st, 1985.

Dr. Ludwik GROSS
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U. S. A.

Dear Dr. Gross,

Thank you very much for sending me first, reprints of your outstanding work in the leukemia virus field and then the reprint of your paper on the HTLV symposium accompanied by your kind letter.

I knew you had been working at the Pasteur Institute before the war, but I did not know your stay lasted for six years. I read with great interest your reprints, some of which I was not aware of. I particularly appreciated reading the interview you gave to Marcel Bessis, in which you talk about your difficulties with your colleagues in the discovery of the first leukemogenic virus of the mouse. It reminds me my own experience of the last two years : we also had difficulties in convincing the scientific community that we had isolated the causative agent of AIDS : difficulties to publish in journals with an international audience, spiteful cricisms, even ridiculization in scientific meetings. Our fault was to be right too soon against the HTLV trend.

By September 15, 1983, we had accumulated experimental and serological evidence that LAV was the best candidate. I presented the data at the HTLV meeting at Cold Spring Harbor, at the last night session, while half of the participants had already left. My 20 minutes presentation was followed by a flood of crticisms, some of them questionning my competence in retrovirology. I had the impression to be the bad guy to derange right-minded people (HTLV). Six months later, the very same critics confirmed our discovery, but only after changing the name of the virus. This recalls your story about the polyoma virus discovery ...

I had given my manuscript to the Editor the very same day of my presentation, believing that it would be published at the announced date, January 84. As you know, it was published in June 84, after the Science publications on HTLV-III.

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Now, all data are available on this virus, including the complete molecular sequence. It is clear that LAV is the prototype of a new group of human viruses, not related to the HTLV-I and II. You will see from the enclosed preprint on the sequence that the most conserved gene, *pol*, is more related to Rous Sarcoma Virus than to HTLV-I ! Calling this virus HTLV-III is misleading, since it means a strain of the HTLVs (cf. Adenovirus type 1, 2 ; Herpes type 1 and 2, etc ...). Some of viruses related to LAV are probably involving in other pathogeny, such as brain diseases or cancers.

Before isolating LAV, I was on the track of another possible human retrovirus, related to MMTV, which would be present as DNA sequences in some breast cancers and in the T-lymphocytes of the patients (cf. reprint). I believe that we are at the beginning of a renewal of the research on human retroviruses, in which you had a now incontested initiator role.

I do not underestimate also the important part played by R. Gallo and, as you say, his talented co-workers ; they have opened the road with HTLV-I and this makes me sad in view of his behaviour over the discovery of the AIDS agent.

In the review you have written on the HTLV meeting, I believe that you have been misinformed on several points :

1/ We first discovered the LAV and showed its role in AIDS, before HTLV-III (last paragraph of page 2).
LAV, or HTLV-III, which is identical to LAV, is <u>not</u> related to HTLV-I (see enclosed preprint).

2/ page 2, 5th line from bottom : this virus proved to be closely related to HTLV-III. This should be stated the other way round HTLV-III proved to be related to LAV.

Last sentence : Gallo did <u>not</u> supply TCGF for isolation of LAV. He sent us 2 years before a crude TCGF preparation, which was used for growing T-cell from breast patients (he is acknowledged for that gift in the corresponding reprint). For LAV isolation, we used purified TCGF made by one of our French Colleague, D. Fradellizi. He is acknowledged in the Science paper.
Likewise, we do not use Gallo's line for growth of our virus. We use for B and T cell lines, different from, but as good for virus production than H9. We asked him his line only for comparison between LAV and HTLV-III.

3/ Finally, I regret that you did not mention the presentation by a member of our team, Dr. Simon Wain-Hobson, at the NCI meeting. This was the first report on the entire sequence of LAV which shows an unique genome structure, with two new open reading frames and no homology with HTLV-I and -II.

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Of course, you have the right to take or not to take into account these remarks. But I should be very grateful to you if you could still modify your text on the galley proof. If this is not possible, I shall still have the possibility to write to the Editor of Cancer Research, after publication, for the points 1 and 2.
I regret not having talked with you on these matters at the NCI meeting.

Perhaps I will find another opportunity to meet with you again and to tell you more about the LAV story ! A very strange affair where extra scientific factors are playing a great or even greater part ...

With best regards,

Sincerely,

L. Montagnier

Encl.