



DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
National Cancer Institute
Bethesda, Maryland 20208

Bldg. 37/Rm. 6A09
(301) 496-8007

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Professor Gerard Orth
Institut Pasteur
28, Rue du Dr. Roux
75724 Paris Cedex 15
FRANCE

Dear Gerard:

As you and Marc Girard requested in Park City, I am providing the following information. "What did I do with the virus of Montagnier?" My answer (see the enclosure) was based on the interpretation that you people were pushing the question whether we "took" "his virus." I reacted to this insult in a way that I felt was complete, appropriate, and conclusive. However, now I have reason to believe that this question was also asked in a manner that is more subtle, technical, and quasi-legal; i.e., we used his virus to learn something which helped us for the blood test. I did not address that question. I will address it here and now.

There is no question we received and temporarily passaged LAVI in the late fall of 1983. We signed a paper to that effect. We did what Montagnier and I agreed we should do! To try to analyze it and convince ourselves that they had something credible. We did this and, as I have said, we found it could be transmitted (i.e., was a virus), had reverse transcriptase, and differed from HTLV-I and HTLV-II. Isn't this precisely what we agreed to do? Why else did Montagnier send the virus? Why did he then write a letter to Nature stating he sent it to us? Why do some people (or at least Montagnier) assume some wrong was then done? What is the wrong? If we learned something so what? Our work was completely and totally independent of his. It is true I cannot unequivocally swear I didn't learn something useful from it. I assume this is in the nature of science and life. However, I am far from sure we learned anything useful to us, to AIDS research, or for the blood test. It is true that we confirmed transmission (temporary) to cord blood T-cells. It is also true that when we did not have a source of cord blood T-cells we temporarily passed it in the HUT 78 cell line. We succeeded in getting low levels of virus, but then it was finished. Certainly, that was not a misuse, as Montagnier has slandered us in newspapers. Moreover, I am sure we did not put it in the H9 clone. I am also sure that much later when we, in fact, tried to see if it would grow in H9 (February 1984), with Chermann's knowledge and in order to collaborate, we could not grow it in H9 under our best conditions and on repeated attempts probably because of the limited amount and we could not repeat growth in HUT 78.


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Let me put the problem in another way. Let us for the moment take a hypothetical situation. Let us say we were not the ones who broke the back of this problem, were not the ones who first succeeded in mass-production of the virus, and were not the ones who developed a truly successful blood test first, successfully patented. Let us say Montagnier's patent was immediately accepted. Would Montagnier then say to NIH there should be a sharing 50-50 in the patent since (1) the idea of a retrovirus causation of AIDS came from Gallo; (2) the technique to grow T-cells came from Gallo; (3) Gallo sent the original IL-2 to Montagnier so he could "practice" and learn how to grow T-cells; (4) Gallo sent reagents for HTLV-1 and HTLV-2 so that the new candidate virus of Montagnier could be checked to determine that it was unique? Obviously, these early contributions from us surely taught them something relevant for their work. This cannot be debated. Yet, I wonder if I am wrong to assume that Montagnier would offer anything to anyone if the hypothetical situation were true? I would love to know your honest opinion.

Finally, I am going to tell you something I have not discussed before and which I would like to forget. When our work first came out before any detailed molecular data were available on LAVI or our isolate, a scientist known to loosely collaborating with me and having some of my isolates was approached by an individual from a commercial firm collaborating with Montagnier. The commercial person made a proposition to my collaborator along the lines of the dirty road we are now walking; i.e., he said "What if the viruses could be shown to be almost the same? what if . . . ? what if . . . ? after all, didn't Montagnier send some virus to Gallo . . . etc.?" He then made an offer of "collaboration" with clear implications. I can assure you there are people here who can demonstrate this to be the case. In short, these antics were going on from day 1 at least by one individual.

Regards.

Sincerely yours,



Robert C. Gallo, M.D.

RCG/DJ

Enclosure