



DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
National Cancer Institute
Bethesda, Maryland 20205

Building 37, Room 6A0
(301) 496-6007

August 24, 1984

Dr. Jean-Claude Chermann
Head of Laboratory
Oncology Viral Unit
Institut Pasteur
28, Rue Du D'Roux
75724 Paris, Cedex 15
France

Dear Jean-Claude,

Thanks for your letter of July 11. I will respond to each of your points:

1. The decline in communication, and trust, and quite frankly friendship, as far as I am concerned, between you and me began with a few developments specifically from you. First of all, there was the CDC problem, and second, there were your rather outrageous comments in the Press. Maybe you did not make all of them, but I just can't believe that they are all misquotes. I have a file during the first two weeks of that horrible period where my only comments were "no comment". Only after hearing protracted abusive comments did I respond. I can prove several statements attributed specifically to you that were in my view malicious. None can be attributed to me until some weeks later. Third, I spoke to you of one personal problem at NIH. You assured me that the individual in question did not have your virus and would not. Very shortly after I found out he did, and that collaborations were underway.
2. The sample of the virus you sent us the first time had, upon arrival, no detectable virus; no intact detectable RNA, no biological activity. The cDNA we got was 100% normal human cellular sequences! This was documented by two different qualified people in this lab. We wasted two weeks on this. The lymph node DNA you sent to us was indeed received, very small in amount, and as you know, used for HTLV-1 hybridization which under stringent conditions was negative. You got these results.

3. I appreciate your acknowledgment of my quick response to your original requests for anti-p24, anti-ol9, HTLV-I cell lines. I appreciate your gratitude. You should also know HTLV-I and HTLV-II molecular cloned probes were also sent. Also some purified human TCGF (IL-2) at the onset.
4. I appreciate your acknowledgment of my stimulating you to publish. I also appreciate your acknowledgment and gratitude that I acted as the supportive referee for your original paper.
5. You are correct. You told me of your experiments, and I was supportive of you as a person and scientist. I too told you of our experiments. No doubt, those were good, useful experiences for me. I acknowledge to anyone that your careful studies on the cytopathic effects were original and influential.
6. If the HTLV-II cell line was dead on arrival you should have called and told us to send it again. No one else in this situation ever did otherwise.
7. I am happy to receive your apology for what you said to Flossie publicly at NIH in denying that you received HTLV-I cloned DNA.
8. You say you are waiting for sera from New York and New Jersey which I promised to send to you. I acknowledge I said this to you, but let's get serious. Those sera are not valuable rare reagents. You went ahead with CDC and others. Like you, I have sera from physicians only secondarily and probably far less than you have through your friends a CDC.
9. You describe as a "thorn" in our "collaboration" that when you asked Mika Popovic if he had succeeded in growing LAV in our lab. Mika replied, "I cannot speak: Only the boss can speak". As you know, I wasn't able to be present at that moment, and I cannot totally control every comment. However, I can tell you this: Mika told me that Francoise said to him, "Gallo said you were growing LAV". Now, I can speak for Gallo, and he never told Francoise anything of the kind. Mika knew that the data we had on HTLV-III was still in the process of being written for publication. Quite frankly, we were not collaborating with you, and therefore, we did not feel a need to tell you details of this work. Mika tells me it is only in regards to HTLV-III independent work by us that he, quite appropriately at that time, said he shouldn't be openly

discussing this with you. Moreover, I do not even think it was appropriate for you to ask him. He said the question about LAV was never asked of him by you. Now regarding that point, I took considerable abuse in newspaper quotes from you that "you sent LAV to Gallo; where is Gallo's virus"? Also, "why didn't Gallo compare them?" Etc., etc. Well first of all, I shall reiterate what I have tried to tell you from the beginning. First, we did not grow LAV. We confirmed your transmission and cytopathic effect. To keep it going in production would have required considerable work (which we would have been doing for you, not us. I work for NCI not you. Second, clearly we would be accused by many of cross-contamination, i.e., we would run the risk of not being able to convince the "tough" that we had independent isolates. Finally, how could we make a comparison? It seems to me that you have not sufficiently appreciated that to make a comparison one needs reagents, antibodies, cloned probes, etc. At that time, we did not have any! You did not have any. In this respect, even now at this stage with all the collaborators you have, you have not yet described a single reagent in the literature. Why in God's name would you change the name of LAV yourselves each time you isolate it, i.e., RUB, LAV, IDAV1, IDAV2, etc.? Obviously, because you yourselves have not even yet compared your own isolates in a manner which yields unambiguous results. I suppose this is because you don't have money. I have read some of the quotes from you and your colleagues on this, too. In fact, for the first year I had about two and one-half people part-time on AIDS, and a rather small AIDS budget. Yet quotes from you, consistently implied we has so much and you so little.

10. You are puzzled (and obviously were mad) by the fact that you did not get the HTLV-III cell line, but Gazzolo and Zaqury of France got it. Why should you be? They were working and publishing with me on AIDS; you were not. You chose to work with CDC. Get a cell line from them. When you finally got a cell line (B-cell line with LAV) did you send it to me? No, it went to CDC, XYZ, ABC, etc., first, even to U.S. companies. However, you were among the first people in the world to get our HTLV-III cell line, still the best producer in the published literature.
11. Regarding the uninfected cell line, you say if I am interested in the comparison, why don't I send it! Now, good God, how does the uninfected cell line affect comparison between LAV and HTLV-III? All I can think of is protein precursor size during virus protein maturation. However, THIS, of course, is no way to compare.

We already did (with Luc) the competitive RIA's, and as you know -- they cross-react nicely. That means they are highly related. The final answer is molecular analysis, clone upon molecular clone -- genomic homology. Now, since I sent Sarang to Paris and paid for his trip, and since Sarang brought our antibody and you had none, I felt it only reasonable that Luc or a co-worker would come here with your putative molecular clones. I offered to even pay for the trip and all local expenses. Montagnier told me everyone is and was too busy to travel. I must say I was surprised by this because there was no shortage of time by Luc, you, Francoise, etc., in running around the U.S. to lecture (three NIH visits in a few weeks isn't indication of people short on time).

The uninfected HT line is not yet generally available because of our desire to have all its' features characterized in detail and published. This will occur this fall. Also the U.S. Government position is to hold this line until then. In any case, you have our infected line and I have Luc's. Therefore we each will isolate DNA and probe with our respective clones. If there is strong homology, we will write a paper combining this and the RIA data. However, there are some problems. Our results with your line show very few cells producing virus. Obviously, you haven't cloned the line. This of course, does not help with molecular hybridization experiments. In any case, I have told everyone I believe LAV and HTLV-III to be the same. What do you want of me? Don't use this argument to get the uninfected HT cells. We will send them to Pasteur Institute soon, but when I think it is scientifically and administratively appropriate.

12. Don't be confused about the date of our first isolate of HTLV-III. It was indeed November 11, 1982. We had three or four more in February 1983. We did not, however, know what they were, only what they were not, i.e., HTLV-I or HTLV-II. We did not know each isolate was one and the same virus. We needed reagents for this which was later solved by the mass production (cell line). I did not (nor did anyone else in this lab.) appreciate the cytopathic effect of the virus as early as you did. We simply thought we couldn't grow the cells because they were in bad shape or too few in number so we froze them away, often only with RT data, sometimes confounded by rapid and somewhat crude EM. We did not appreciate the similarity to the LAV published picture until January or February 1984.

13. I am happy you have had such good exchanges with the CDC and discussions. I hope they will continue just as I hope you have the same with Genetics Systems, other U.S. companies, French companies, etc.

14. I have a list already of what you sent to us. I believe it is already stated in this letter.

A final point: look at the latest publication in the ARC magazine. Look at the quotes of Rozenbaum and Montagnier. This is unbelievable! We are simply listed as one of the three "confirmers" of you (we, CDC, Sloan-Kettering). Do you think we accept this? Do you and your colleagues really believe you can get away with saying that the isolation of these viruses and the link (etiologically) to AIDS is just one of several confirmations of you?

(Sincerely yours,

Robert C. Gallo, M.D.

RCG:tas

cc: Dr. P. Fischinger
Dr. D. Bolognesi