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Dr. Serge Lang  
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Dear Dr. Lang:

I have seen your recent letters to the Academy. Your continued references to me regarding matters about which you have no facts and certainly no understanding of, are deeply disturbing and shocking.

You are wrong in your understanding of the events concerning our HIV research or you have been misled. What you have not perceived in any of your harangues is what I meant by the question "are you crazy" and the comment "this is incredible", which I marked in Popovic's first manuscript. Moreover, neither has Dr. Richards realized the context of these comments. You and Dr. Richards know none of the background, none of the reasons, none of the purposes of those remarks. An outline fragmentary explanation follows:

I had little idea of how much or long Popovic cultured LAV. Indeed, he always indicated to me it was very short term.

Indeed, LAV grew only with great difficulty. Another HIV (in 1991 identified as LAI) contaminated one of the French cultures in the summer of 1983. It is LAI that also contaminated a few of ours, and that was inadvertently grown. The true LAV could only be grown with great difficulty. That is true today. Indeed, the Pasteur group had said it was impossible to grow LAI in a cell line. Those were facts that impressed me.

Popovic was not my only co-worker making HIV isolates. I was preparing 3 other Science papers and 1 Lancet paper for publication with the full belief that all of our many isolates were from our lab. Most were. We had several other isolates. It is media distortions and the manipulations of a few that say otherwise.

In July 1983, Montagnier, of the French group, told me he wanted the analysis of "LAV" to be done in France. In other words we were to avoid doing it alone.

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In Popovic's early draft he simply wrote LAV was transmitted transiently to a cell line. In and of itself, this statement is useless, unless the virus is analyzed. Indeed, I thought the statement bizarre without more data. As a group, we discussed an alternative which I believe was quite honorable. I would go to Paris immediately (I did, within 1 week of submission of our papers. I would tell them our results (I did)). I would arrange for an immediate collaboration (I did). My co-worker came to Paris the following month, and within a few months papers co-authored by the French and us were ready. These papers were available to see, but Dr. Richards apparently never saw them. Montagnier later decided against publishing them because a more detailed analysis (full nucleotide sequencing of the whole genome) of one of our isolates and theirs would soon come out.

Do you honestly believe I was "hiding" culturing the "French" virus when such data is included in these papers only a few months later? This is absurd.

As to your other point that in the Popovic et al. paper I denied culturing LAV in a cell line but in fact, we had succeeded in doing so, is a flagrant misrepresentation. Every scientist who I spoke with who read the paper in its full context understood that that statement was in reference to the published literature. It was in the Discussion. The discussion implies LAV and our isolates may be the same subtype, but there were differences (in the literature) of some LAV characteristics and what we knew of our isolates. Then I go on to say or imply that these differences may be artifactual (technical) because of insufficient production of LAV because it has not been produced in a continuous cell line. Obviously, I was referring to the French group, not to what we had recently (and in my thoughts quite transiently) succeeded in doing. Is that wrong? Is that unethical? What should I have said: The French couldn't do it, even said it was impossible, but we just succeeded?

You seem to believe I forced everyone to follow this approach. We all ultimately agreed this was the best thing to do. The OSI Inquiry Team understood this very well. They read the papers. They interviewed us. They found no misconduct. NIH scientists reviewed it, and understood it perfectly well. Yet you sit in judgment with no understanding and cast stones!

You are also wrong and mean spirited about the "narrowness" of my scientific contributions for entry into the Academy. Though it is a bit degrading to discuss this, let me at least remind you that it means something to most scientists that I received the Lasker Prize (usually regarded as the highest prize in the U.S. biomedical science) before AIDS. (I also received a second one for AIDS). In addition, I also received other recognition from peers, like the General Motors Cancer Prize, the Israel Cancer Prize, and the French Griffuel Prize, among others; all for cancer research prior to AIDS. I was also the most cited scientist in the world for the decade of the 1980's. In all fairness, is that so narrow?

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But none of this is really the issue. I have been a target. Much of it, in my opinion, is derived from and planned by people maneuvering over patent money and using the media. A few more joined in this when they saw a chance to make publicity for themselves. In this isolated position, a fellow victimized colleague could greatly use the help of someone with your energy and dedication. Instead, you are helping forces of repression, greed, hatred, jealousy, and ignorance.

If you would like a fuller understanding of these issues, I would be pleased to review with you and to document these events. I realize your notions are greatly colored by Dr. Richards' letter. Please keep in mind Richards has never met me, never questioned me, and states he did not look at our responses to OSI. Thus, he knew only what he was told by Ms. Hadley, a psychologist working for Mr. Dingell. Please also know that the only scientific committee that reviewed our records in detail, interviewed me (more than 20 lengthy interviews) were the OSI Inquiry Team (Hadley chaired it). They made no such criticisms of me. They found no misconduct. No scientific investigators found me guilty of anything. Richards, as he himself noted, was an outside advisor in a unique position, i.e., having influence with little information. This was a novel process, and in my opinion, one that did not work well.

The conclusions you harshly drew about me in your letters were not made by anyone who was involved in the investigations and reviewed the evidence first hand. Thus, how can you make such statements? With this in mind, don't you believe you are unfair to me, when instead you could be helpful? I realize that may be asking you to have the courage to change your direction in a way that may be against your nature. I hope you will.

You have publicly complained that you did not get a reply from your Russian mathematician colleague. I hope you will be consistent in your views that people properly answer their letters and answer this letter, and I hope it is not in the way of avoiding an answer by stating "you stand on your opinion" as you told me by telephone.

Sincerely yours,



Robert C. Gallo, M.D.  
Chief  
Laboratory of Tumor Cell Biology

RCG:kap

cc: Mr. Joseph Onek  
Dr. Frank Press  
Dr. Fred Richards