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VIA FEDERAL EXPRESS

Frederic M. Richards, Ph.D.  
Department of Molecular Biophysics  
and Biochemistry  
Yale University  
260 Whitney Avenue  
New Haven, CT 06511

Dear Dr. Richards:

As counsel to Dr. Robert Gallo, I was recently given the opportunity to review your letter of February 19, 1992 to Dr. Bernadine Healy. I was shocked by its inaccurate, misleading and defamatory contents.

The letter sets out a "sequence of events" which is described as "largely or entirely undisputed." In fact, the sequence of events you rely on is incorrect in every important respect.

1. Your letter says that Dr. Gallo "went to school" with the French virus because his laboratory (the LTCB) grew LAV in a permanent cell line. This greatly overstates the role that LAV played in the work of the LTCB.

The Pasteur scientists had stated that LAV could not be transmitted to a permanent cell line. Based in part on his experience in the LTCB with the Chardon isolate (CC), Dr. Popovic believed, to the contrary, that a retrovirus suspected of infecting T4+ cells could be transmitted to a permanent cell line. There were other isolates in the LTCB he could have used for his first experiments, but he chose to use the virus he received from the Pasteur in late September 1983. According to Dr. Popovic, LAV was put in five cell lines in late October 1983. It did not grow



successfully, although Dr. Popovic noted some infection in one cell line. In November 1983, Dr. Popovic put LAV into two cell lines. But during this same time period he also put several other isolates into cell lines, including MOV, SN, HP, RF and the various isolates constituting the "pool" (subsequently called III-B). All of these isolates grew as well as LAV, and MOV, RF and the pool grew better. LAV was not knowingly used by Dr. Popovic in any of the key experiments performed by the LTCB to prove that HTLV-III (now HIV) was the cause of AIDS.

Consistent with these facts, Dr. Gallo did not believe that the LTCB went to school with the French virus. To the contrary, it was his understanding that LAV did not grow especially well in the LTCB and, significantly, that it did not grow at all in Dr. Popovic's best cell line -- H9.

In light of these facts, your attack on Dr. Gallo's discussion of LAV in the May 1984 Science paper is totally misguided. First, the 1984 paper does not state that Dr. Gallo's laboratory had never transmitted the French virus to a permanent cell line. This is a willful misreading of the relevant sentence. Dr. Gallo noted in the paper that there were apparent differences between HTLV-III as reported by his laboratory and LAV as reported by the Pasteur. He then stated that these apparent differences might be the result of the failure of LAV to be adequately characterized because it had not yet been transmitted to a permanent cell line. It is obvious in context that Dr. Gallo is referring only to the failure of the French to grow LAV in a cell line; it is, after all, the accuracy of the French reports that he is questioning. If the relevant sentence is read with even a modicum of care, it is clear that Dr. Gallo is saying nothing about whether his laboratory grew LAV in a permanent cell line.

Second, your letter assumes without reflection that Dr. Gallo should have described his laboratory's limited work with LAV. But this was not so clear to Dr. Gallo. He was uncertain whether it was appropriate for him to describe his laboratory's work on the French virus at a time when the Pasteur scientists were continuing their own research on that virus. Moreover, he was concerned that it might be uncollegial and indeed insulting to state that his laboratory had grown LAV in a permanent cell line when the Pasteur scientists had reported their inability to grow LAV in a cell line.

Instead of writing about the French virus in his paper, Dr. Gallo flew to Paris immediately after submission of the paper and proposed that the two laboratories prepare joint papers comparing their respective isolates. The French scientists agreed and the two laboratories promptly began collaborative efforts (Dr. Sarngadharan and Dr. Montagnier did a preliminary comparison of

HTLV-III and LAV in Paris in May 1984, just two weeks after the Science paper was published). Two joint manuscripts comparing the French and American isolates were readied for publication in the summer/fall of 1984. (These manuscripts, it should be noted, indicated that Dr. Gallo's laboratory had grown LAV in a permanent cell line.) It was Dr. Montagnier who decided against publication of the manuscripts because he felt that new sequencing data would soon make them superfluous.

Perhaps Dr. Gallo should have dealt with LAV somewhat differently in the May 1984 Science paper. But the suggestion that he engaged in "intellectual recklessness" or "intellectual appropriation" is ludicrous. From Dr. Gallo's standpoint in 1984, the contribution of LAV to the Science paper was minimal. If his failure to mention that contribution was "intellectual appropriation," what do you say about the fact that the Pasteur scientists did not acknowledge in their May 1983 Science paper that Dr. Gallo had provided them with the very idea to look for a retrovirus in AIDS patients and with his T-cell culturing protocol? You are plainly making up new standards of scientific conduct, and it is you, not Dr. Gallo, who is guilty of intellectual recklessness.

2. Your letter states that Dr. Gallo's laboratory used HUT-78 as a cell line and called it HT. But you ignore all the crucial facts.

The Gallo laboratory had grown HUT-78 in the early 1980s primarily in attempts to find a leukemia virus in it. Perhaps because of co-culturing HUT-78 with other cells, the cell lines labeled "HUT-78" in the laboratory were all different. Dr. Dean Mann, who had done HLA typing on the lines, cautioned Dr. Popovic that the "HUT-78 cell lines were a mess." As a result, Dr. Popovic did not know whether he was using the original HUT-78 when he began to infect it with virus samples in late 1983. He turned to single cell cloning as a way of avoiding mixed parentage cell lines. Moreover, by December 1983 he began using an alternative designation for the cell line ("HT"). He did not call the cell line "HUT-78" even in the earliest drafts of his 1984 Science paper.

Dr. Gallo and Dr. Popovic decided to use the term HT in the Science paper. Dr. Gallo did not believe it made sense to delay publication of the paper in order to track down the origins of HT. After publication he arranged for Dr. Popovic to go to Dr. Paul Bunn, one of the original authors of the HUT-78 paper, to obtain the necessary clarification. Unfortunately, analysis of the cell line Dr. Bunn provided to Dr. Popovic at that time showed a cell type different from those in the LTCB and different from the original HUT-78, since the patient from whom HUT-78 was derived was male and the cells obtained from Dr. Bunn were of female origin. Thus, the issue was not promptly clarified and required more analysis.

Contrary to the outrageous assertion in your letter, no effort was made by the LTCB to deny others the use of the H9 clone or to hide the origins of the parental line. Records indicate that the LTCB made H9 available to at least 45 laboratories in 17 different countries in 1984 alone. Dr. Popovic reported the HLA pattern of the H9 cell clone in Lancet in 1985. Thus, scientists throughout the world had the opportunity to determine for themselves the relationship of H9 to one or another HUT-78. This includes Drs. Bunn and Gazdar, who were early recipients of H9.

Finally, neither the Science paper nor the patent application claimed HT or H9 as a new cell line or discovery. The patent application described a process for growing HIV in cell lines and made clear that HIV could be grown in several different T cell lines.

3. Your letter states that the pool culture experiment was haphazard and that its "haphazard nature is established by the fact that the HTLV-III culture was ultimately shown" to be HIV-Lai. This statement demonstrates the most extraordinary ignorance. HIV-Lai contaminated the Pasteur's own cultures, as well as cultures in Dr. Robin Weiss' laboratory in London and several other well-known laboratories. Are you suggesting that work in all those laboratories was "haphazard?" You must be unaware that such accidental contaminations are common in retrovirology.

4. Your letter states that there was a discrepancy in the treatment of Drs. Gallo and Popovic with respect to several allegations. But your analysis of each of the allegations is totally wrong.

A. The statement in the 1984 paper that the concentrated fluids were first shown to be RT positive was not written by Dr. Popovic. It was written by a subsequent editor, probably Dr. Gallo or Dr. Sarngadharan. If you had bothered to read the previous draft of the paper, you would have seen that the final version simply reflected a logical (but, in fact, mistaken) editorial clarification. There was no deliberate misrepresentation and no misconduct by Dr. Popovic or by the subsequent editor.

B. With respect to the description of the immunofluorescence assay results, both Dr. Gallo and Dr. Popovic have provided letters to Dr. Healy demonstrating that there was no misconduct. But, even if there were misconduct, it obviously could not be blamed on Dr. Gallo. He cannot be held responsible for an error in a single number in a single table that he did not construct.

C. You complain that the opinion of Dr. Gallo was accepted over Dr. Gonda with respect to the interpretation of EM photographs "in spite of the fact that the latter was an acknowledged expert in this technique..." But you fail to add

that Dr. Gallo was right in his interpretation and that Dr. Gonda reversed his opinion a few months later. Isn't this failure an example of intellectual recklessness?

4. You state that Dr. Popovic had a "known inadequacy in record keeping." This allegation, which was contained in the preliminary OSI draft report, was definitively rebutted by Dr. Gallo's response. The preliminary draft relied on a conversation about Dr. Popovic's record-keeping that Dr. Gallo had with Dr. Ken Nielson of Uppsala. But that conversation took place in June 1990; Dr. Gallo had no knowledge of Dr. Popovic's record-keeping habits in 1983. (It should also be noted that Dr. Popovic relied on the technicians working with him who generally provided excellent notes for him.)

It is extraordinary that you could make serious charges against Dr. Gallo without carefully reading his response to the preliminary draft report. Again, one could well call this intellectual recklessness.

5. Your letter states that there was no "pressure to publish" the 1984 papers "as a necessary prelude to development of the blood test" and that the "timing of the papers was irrelevant." Are you suggesting that if the 1984 Science papers had been published several months later there would have been no delay in the availability of a blood test? Have you confirmed this with Abbott Laboratories and the other manufacturers of the blood test?

Your suggestion that the pressure to publish is only due to a desire for priority is outrageous. Dr. Gallo and Dr. Popovic are physicians as well as biomedical scientists, trained to regard saving even one life as all important. They had seen a dramatic increase in the infection of Japanese hemophiliacs due to the use of American blood during the very period the 1984 Science papers were being prepared. Do you really believe this did not create a pressure to publish promptly?

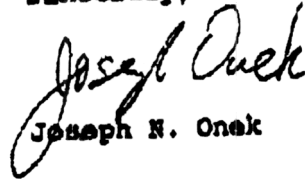
By all accounts, you are an outstanding and fair-minded scientist. But you have committed an extraordinary, indeed scandalous, blunder. You have written a letter about a fellow scientist (which you must have known would become public) filled with egregious factual errors and defamatory conclusions. And you wrote such a letter without even giving Dr. Gallo the opportunity to present his views. There can be few more blatant examples of uncollegial behavior and misconduct in the history of modern science.

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Given your fine reputation, I am sure you will immediately retract your letter. Fairness to Dr. Gallo and to the scientific community demands no less.

Sincerely,

  
Joseph N. Onak

cc: James O. Mason, M.D.  
Bernadine Healy, M.D.  
Frank Press, Ph.D.  
Lyle Bivens, Ph.D.