DEPARTMENT OF HEALTH & HUMAN SERVICES

Public Health Service

National Institutes of Health
National Cancer Institute
Bethesda, Maryland 20892
Building 37
Room 6A11
Tel: 301/496-6007
Fax: 301/402-1338

December 15, 1994

Dr. Suzanne W. Hadley

Dear Suzanne:

I am pleased we have had these last discussions, and I am hopeful that you will use the opportunity available to you now to rectify numerous distortions in the media that have been harmful to me. I realize that both understanding and courage will be required to correct the many misimpressions of the past that have been unfair and cruel. I have three general areas of concern. I list these below and follow with 18 specific examples. There are others as well, but these are the major ones.

General:

I. It is not well known that every time I (we) have had a fair hearing in which both sides of issues were studied and people genuinely looked at the problem, I was not found guilty of any wrong-doing or misconduct. Only the ORI (non-scientists) concluded that I had any misconduct (by changing conclusions of the OSI Inquiry), based on their erroneous interpretation of a single sentence in one paper. As you know, when I made an Appeal, which would have allowed me to speak openly, my case was dropped. Addionally, despite the fact that the concluding remarks of the Appeals Board (regarding the case of my co-worker Popovic) were that "after all the sound and fury one would have expected some iota of wrong-doing--but there was none," nonetheless the ORI statement to the media when it dropped my case was that they (ORI) could not win because of the "new high standards" required to prove guilt!

II. I believe you have an ethical responsibility to correct the character assassination done to me in some of the media and from the comments of some members of the Richards' committee, who, by being given one-sided, incomplete information have been led to a belief that there was a pattern of non-collegial behavior. Like Cervantes, I believe the greatest distortion comes from half-truths and yields the greatest slander. I believe the Richards' committee members were a victim of receiving half-truths, as I have been a victim of slander. You have the records and the facts, and you now have the opportunity to correct these misimpressions.

In the early history of AIDS I was not a villain or hindering AIDS research. As you well know, the truth is the opposite. I entered AIDS research early, voluntarily, and my colleagues and I were among the first who regularly accepted and cultured cells from AIDS patients. Many labs who regularly accepted and cultured cells from AIDS patients. Many labs would not would not allow an AIDS specimen to enter their buildings until years later due to fear of infection.

At no time did I manipulate patents for my gain. As you well know, I did not initiate the blood test patent and there was no patent money for Government employees in 1984. Some years later Pres. Reagan modified the law to allow some royalties to us. I always made reagents widely available, and I encouraged AIDS research all over the world including in France. Indeed, it is likely that I made more reagents available than anyone else in the world, and it is very evident that I stimulated numerous collaborations in the field of human retrovirology.

III. Finally, I think you have an obligation to make clear the contributions of this laboratory to the discovery of the AIDS virus which have not changed since Montagnier and I summarized them for Nature (as a chronology) in 1987.

Some Specifics:

1) I was criticized for statements I made about not culturing "LAV", (whereas Popovic actually had cultured LAV), and in particular about the statement in the Discussion of the May '84 Science paper of Popovic et al. in which I indicated LAV was not grown in a continuous cell line. As you know I have maintained that the sentence in that paragraph obviously referred to the French lack of success in that regard--and not of Popovic's then recent success. Otherwise neither the statement nor the paragraph make any sense. As you also know, my verbal references following publication of the four Science papers that "LAV was not cultured ...." was in response to the malicious innuendos that we may have deliberately mass-produced LAV for our blood test. I believed this could be the only possible concern of any rational person. In general that has been the basis of my denials--to have temporarily cultured LAV in a cell line would not only have been appropriate, it would have been expected. However, ironically, as you now know, Popovic did not culture LAV in a cell line. He cultured LAI, a French contamination of their LAV sample (but unknown to any of us at the time).

But more important to the issue, I did not even know of the "LAV" (really LAI) culturing except that it was transient in a cell line. In short, my impression was that our work on "LAV" was trivial and very transient. Obviously, Popovic did not know his pool virus (IIIB) had become contaminated with "LAV" (really LAI).

Both Popovic and Sarngadharan have repeatedly stated how little I knew of the "LAV" culturing experiments at the time, (and I say the same) and other than myself they are the only ones who could know. I believe Popovic would be willing to write a letter affirming this as well as Dr. Sarngadharan. Any other interpretation , e.g., what I believe the Richards' people have been led to believe and what ORI and the IG have written (I assume by being misled also) is wrong and libelous. You have a responsibility in this; I hope and expect you will correct it.

2) A second issue, but somewhat related, is the presumption that I knew "LAV" (really LAI) was the same subtype as our isolates of HIV because of the significant positive results with AIDS sera (reacting with "LAV") Don Francis is to have shown me in April '84 in Paris, i.e. presumably the same exact virus subtype as IIIB. I assume you now understand several aspects about this, since I have gone through it with you in detail on several occasions.

(a) Our papers were <u>already submitted</u> (March '84) when I was shown this data.

(b) I did not have the data to examine. I was simply quickly shown some table or tables.

(c) Those data were <u>without confirmatory Western blots</u>, and <u>I knew their data, based on ELISA only, was changing rapidly--depending upon where they arbitrarily made their ELISA cut off line</u> e,g, at one meeting in November '83 it was 18-20% seropositivity and a few days later it was 36%. Consequently, I did not trust their results. Obviously I was right not to, because several months later they still had only 41% seropositivity with AIDS sera, and of course the French had no blood test in 1984 nor in early to mid-1985. Furthermore, Montagnier was arguing that there was <u>no</u> antibody in AIDS sera to "LAV" envelope, and I knew our data indicated the <u>major</u> antibody response to our viruses was to <u>envelope</u>. Most significantly, in July 1984 Don Francis and the Pasteur scientists published a paper stating that "<u>it is possible</u>" that LAV and HTLV-III are the same. Thus, it is clear that in July '84, Francis did not believe the data he showed me in April demonstrated that the two viruses were the same. One can only speculate as to why he subsequently chose to rewrite history.

(d) The information I had (verbally) from Popovic was that there were significant culturing differences in the properties of our pool virus and our RF compared to "LAV". Remember: Popovic first worked with the <u>real</u> LAV (from patient Bru), <u>and real LAV failed to be transmitted</u>. Popovic could not know the Sept. 22, '83 - later sample of "LAV" was in fact contaminated by the French with another virus. One interpretation (he later gave to me) was that he thought he may have contaminated LAV with one of his U.S. AIDS samples. As you know, only in 1991 did <u>we</u> discover that the Sept. 22nd sample was <u>not</u> LAV, i.e., that the French sent us something different that they had never described. Remember also: Montagnier insisted LAV could not be grown in a cell line.

(e) I had never been shown or told of <u>any data</u> on Popovic's results with "LAV", <u>except that it had been transiently cultured</u>.

(f) I had just experienced the story of HTLV-1 and HTLV-2. In that my colleagues and I discovered these viruses; in as much as these were the only known human retroviruses; in as much as we were the only ones with such experience in human retrovirology; and in as much as HTLV-1 and HTLV-2 are very different subtypes--with major differences in disease causation, yet completely immunologically cross-react (i.e. sera of patients with leukemias caused by HTLV-1 will cross with HTLV-2), it seemed to me that before we could conclude that "LAV" and various isolates of ours were the same subtype more refined analysis would be needed. Whatever you have been told, the fact is that Montagnier told me in July 1983 that molecular analysis of "LAV" would have to be done in France. Consequently, my position was that LAV should not be analyzed or studied in any serious way until we collaborated with the Pasteur group after our papers with our own isolates came out. As you also know, I went to see the French group only one week after submitting our papers, arranged for such comparisons, gave them the cell line producing high titers of our IIIB in May (they had no cell line producing virus at that time), gave them their first specific reagents (antibodies) to HIV, and prepared collaborative papers to publish with them--one, on their protein immunology relatedness by Chermann et al. prepared some time in the summer of 1984 and a molecular (nucleic acid) comparison by Wong-Staal et al. prepared in the fall of 1984. As you know, those draft manuscripts exist. As you know, Montagnier did not want to publish them. As you also know these draft manuscripts also show "LAV" in a cell line from our lab, and as you know the comparison (immunological) paper (Chermann et al.) was typed in Montagnier's office, again proving Montagnier knew "LAV" was cultured in a cell line in our lab.

(g) Please note that Montagnier's own data and his own words argued that "LAV" was more linked to lymphadenopathy than to AIDS, and that he had said he had no immune reactivity to the envelope of the virus. In contrast, the data I knew of from our lab indicated that AIDS sera reacted predominantly with envelope, and the overall serology showed clear linkage to AIDS. Of course, I speculated that this could be due to technical problems of the French group, but I could not know this. You have sometimes countered this point by stating that in fact Popovic had serological data with "LAV" (really LAI) because MoV and the pool virus turned out to be "LAV (LAI)." True but you leave out the critical points that I did not know this, and obviously Popovic states he did not know this.

(3) Please also note (see enclosure) that the transcripts of Heckler's April 1984 press conference exist, including my remarks. My remarks speak for themselves. I ask a reporter not to call the virus HTLV but HTLV-III/LAV because the viruses (ours and the French) may be of the same subtype. Note well: This is in April '84. I hope and I expect you will rectify the misimpression.

(4) I am flabbergasted by your statement that "Popovic is a real tragedy in all of this." Obviously--that is true--he is a tragedy. But who did it if not this very process? Though I know Popovic did nothing wrong, you have made much about his use and naming of MoV, its lack of identification

etc. But how could this even conceivable have anything to do with my conduct? I do not even know when I first heard of MoV. It was probably even after our papers were published. The only point you can make about my putative role in any harm to Popovic is my criticism for his handling of "LAV" in one manuscript, i.e. Popovic had some sentences in the first draft of his paper on "LAV" culturing. However, what you did not realize is that my statement "Mika you are crazy" was because I truly believed it was absurd to begin a paper about a virus I believed we did not know was the same subtype as ours, and one which I believed he had cultured for only a very transient period, and without any characterization. It was a shock to me to see it at the start of his paper and that is what I criticized. Keep in mind the context: There were four other papers (three for Science and one for Lancet-- dealing with our isolates. Admittedly, some of that data was with IIIB, but it is much later that we could conclude that IIIB likely came from a virus (LAI) from France.

(5) It seems from comments from you and from the IG report and elsewhere that you accept Barre-Sinoussi's statement about what Popovic is supposed to have told her on a bus ride in 1992 at my lab meeting (i.e., he intentionally put "LAV" in the pool"). Acceptance of this sudden statement from out of the blue is astonishing. How can anyone discount (a) Mika's disavowal of this 100 times over; (b) its absurdity (suddenly Popovic tells "the French" but no one else?); (c) my testimony that Barre-Sinoussi recanted the essence of this in a telephone conversation to me: and (d) that her letter to Jon Cohen of Science shows her incredible ambiguity which I feel certain will grow in time; and e) the obvious financial motives of the Pasteur Inst. and especially their N.Y. lawyers. It is no less than a "Comedie Francais" that the ORI swallows it, or perhaps, like the geese for fois gras, they were force-fed. But this is not the point. If you really believe this, how then could Popovic be a tragedy? He would indeed have done wrong! Moreover, Popovic has consistently told me hedid notintentionallyput LAV in the pool--what does this have to do with me? Am I not at least a minor tragedy?

6) I remind you again: Popovic states he told Montagnier in Dec. 1983 that he knew how to culture "LAV", and he has testified that it would be obvious to anyone that he meant in a cell line, because handling "LAV" in short term culture in blood cells is something Montagnier et al. could already do and it did not and could not bring him very far. If this is true, Montagnier knew that "LAV" was cultured in our lab before I did, because I first learned this in Jan. 1984.

Moreover, and as I have demonstrated to you, Montagnier's own writings (his letter to Nature in July '84, and his draft manuscript, Chermann et al., apparently typed in his office, I think, sometime in the summer of 1984) clearly acknowledging "LAV" in cell lines from my lab, proves he knew by July 1984 at the latest. I recall telling Chermann (then a co-worker of Montagnier) in the first week of April 1984 when I visited Paris, and Chermann has acknowledged to me and to Joe Onek that he is sure he knew by June 1984. Therefore it is obvious the "French" knew of Popovic's success with "LAV" within a few months of my knowing it. Isn't that quick enough? How do you reconcile this with your statements? with the IG report? Will you now point out these things for the first time?

7) You and the investigatory arms influenced by you and Mr. Stockton (such as the ORI and IG) refer to the French virus as LAV. As you well know, LAV was an isolate from patient Bru, and there was no successful cell line culturing of true LAV by my lab. LAV was contaminated (unknowingly) by the French in 1983 with a virus called LAI. It is LAI which contaminated Popovic's pool, and samples in many other laboratories in the world later on (because of its greater replication capacity).

It was my lab as you know which discovered this contamination in 1991 (*Nature* paper, Reitz et al.). It is a distortion to refer to LAI as LAV, giving the false impression that (a) "the French" were able to keep a virus going from May '83; and (b) that only we were contaminated (unknowingly) by the French virus in 1983. Isn't it odd that not even "the French" call LAI, LAV--only the ORI, IG, and you. Will you correct this?

8) The IG stated that the evidence indicated there was "no pool". This is an outrageous absurdity, and blindly follows Crewdson's obsession. I do not believe the IG understands any of this. Where did they get this impression? Only Popovic himself could know, and Popovic says there was a pool. The fact that no early aliquots exist is to be expected. (Why would anyone keep such aliquots?) There is no evidence there was not. Having said this, I do not understand how, in any event, I could be criticized. I was probably first told of the pooling experiment sometime in March 1984.

9) More on misinformation and distortions concerning the "pool virus" (IIIB):

> As you know Popovic successfully infected many cell clones derived from a parental T-cell line, and these clones were given the titles H1, H2, H3, ...etc. He reported that these could be infected with IIIB and in come cases with RF and other HIV-1 strains we isolated in 1983 or early 1984. Of course most of these infections were one time, short-term, experiments. Obviously, he made no attempt to maintain most of these. It would serve no purpose and be costly. He only maintained virus production in H4 and especially in cell clone H9, and of course, H9 infected with IIIB was distributed to labs all over the world and was used all over the world for the blood test. The fact that the other cell clones can be and were infected with IIIB is, of course, indisputable, because the results are recorded and the experiments readily reproducible. During your investigations you came to my lab in the absence of Popovic and myself to get an early sample of the "pool virus" (IIIB). Popovic may not have retained aliquots of those early pool experiments, and, not unexpectedly, none were available a decade after the experiments. Howard Streicher (who was not with us in 1983 when these experiments were done) and a technician who was with us but did not participate in these experiments provided you with a sample they found in the

freezer which might be related to a portion of those experiments. Dr. Streicher found a tube labeled H17. He believed it might be an infected cell clone (with IIIB). Consequently, Dr. Nancy Miller, then working in our lab as an administrative aide, labeled it herself H17/IIIB, and gave it to you. Since the pool virus was never maintained in H17 but rather in another cellular subclone, namely H9, H17 obviously was irrelevant to the pooled virus experiment, nonetheless the sample was analyzed. The results of the analysis of H17 showed that these H17 cells were uninfected, and as was typical then and always, these results were somehow leaked to Crewdson. In his fashion he produced still another article with another distortion and more maligning of me, despite the absurdity of his conclusion that this result meant there was no pool! Even worse, it was picked up by David Hamilton who was at that time writing articles for Science magazine, and he published much the same. Obviously, this ridiculous distortion hurt us, and you have done nothing to counter any of this. Since the H17 cells were never claimed to be maintained as a producer of HIV, one could hope that the reader would see through the nonsense. Whether H17 was or was not infected is a moot point. However it is also obvious that the sample given to you had to be an uninfected control because the simple fact is that H17 is readily "infectable" by IIIB! If the H17 given to you was one used in an infection experiment, HIV-1 sequences would have been found, even if H17 were not infected because extracellular viral particles would be in the tube and easily detected by PCR!

But no matter how ludicrous, people do not know, or do not remember such details. Crewdson, the "expert" has the details but not the truth. Consequently, this hurt us, and once again there was nothing we could do about it. Will you now help and tell the truth in your planned report? Will you correct Crewdson's misinformation half-truths, and resultant libel?

10) You recently started to question my 1985 paper describing 101 HIV isolates, yet you have no reason to do so since I more than documented the 48 detections and/or isolates described in the earlier Gallo et al. May '84 Science paper (following Popovic's paper and the papers on our development of the blood test). Indeed, and as you yourself brought to my attention during the inquiry of 1989-1991, I documented that we had over 50 isolates by the time we submitted the May '84 papers, so I underestimated the number in the 1984 paper. Moreover, the PNAS March of 1985 paper has nothing to do with the area you were investigating. However, and as you know, cooperating with your request, we can and have documented the bulk of that data despite it being data gathered in 1983-84, more than 10 years ago, with once again more loss of our research time. Better, we documented more than 200 isolates. We probably included only 101 because there may have been insufficient clinical information about some and some may not have included serological analysis. It is true that the Abstract states the isolates were a total of all our isolates from the earliest (late 1982) to the time of submission of the paper (March 1985), and it is true that the earliest ones in 1982 could not then have had serological

analysis because no antisera was available at that time (this should have been self-evident to readers.) Also, it is quite possible that we had those early samples serologically evaluated in 1984 or early 1985. This is an example of a failure of investigators to see the forest for the trees. This was an important paper, documenting the vast majority of all the HIV isolates in the world at that time. That key work is simply swept away because small possible inaccuracies are dwelt upon. Isn't this a kind of witch hunting?

11) Possible misappropriation of "LAV". This is the biggest and cruelest joke of all. First, I do not work in the lab. I discuss results, provide ideas and critiques, and set directions. Second, we had other isolates including HIV-1 isolates in 1983. Your response on this changed at some stage around 1992. Having other isolates means we had no motive, simple and pure. Obviously we cannot prove a negative, just as you cannot prove that you are not doing this with Crewdson and Stockton for self gain of some sort. By now even you must be suspicious of Crewdson's eight years on my case (even the "Bobby socksers" outgrew Frank Sinatra before eight years. Am I soooo interesting?)

12) More recently (and since the Inquiry, Investigations, ORI report, Appeals, etc. etc. were completed) you have showed Dr. Broder my confident statements made to some inquiries (media or elsewhere) (before 1990) that our "pool virus" (IIIB) could not be a contaminant with "LAV." I accept the criticism of being over-confident on this and being wrong. However, there appears to be a spin on this that somehow my statements may not have been honest. What has been left out of the story is the following:

a) There was in fact no contamination with LAV. The contamination was with LAI, and this is not unimportant because the (unknown) contamination by the French of LAV with LAI in the last sample sent to us at minimum caused considerable confusion.

b) The differences in properties of true LAV (from the early French work and from Popovic's own studies) from the pool virus were very much on my mind.

c) Montagnier in the early years repeatedly stated it was impossible for him to have contaminated his LAV (Bru). We all now know that LAV (Bru) was contaminated by LAI sometime in mid or late 1983, and it was LAI which subsequently contaminated our pool virus IIIB and MoV (but not our other 1983 and 1984 viral isolates used throughout the world to this day, such as RF, MN and others). It is LAI which subsequently contaminanted many other laboratories. You know why: it grows better than most HIV-1 strains in the T cell lines. Could you now point this out? Would you also point out that I am not the only scientist to make the mistake of over-confidence?

13) The IG report implies that I put pressure (and worse) on Murray Gardner at U.C. Davis not to compare "LAV" and IIIB (the pool virus). Gardner told me that this is a lie by the IG, that he said nothing of the

kind, and that he was repeatedly pressed by the "investigators" to take this position. The simple truth is I called him to tell him I was already doing the comparison with the French as collaborators. He volunteered that this was the most appropriate thing. Of course and as you know very well, that is precisely the truth. I did arrange for the comparisons--immediately upon submitting our papers to Science; the comparisons were done; papers were drafted; and Montagnier decided not to publish because (he argued) that the genomic sequences would soon be coming out from our respective labs and such data would be more definitive. What could be more proof of my honest desire for rapid appearance of the truth? Will you correct the distortions of the truth by the IG?

14) Much has been made by the IG and from the statements coming out from the subcommittee about one of the numerous statements made in my Declaration of 1987 in recalling 1983-84 knowledge. The contested statement sometimes called "possible perjury" before the charge was dropped, refers to the fact that in April 1984 lectures I openly stated our viruses may very well be of the same subtype as the French LAV; whereas for the patent declaration my statement to the HHS lawyers was to the effect that I had no reason to believe the viruses were substantially the same. What has been left out of all of this is: I answered in truth and precisely all the lawyers' questions. They advised me that the differences were substantial, e.g., much more different than we knew between HTLV-1 and HTLV-2 (which by any definition were substantially different). For example: (a) Montagnier's lack of gp41 (see his gels in 1983) in "LAV", whereas we had discovered gp41 and shown it to be a prominent antigen; (b) the greater linkage (of his data) of "LAV" to lymphadenopathy than to AIDS; (c) the lack of immunogenicity of LAV in rabbits (according to Chermann, Montagnier's then major co-worker), whereas we had good antibodies in rabbits; (d) the lack of antibody to envelope of "LAV" (according to Montagnier), whereas antibody to envelope was a key to our blood test; (e) Montagnier's statements to me that LAV (really LAI) grew less effectively than IIIB. These were the facts as I was told or showed. These differences, according to U.S. HHS lawyers, made them substantially different. Indeed, I certainly considered that they might be different subtypes of the same kind of virus, i.e. like HTLV-1 vs HTLV-2, or as we see today, HIV-1 vs HIV-2.

15) Reagents. I have been criticized about giving out H9 and/or the virus, at least in the IG report. Told as a phenomenally incomplete and very biased story, it focussed on a few slip-ups and on the few cases where some restrictions were placed. This was unfair and misrepresented the fact that our lab was enormously responsive to reagent requests in this critical period, before, and since. In fact, our lab has been widely recognized as being abundantly generous in this regard. As you know, we received close to 300 letters of support for me on this subject. I have been absurdly criticized for the temporary Material Transfer Agreement (MTA) of NCI; but as our former Division Director, Richard Adamson, would have testified (if my case had not been dropped), this temporary MTA was not as restrictive as others in use and certainly not as restrictive as the final one put into effect by the NCI Director. Moreover, I did not

create the temporary MTA. Dr. Ann Sliski (a former colleague and administrative aide) would have testified that she created this form with input from NCI administrators.

Where I am involved was in forming some restrictions on the CDC and also Mal Martin, but most of this was due to the fact that I wanted the FIRST comparisons of IIIB and "LAV" to be done with Pasteur scientists as we had agreed. In addition, there were other good reasons for these restrictions, which as noted by the CDC did not effect public health related issues. In May 1984 we decided to re-evaluate our cell stocks for possible adventitious contaminations, e.g., mycoplasma, herpes viruses, etc. and then to "scale-up" production for the numerous scientists all over the world who requested samples. This was done by NCI contractors. By September 1984 they could send these samples out for us, and they did, and even advertised it in the Federal Register.

It would also be nice if you would note that I sent Montagnier his first IL-2 to grow T-cells, the necessary reagents to distinguish his virus (LAV) from HTLV-1 and HTLV-2 for his 1983 paper, and the protocol for growing human T-cells by using newborn umbilical cord blood T cells with Il-2 (sent to Chermann and acknowledged by him).

16) The examples and the pattern of incomplete information and/or information out of context directly leading to character assassination of Popovic or of me is endless. Crewdson and some reports you have been involved in have implied that because MoV is named and used in our lab and then disappears from our later work except in one paper, this is suspicious. MoV is focussed on because you learned of its contamination with LAI. Although you provide some of our reasons for not using it (Sarngadharan's suspicions it might be contaminated with an HTLV), the suspicion is still presented that this may have been a deliberate taking of LAI and renaming of it by Popovic. What is left out is that many, many isolates that we have had were not propagated or used, for many reasons. This was true of the French isolates IDAV and IDAV2. We heard about them for a short time and then no more. Does that mean they too are under suspicion for some improper procedures?

17) The ORI criticized me for my role in the preparation of the French 1983 publication in Science. Specifically, they criticized me for (1) the unorthodox fact that I wrote the Abstract for the French paper; (2) that I put some phrases in that were self-serving, i.e., that the new virus was likely a new member of the HTLV family (supposedly Montagnier only agreed to this because he feared that I will otherwise reject his paper); (3) that I stated that their data showed a cross reaction with HTLV-1 infected cells when their data did not say this; and (4) that unlike normal practice I did not remain anonymous but openly discussed issues of the paper with the French by telephone. Of all the criticisms, these are the most bizarre. First, I was asked by the French to write the Abstract because they had forgotten to write one. Second, I reviewed the paper (positively) and expedited its publication while my own was held up. Third, I could easily have remained anonymous and delayed and/or rejected

their paper if they did not follow the reviewer's requests (mine). Instead, I was open. At no time did I threaten not to accept the paper. To the contrary, the French paper had already been rejected by Nature. I intervened to help them--the opposite of the ORIs position. To remain anonymous or not is arbitrary for many journals. Next, my opinion that the virus would be a member of the HTLV family was based on obvious information and logic of the time, e.g., the French defined LAV as a typical type-C virus (not a lenti-retrovirus). The HTVLs are type-C. They showed a small major core protein (unlike most retroviruses but very much like HTLVs) and their assay conditions for reverse transcriptase were those we defined for HTLV-1. ALso, a reaction of their sera with HTLV-1 infected cells but not with the same cells uninfected, strongly suggested that the positive reaction was with a viral protein. Finally, Essex had shown better data of blind tests of AIDS sera using HTLV-1 (35% positive) than did Montagnier with LAV (18% positive). How could I know Essex's data would be due to a cross reaction with a cell membrane protein and Montagnier have a poor assay? How could I know the French electron microscopy would misclassify their virus? At that time it would have been foolish to think that LAV, which targeted T cells, (like HTLVs) was not in the HTLV family. Also, I have since learned that Montagnier did indeed receive galleys and had the chance to modify his paper if he had wished to. In fact, he did modify the galleys and even made changes to my proposed changes. Also remember it is the prerogative of the reviewer to suggest, and that is what I did. A reviewer can reject, but I did not. Finally, in any case, the abstract clearly defines a new virus, a discovery. Being in the HTLV family would not reduce the value of the discovery. As you know, however, this paper did not define the new virus as the cause of AIDS nor claim to do so.

18) Finally, if people want to know the truth and you "seek truth", as you have indicated to me--will you convey how people, e.g. the Richards' committee, have been misled about me (both on scientific contributions and my integrity)? Will you note our achievements in AIDS--including making many independent HIV isolates of our own, the development (with Max Essex) of the idea that AIDS would be caused by a retrovirus (1982) the IL-2 T-cell culture technology, the development of the blood test which not only saved lives but allowed the epidemic to be properly monitored for the first time, convincing evidence that HIV is the cause of AIDS, and the mass productions of the virus--to name the earliest ones? Will you explain why Montagnier asked me if I would accept the position as co-discoverer of HIV? Would you clarify that I never "stole" or misappropriated anything? Would you note that the history or chronology as published in Nature by me and Montagnier in 1987 of the respective contributions of our groups has not changed one iota despite the campaign to the contrary: the brutal but idiotic Sam Donaldson ABC report, the HBO film farce, and a barrage of other media attacks which I am often told were influenced by Stockton or you.

In closing, I repeat that the things mentioned above are a small sampling of matters in which slander and libel have been done to me. I have personally mentioned to you areas where I could have done better.

Particularly, I wish I had requested a French presence at the Press Conference of Secretary Heckler. I wish I had not been so confident of everything and obviously I wish I had had better luck and had selected another isolate (especially RF) for our blood test. I also wish I was more sensitive to the sensitivities of a few scientists. I imagine that you and Mr. Stockton also wish you could have done better. Now's your chance.

Sincerely yours,

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