

I just heard that Francoise Barre- Sinoussi and Luc Montagnier have been awarded the Nobel prize for the discovery of HIV. This is a thoroughly deserved honour, and it is completely appropriate that Francoise Barre Sinoussi has received such a resounding recognition for her work. In 1985 circumstances brought me into contact with Luc Montagnier and Jean- Claude Chermann, a retrovirologist at the Pasteur Institute who was very closely associated with the work that led to the discovery of HIV, called LAV by the Pasteur Institute workers. This contact came at a time when the sequences of LAV and the virus isolated by Robert Gallo, which he strangely named HTLV-3 had been published, and it was evident that they were identical to an extent that excluded the possibility that they were separate isolates. To commemorate this deserved honour, I have interrupted the planned series of articles, to write about the 1985 encounter with Montagnier and Chermann in New York.

In February of 1985 the AIDS Medical Foundation of New York, together with the Scientist's Institute for Public Information (SIPI) organized a meeting for science writers to inform them of the most recent developments in AIDS research. The most prominent scientists working on AIDS presented their findings to the science writers of America. Among those invited were Robert Gallo, Jean-Claude Chermann and Luc Montagnier. Dr Mathilde Krim was the chairman of the Foundation and I was its Scientific Director, and the night before the meeting we had dinner with Luc Montagnier and Jean-Claude Chermann at the Harley Hotel, where the meeting was to be held. Robert Gallo had said that he was unable to attend, a decision which angered Dr Krim, who that day had sent Terry Biern, of our Foundation to personally ask Dr Gallo to reconsider his decision. Terry told us, during dinner that Dr Gallo would not budge and so would be absent from the meeting.

In the days before this dinner, a remarkable event had occurred that directly touched on the issue of where HIV was discovered. The sequences of two supposedly different viruses, HTLV-3 isolated in Dr Gallo's lab, and LAV isolated

by Francoise Barre Sinoussi and Jean-Claude Chermann at the Pasteur Institute had been published. The sequence refers to the ordering of the four building blocks of the virus' genetic material. There will always be some variation in these sequences when the same virus is independently isolated, as these differences are generated by mutational changes when the virus replicates, and these changes will never be identical when replication occurs in different hosts.

The startling information that emerged from a comparison of the sequences of HTLV-3 and LAV was that the differences between them were much less than what might have been expected from different isolates. They were in fact one and the same virus. Since LAV had been isolated before HTLV-3, it was clear that in fact HTLV-3 was the same virus as that called LAV. The most likely explanation of this is that the samples of LAV that had been sent to Dr Gallo accidentally contaminated cultures in his lab. Such contamination is unfortunately a common accidental occurrence when working with viruses in tissue culture.

At dinner the night before the meeting, both Chermann and Montagnier seemed to me mostly embarrassed about what they had just learned about the identity of HTLV-3. This was obviously the big news of the moment and coming within weeks of a meeting where research results were to be presented to science writers, one might have thought that this information would be presented to them. We did speak at some length about this finding that night; the impression that remains with me is that both Montagnier and Cherman were very much taken aback and certainly too embarrassed to bring up the issue themselves. It seemed obvious that the reason why Dr Gallo backed out of the meeting was the recent publication of the sequence results with the clear implication that HTLV-3 was in fact LAV. I believe Margaret Heckler who as Secretary of the US Department of Health and Human Services and who had announced that Dr Gallo had discovered the cause of AIDS in his isolation of HTLV-3, had also backed out of attending the meeting.

I was involved in organizing this meeting but was not a speaker, so I told Montagnier and Chermann that I would attempt to get one of the speakers to present this obviously important information to the top science writers of America.

The following day those scientists I spoke to were fully aware of the implications of the sequence similarities, but none was willing to announce these results to the journalists. In fact I had the strangest encounter with David Baltimore, who was a member of the Board of our Foundation. He was next to me in the line waiting for lunch, and I told him of my lack of success in getting a speaker to bring this news to attention, and said that I would therefore have to do it myself. He asked me not to do so. I had just assumed that he would be supportive in bringing this information to attention, and so was quite surprised at his response. I never asked him why he wanted this information withheld – maybe because I was so surprised at his response.

Later in the day I separately told Montagnier and Chermann that I had not succeeded in my attempts to get one of the speakers to announce the news, and that at the end of the meeting, I would just do so myself. One of them said that he did not want to be in the room when I did so. The other who was sitting next to me, asked that I sit elsewhere when I made the announcement. Quite clearly they were concerned that there might be some thought that they put me up to this. Of course this was entirely my initiative, but illustrates the embarrassment I think they felt at being in a position where it had become known that the new virus Dr Gallo had discovered turned out to be the one they had in fact isolated earlier.

When the meeting ended I did indeed go up to the microphone and made this announcement. This is accurately described by Randy Shilts in “And the Band Played On”. I add the relevant paragraphs from the book here:

February 8, 1985 HARLEY HOTEL,

NEW YORK CITY

Dr. Joseph Sonnabend looked troubled. The panel of journalists, which included most of the nation's leading reporters on the AIDS epidemic, looked confused.

"The implications are terribly important," said Sonnabend cautiously.

Sonnabend, one of New York City's leading AIDS doctors, was trying to explain the significance of an earlier presentation by Dr. Luc Montagnier in the day-long AIDS

conference co-sponsored by the AIDS Medical Foundation and the Scientists' Institute for Public Information.

In his patrician, professorial manner, Montagnier had described the genetic sequencing the Pasteur Institute had performed on the prototypes of the three AIDS viruses, LAV, HTLV-III, and Jay Levy's ARV. The gene sequences of the French LAV and ARV varied by about 6 percent, which was normal, the scientists at the conference agreed. The genes of any two different isolates of the same virus are expected to deviate from each other, usually by 6 to 20 percent. Montagnier's lips tightened, however, when he said flatly that the genetic sequence of the HTLV-III prototype isolate had varied from LAV by less than one percent.

Those words started the AIDS researchers present mumbling among themselves, even while the reporters yawned. Journalists had long assumed HTLV-III, LAV, and ARV were all different names for one virus. The reporters, however, were missing the point.

"It would appear that HTLV and LAV are too identical," Sonnabend said, stepping delicately around the fundamental issue. "They are identical to a degree that would not be anticipated with two independent isolates from the same family."

The reporters still didn't get it. The doctors did, but they were afraid to say it aloud.

"Would you be brave enough to voice explicitly the implication of what you're saying here?" one doctor shouted to Sonnabend.

"No, I wouldn't," Sonnabend answered. "I'm not the right person to be saying that."

"Neither am I," the other doctor said.

"What are you talking about here?" asked the Associated Press reporter. "Do you know something that you're not saying?"

"They appear to be the same actual isolate," Sonnabend finally said. "Or some strange coincidence."

"What are you suggesting?" somebody asked.

Dr. Mathilde Krim, who had organized the conference, stepped to the microphone.

"Dr. Montagnier," she said, "felt very appropriately that he was not the person to point this out."

"Nobody's pointed it out quite exactly yet," said one of the exasperated reporters.

"It's perhaps a complicated notion for you to understand," Krim said, "but I think you are coming close."

Veteran science writer Donald Drake of the *Philadelphia Inquirer* was one of the two or three journalists in the room who understood the implications of Sonnabend's remarks.

"Are you suggesting that Gallo swiped his virus from the French?" Drake asked.

"Or Montagnier swiped Gallo's virus, or we are dealing with a very strange coincidence," said Sonnabend diplomatically.

"A light bulb goes off," said the *San Francisco Chronicle* reporter on the panel.

The reporters now understood what the scientists had been discussing in Harley Hotel hallways all day. In the world of virology, it was inconceivable that there could be a genetic variation of less than one percent between two different viral isolates. That would be like finding two identical snowflakes. It simply didn't happen.

What made the similarities more unlikely was that the prototype isolates of LAV and HTLV-III were supposed to have been taken seventeen months apart, from two different men living on two different continents. The only way to account for the identical properties of the two prototypes was if they were the same virus taken from the same person. If this was true, the scientists speculated privately, it would appear that Dr. Gallo had not only stolen credit from the French for discovering the AIDS virus, he had stolen the virus itself.

Montagnier knew enough about the chronology of Gallo's discovery to be suspicious of this, although he never publicly made the accusation himself. Even by Gallo's account, he did not isolate HTLV-III until late 1983—well after September 1983 when the Pasteur Institute sent him LAV samples. To both the French researchers and many of the AIDS doctors at the conference that day, Montagnier's comparisons indicated that the NCI prototype of HTLV-III, announced in April 1984, was probably grown from the same virus the French had cultured in January 1983. This had the makings of a scientific scandal of immense proportions.

On a number of counts, the AIDS Medical Foundation conference in New York on that bitterly cold Friday in February delivered the first sign of what was to come in the AIDS epidemic. The butcher's bill was so high that long-tolerated transgressions could no longer be ignored. Reckoning was at hand.

The most astounding consequence of this event at the Harley Hotel, where those who were regarded as the best science writers in America had been assembled to hear research results directly from the researchers, was that this important information was omitted from all the reports I saw. Randy Shilts was there, and maybe he did report on the meeting in the San Francisco Chronicle; but the above report is the only one I know of. As it is in a book, unlike the other reports of the meeting which appeared within days, his was published two years later. Certainly the New York Times reporter chose not to mention it. In fact the common identity of these viruses was kept from the public by these eminent journalists so effectively, that it was left to John Crewdson, a reporter for the Chicago Tribune to later bring this to public attention. Why had the reporters who were present at the Harley Hotel meeting in 1985 neglected to report on what was told to them? Even if a light bulb did not go off for all as it did for the San Francisco Chronicle reporter – Randy Shilts, they might have sought clarification. Information was handed to journalists and the fact that they chose not to report it has quite awful implications. The most worrisome is that, if this is an example of how they go about their business, how can the public can trust them as a reliable source of information? Of course questions such as their susceptibility to pressure regarding what they do or do not report, unavoidably come to mind.

Why did David Baltimore ask me not to reveal this information? One will never know, but he certainly lost no time in resigning from the Board of our Foundation. The reason he gave was that he thought it irresponsible to have allowed Jean-Claude Chermann to report on his use of HPA-23, an antiviral he had used to try and treat patients. I do not know if he expressed similar

reservations regarding the use by Dr Broder, then Head of the National Cancer Institute, of Suramin, an extremely toxic drug, to treat patients with AIDS.

Contamination of tissue cultures by viruses is common. Indeed, even LAV cultures at the Pasteur institute were also contaminated by another similar virus LAV-Lai, so actually two viruses, LAV-Bru and LAV-Lai were sent to Dr Gallo by the French scientists. It is probable that LAV-Lai contaminated cultures in Dr Gallo's lab as it was found to have done in several other labs. There are no villains in this story; there was no theft – only contamination by a strain of HIV (Lai) which had a propensity to overgrow other strains. However this incident was not journalism's finest moment. For science writers, they appeared to be poorly informed, and too eager to impute wrongdoing to Dr Gallo. And finally, failing to inform the public of the common identity of LAV and HTLV, either because they could not comprehend the evidence or for other reasons. Randy Shilts was the exception regarding his comprehension and reporting of the common identity of these viruses.

Accidental contamination of tissue cultures, although a depressingly familiar occurrence, would probably not have been thought of under the circumstances where the common identity of these two viruses was revealed. At the time, I was just not quick enough to point out that contamination might be a possible explanation. The journalists were not familiar with the problem of contamination and so were free to speculate on intentional wrongdoing. I was quite nervous and absolutely hated having to make this announcement – which really is no excuse for not suggesting contamination as a possibility. I had experienced this problem myself. Still, had other scientists been willing to inform the journalists that LAV and HTLV-3 were identical, - after all , it was perhaps the most important information that had just become available, and join in with making this important announcement, it is more likely that we would have considered the possibility of contamination. A considered announcement might have been made, rather than the impromptu one forced on me by the unwillingness of others to do so. Ultimately the rather unseemly attributions of wrongdoing could be seen to have stemmed from a reluctance to be open and truthful on the part of the presenters at the meeting.

I should add that in making the announcement at the end of the meeting, I knew that I was going against the wishes of those who wanted to be silent on the issue. These were among the most respected researchers in the field, and I was perfectly aware that I was incurring their displeasure. And at some level I knew that I was courting enmity.

A last note regarding the Nobel prize: Jean-Claude Chermann played an essential role in the discovery of HIV. I wonder why he was excluded from the award?