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Rhetoric and the AIDS Virus Hunt

Carol Reeves

By comparing the papers produced by the laboratory teams of Robert Gallo and Jean Luc Montagnier during the AIDS virus hunt, we have an opportunity to discern the fine line between a bold, explicit rhetoric that may convince as well as offend and a bald, reserved rhetoric that may actually conceal important implications. Going too far in either direction may create misunderstandings and ethical dilemmas as will be demonstrated in a textual analysis deepened by an exploration of historical context and interviews with key participants. Since a public health crisis calls upon communication that thwarts misunderstandings, scientists should understand the nuances of particular contexts and the blessings and banes of specific rhetorics employed in those contexts. Key words: rhetoric of science, rhetoric and medicine, rhetoric and AIDS, hypothesis-generation, scientific competition.

I have learned more of politics than of science during all this. I never thought I would have to be a good salesman in order to be heard.

-Jean Luc Montagnier, co-discoverer of HIV (in Shilts 496)

Montagnier did not conclude that their virus was the cause of AIDS. His style of presentation was matter of fact.

-Robert Gallo, co-discoverer of HIV (1991, 167).

The first act of *King Lear*, the king asks his three daughters "which of you shall we say doth love us most?" (I, i, line 52). After hearing her sisters' flattering amplifications, Cordelia refuses to use words "more opulent" to prove her devotion, insisting that her love is "richer than my tongue" (I, i, 80) and should speak for itself, without the "glib and oily art to speak and purpose not" (I, i, 228). Unfortunately, Cordelia's matter-of-factness, her refusal to exploit the resources of language to mollify her dear, old, deranged dad, confuses and offends him, resulting in her banishment. Though initially winning their father's favor with effusive rhetoric, Goneril and Regan fare no better in the end. And Lear loses all he loves because his ego and emotional vulnerability compel him to believe the wrong people, listen to the wrong rhetorics.

Cordelia's dilemma¹ encapsulates the question of how one proceeds rhetorically in matters of great import-when one's father is parceling out his estate or when the world awaits an explanation of a catastrophic epidemic disease. In 1983 and 1984, the medical and lay communities were asking scientists "which of you shall we say doth" have an explanation and a blood test for AIDS? And those providing answers faced a critical rhetorical situation involving diverse audiences, competing explanations, and the opportunity to garner patents and prizes. By May, 1983, when the first papers from the Robert Gallo and Jean Luc Montagnier laboratories were published in *Science*, there had

emerged a cacophony of voices, a proliferation of causal explanations of AIDS ranging from "lifestyle" factors to fungi (See Grmek, chapter 5; Gallo [1991] 127-162). Both Gallo and Montagnier, as well as many other virologists, believed these theories to be faulty. The challenge in 1983, as Gallo saw it, was "to stem the flow of ridiculous theories" (Gallo, interview, National Cancer Institute, August 3, 1995)² and theoretically to align the emerging AIDS research community with a synthesizing hypothesis. But it was not just the biomedical community that awaited an explanation for AIDS and that would serve as an audience for published research reports. The blood banking industry was not budging toward any additional screening until there was solid proof and a reliable blood test despite early warnings from virologists and despite seemingly obvious cases of transfusion AIDS. Journalists were combing science journals for the latest breaking AIDS news, feeding public hysteria and homophobia, reporting everything they found, including odd or outlandish explanations such as that semen caused AIDS, that amyl nitrite caused AIDS, and that AIDS was actually a variant strain of syphilis. Many among the first AIDS population and their caregivers also sought information from science journals when their own physicians could provide no definitive explanation or effective treatment.³

How should one proceed rhetorically in such a context? Boldly or baldly?

In an essay comparing Oswald Avery's and Crick and Watson's papers on DNA, Carolyn Miller suggests that the appropriateness of a decision to be bold or bald is governed by the opportunities available in a given situational context. Extending the Greek idea of *kairos* to the rhetoric of science, Miller suggests that a *kairos* within a research community is an opening, an "opportunity provided by explanatory problems, and a social or professional space, understood as the opportunity provided by a forum of interaction" (323) in which a claim may be expected and accommodated. A bolder rhetoric, such as Crick and Watson's, may be the result of scientists' confidence, not only in their claim's veracity but also in its timeliness, while a balder, more cautious rhetoric may reflect sensitivity to a closed professional space. Miller explains that Avery's failure to influence researchers in genetics and Crick and Watson's success was at least partially due to the vastly different professional forums they faced. While Avery offered in 1944 what is now considered the first paper demonstrating that DNA is the genetic substance, and while he had confidence in his findings, there was not yet an opportunity, an intellectual opening or professional forum for his findings. Miller suggests that his bald rhetoric-his reluctance to proclaim boldly or to speculate-reflects his sense that the time was not right for proclamation and was appropriate to the situational context. Crick and Watson, publishing their double-helix model of DNA nine years later, had both the opportunity and the professional space within which their claim could be accommodated. They took full advantage of the situation; as Miller notes, "their coy self-assurance . . . can be seen as opportune, rather than sheerly idiosyncratic . . ." (311).

The situational context of epidemic is replete with exigencies-the urgent need for a synthesizing hypothesis, definitive etiologic explanations, accurate blood tests; the economic and political demand for patents on those blood tests; the social necessity of bestowing discoverer status upon the scientists responsible for leading the way; and, finally, the emotional demands for reassurance and hope that all is not lost. All these exigencies provide critical opportunities for making claims. In 1983 and 1984, Robert Gallo and Jean Luc Montagnier responded to these opportunities with very different

rhetorics that are monuments to the blessings and banes of bold, explicit persuasion on the one hand and bald, elliptical reserve on the other.

Look again at the above epigraphs. Jean Luc Montagnier, the Pasteur Institute virologist whose laboratory first isolated HIV, equates rhetoric with salesmanship, exhibiting a Cordelia-like trust that the evidence should speak for itself. Commenting on his approach to reporting laboratory results, Montagnier explained that the Cartesian influence upon the Loire valley where he grew up gave him a distrust of language and a "sense of measure, logic" which he believes are not part of American culture: "the US is not very Cartesian" (1988, 128).⁴ The French team's rhetoric-cautious, elliptical, matter-of-fact-resists overt persuasion and relies on specialized readers to make necessary connections and draw meaningful implications on their own. As Françoise Barre-Sinoussi, lead author of the first French paper on the AIDS virus, explains, "We thought the readers would put two and two together. We didn't think we needed to elaborate" (interview, Washington, DC, July 23, 1995).⁵ While the French team's rhetoric was conservative and reasonable, it underplayed, as I will show, the meaning of their evidence, the novelty of the French virus and its importance, so that the French team made themselves vulnerable to accusations by Gallo and other scientists of not having enough evidence, thus losing, at first, their bid for discoverer status and their rights to the first patent on the blood test.

In his statement, Robert Gallo, whose National Cancer Institute Laboratory won the first patent on the AIDS blood test, exhibits his understanding of the inevitability of sensory action, calling to mind Lear's recommendation to Cordelia to "Mend your speech a little lest it may mar your fortunes" (I, i, 96-98). Noted for his rhetorical brashness in public and in his writing, Gallo believes his rhetoric is typically American: "One French reporter told me, 'You're 150% American'. I took it not as a compliment because she didn't necessarily mean it as a compliment" (interview). But Gallo also honed his rhetoric in the seventies while tackling the frontiers of medical science. Working against the dogma at the time, the widely accepted view that cancer-causing viruses did not exist in humans, Gallo persuaded his audiences to accept the existence of Human T Cell Leukemia Virus (HTLV). But Goneril and Regan's empty persuasion is not enough, of course. As Gallo explains, "You must have the proof, the evidence, but if you're confident, you should be direct, bold. You learn that when you're working against dogma" (interview). Gallo's rhetoric-bold, aggressive, optimistic-exploits the resources of scientific rhetoric to establish theoretical orientation as well as to reassure the world that science can solve the AIDS puzzle. Unfortunately, such a rhetoric can also manipulate and mislead, generating hostility and suspicion. While his aggressive approach helped early on to establish the hypothesis, the *idea* of a viral etiology in AIDS, it later attracted criticism from a panel of legislators and scientists who equated Gallo's bold rhetoric with unethical behavior.

By comparing the papers produced by the Gallo and Montagnier research teams during the AIDS virus hunt, we have an opportunity to discern the fine line between rhetorical aggression that may convince as well as offend and rhetorical passivity that may actually conceal important implications. Going too far across that line in either direction may create misunderstandings and ethical dilemmas as I will demonstrate in a textual analysis deepened by an exploration of historical context and audience reactions as well as by my interviews with key participants in the AIDS virus hunt.

Stemming the Flow of Ridiculous Theories: Establishing the Hypothesis in 1983

By May, 1983, Robert Gallo and his colleagues had already suggested that the leukemia virus he discovered in the seventies, Human T-cell Leukemia Virus (HTLV), was responsible for AIDS, suggesting at various conferences in 1982 and early 1983 that the AIDS virus could be an HTLV (see Gallo, 1991, 136-162 and Grmek 58) and publishing in April the first announcement that HTLV-1 had been isolated and cultured from the blood of a person with AIDS (CDC). Gallo defends the efforts to connect HTLV with AIDS because this was the only known family of human retroviruses and because "certain questions must be locked up behind you before you can comfortably go on to the next possibility" (144-145). However, AIDS historian Mirko Grmek argues that HTLV was more of an epistemological barrier "that derives from the legacy of prior successes, especially their [the discoverers'] own" (71). At any rate, Gallo was well aware that he had both the opportunity to make his claim and the professional space within which to make it. He enjoyed public confidence in his laboratory garnered by previous success, and his claim fit nicely into the conceptual groundwork that he himself had laid—the accepted reality of human cancer viruses. He responded to this opportunity boldly.

Meanwhile, at Montagnier's Pasteur Institute Lab, Françoise Barre-Sinoussi had been trying to grow a virus isolated from the lymph nodes of a man with lymphadenopathy or swollen glands, a condition that was coming to be recognized as a characteristic of the early stages of AIDS. From the beginning, the French virus looked and behaved differently from known isolates of HTLV. First, unlike an oncovirus, this virus did not encourage proliferation of cells it infected; in fact, Françoise Barre-Sinoussi commented in her laboratory notes that the virus had "cell-killing" effects (Grmek 64). Since variants of the same virus family may behave differently, it was possible that the new virus was a member of HTLV. But it was also just as possible that it wasn't. In reporting their laboratory observations and interpreting their data, the French were in a delicate political and rhetorical position. *Political* advantage might be gained from deferring to the powerful Gallo and the National Cancer Institute and interpreting their data conservatively, aligning their virus with the known entity, HTLV, and its discoverer. But *rhetorical* advantage might also be gained by arguing from their evidence that they had an entirely new class of virus. They chose to take the former course. Barre-Sinoussi, lead author of the first *Science* paper from the Montagnier laboratory, asserts, "We knew we weren't dealing with a leukemia virus, with HTLV-I or II. A lymphotropic [attacking T-cells] virus, yes, but not a Leukemia" (interview). But whether or not this new virus was a member of the HTLV family or whether it represented an entirely different or new family of viruses could not be answered definitively. Barre-Sinoussi now says that they "could have said it wasn't HTLV at all, but all we really had was the evidence that the virus was killing cells, and we didn't know why, but we could have said it wasn't HTLV. Instead, we were a little shy, a little deferential" (interview). With evidence of a new agent that didn't behave like known variants of the HTLV family, the French scientists had to specify, if not a new family of viruses, at least the novelty of the virus they had isolated. They had to articulate their contributions against the rhetorical and institutional authority of the discoverer of that well-established candidate. They responded with a cautiously elliptical rhetoric that actually downplayed their contributions and the novelty of their findings.

Science, 1983: The First Papers

In the May, 20, 1983 issue, *Science* editor Ruth Kulstad rushed into print the first group of papers about a viral cause of AIDS.⁶ The French (Barre-Sinoussé, et al.) report isolation and culturing of a retrovirus-that the authors suggest is a newly discovered isolate of the HTLV family-from patients at risk for AIDS who have lymphadenopathy (swollen glands). The paper also reports evidence that this virus did not grow well in cell cultures, did not contain key HTLV molecular proteins, and that their patients had antibodies to this virus but not to any known HTLV variant. Despite the evidence they report, the French team's implicit rhetoric fails to specify and emphasize the *novelty* of their findings.

The American papers all foreground HTLV-I as the likely candidate. The paper whose lead author is Gallo, "Isolation of Human T-Cell Leukemia Virus in Acquired Immune Deficiency Syndrome (AIDS)," reports one isolation of an HTLV-related virus from an AIDS patient, while the other Gallo laboratory paper (Gelmann, et al.) reports finding proviral DNA of HTLV in two AIDS patients. The third American paper, (Essex, et al.) did appear to contain striking evidence-38% of the people tested had antibodies to one variant of HTLV.⁷

Gallo explains that he knew at the time he had no linkage evidence; his laboratory had found only 8% of AIDS patients tested having antibodies to HTLV. But with Essex's antibody results and his own intuition, Gallo had confidence in the HTLV hypothesis and believed that despite the fact that no etiologic conclusions could be drawn from these papers, it was prudent to

put forward the hypothesis, to focus evidence on retroviruses, to establish a record that this is a serious hypothesis, and what I thought I was isolating would be predictably variants of HTLV I. The hypothesis was incorrect but the idea was productive (interview).

In his discussion of warrants, Prelli notes that "when rhetoric is made about controversial and contingent matters, the discourse must be grounded in positive standards of value if it is to be judged reasonable" (29). A productive idea emerging from a cacophonous medley of unproductive ideas is a positive standard in any science and a necessity in biomedical science, especially during an epidemic. In these papers, Gallo emphasizes the thematic productivity of HTLV through organizational and linguistic choices that *direct* reader attention to positive standards of value, building a theoretical frame for the *idea-if* not definitive evidence-of HTLV as the likely cause of AIDS.

The Gallo Rhetoric

The Gallo rhetoric-which I will describe by focusing my remarks on Gallo, et al. (1983)-is rich in rhetorical strategy. The main evidence reported in the paper is isolation and culturing of an HTLV-related retrovirus from the blood of three AIDS patients. Since many kinds of viruses and other agents had already been found in the bodies of AIDS patients-cytomegalovirus, for example-it was necessary to prove the causal link. But with only 8% of the patients testing positive for HTLV-I antibodies in his own studies, Gallo had no linkage evidence. Linkage between HTLV and AIDS is not made with direct evidence in this paper; it is *suggested as a possibility*, a powerful, sustaining theory, through the resources of language. With a tiny bit of evidence and some rhetorical boldness, Gallo builds the plausibility of a massive idea through linguistic and organizational choices that emphasize possibility and optimism.

The possibility of HTLV as the cause of AIDS is first developed in the introduction which begins with an overview of knowledge about HTLV, its clinical manifestations, and the Gallo laboratory's contributions to that knowledge. The first sentences foreground HTLV and its discoverers. Of the sixteen references to established knowledge about HTLV in the first paragraph, ten are to the Gallo laboratory's publications. This summary, with its appeals to agency and community consensus, prepares the readers for the idea fueling the team's current investigations: "We are *testing the possibility* that HTLV is associated with the newly described acquired immune deficiency syndrome (AIDS)" (1983, 865; emphasis added).⁸

The phrase "We are testing the possibility" may be compared with Crick and Watson's famous phrase introducing their double helix structure for DNA, "We wish to suggest" which scholars have noted for its elegant ethos (Halloran) and its politeness (Myers). But in Miller's reading, Crick and Watson were opportunists, confident that the significance of their discovery was obvious to readers. Likewise, Gallo's position as "the father of human retrovirology" as he had been called, and his directorship of the powerful National Cancer Institute Laboratory at the National Institutes of Health allowed him to take full advantage of the professional space for HTLV that he had created and to be confident that the significance of this "possibility" and his laboratory's ability to test it would be obvious to readers.

The clinical manifestations of AIDS are enumerated in the second introductory paragraph, leading to another statement announcing the actual evidence the team has to report: "Here *we describe the isolation* of HTLV from peripheral blood T lymphocytes from one US patient with AIDS. . . ." (866). The idea, the possibility, in the research purpose statement rhetorically frames the modest bit of evidence. As a close reading of the paper reveals, it is that massive idea that emerges not so much from that evidence as from the argumentative and linguistic resources that Gallo employs. With virus isolation from only three AIDS patients to report, Gallo chooses to emphasize a half-full rather than a half-empty cup.

Throughout the methods narrative, employing the language of certainty, Gallo emphasizes the similarity between HTLV I and the virus isolated from his patients. After describing how the patient's peripheral blood T cells were grown, Gallo provides the main evidence for their claim that the virus isolated from patients with AIDS is an HTLV, explaining that "*we determined* that the T cells contained the HTLV core proteins p19 and p24, respectively. These results also *showed* that the new *isolate belongs to* the HTLV group" (866). The similarities between the new virus and HTLV are repeated throughout the methods narrative to the point of redundancy:

...and other properties *similar to* other HTLV-infected cord blood T cells (866).

...extracts . . . were *very similar to* those for p24 from isolates of the HTLV-I subgroup, indicating that the isolate from patient EP is *closely related* to the HTLV-I subgroup (866).

This "intentional redundancy" as Gross refers to it in his discussion of style in biological prose (71) is significant because what is highlighted—the similarity between the very few virus isolates from very few AIDS patients and HTLV-I—is *not* evidence that a link between HTLV-1 and AIDS has been established. Yet the redundancy builds a thematic link between the evidence and the possibility that an HTLV sub-member is the cause of AIDS.

Next, Gallo makes a highly important symbolic move. He names their virus: "*We call*

this isolate HTLV-IIEP," (866) thus making a move toward rhetorical ownership and discoverer status. The French do not name their virus in Barre-Sinoussi, et al.

In the conclusion, the Gallo team takes full advantage of the rhetorical emphasis that can be used in the summation statement. In discussing conclusions, Swales quotes a biologist informant as claiming that "the first paragraph of a discussion should always be reserved for the strongest claim in the study" (172). Here, Gallo asserts an idea, a possibility that lacks proof but is nonetheless "interesting to consider." He states, "Whether or not the etiologic agent of AIDS is an HTLV, it is *interesting to consider* the *possible* routes of transmission of such an agent" (867). A few sentences later, even though he has conceded that he has no direct proof "whether or not the etiologic agent of AIDS is an HTLV," Gallo attempts to directly influence readers' opinions by emphasizing both the possibility that the cause of AIDS is, in fact, HTLV and the possibilities for research afforded by that explanation. Here Gallo reminds readers of the future relevance of his laboratory's coculturing method in detecting HTLV in AIDS by stating that "it may therefore be *possible* to detect HTLV antigen in these secretions and to transmit the virus to cord blood by the coculturing method" (867). The paper ends by identifying a common goal, an animal model, that could be met if the authors are correct about HTLV:

... should this virus be established as the causative agent of AIDS it may also be *possible* to develop an animal model of the disease (867).

To end with such optimism and reassurance of technological possibility is to attempt to influence readers' attitudes by reminding them that science can and will solve the AIDS mystery and inviting them into this "interesting" (see quote above) scientific quest. To emphasize optimism about the capabilities of science early in the epidemic-in early research reports as well as in media events-was to appeal to values held by audiences, esoteric and exoteric, about the capability of science. AIDS was a challenge, but, according to a pervasive faith in science, no more a challenge than Ebola or Legionnaires or Polio or Rabies or Anthrax and on and on.

In his reading of Darwin, Kitcher (1991) notes Darwin's appeal to optimism at the end his work where he outlines the innovations in natural science that result from the theory of evolution. "As I read Darwin," Kitcher writes, "join me,' he seems to say, 'and leave your mark upon science' " (20). Gallo is also asking his readers to join him in keeping the faith, and he is addressing a situational context that allows him to do so: there is an urgent gap that needs to be filled by a productive idea and a professional forum in which technological usefulness and productive ideas are highly valued and in which he has established his reputation for providing both.

But such rhetorical maneuvering risks the accusation of unethical rhetorical manipulation, of misleading audiences and downplaying the severity of the problem. Optimism about the possibility that science could quickly and easily solve the AIDS puzzle rang a hollow note as morbidity and mortality continued to rise without effective treatments. Referring to this early period of AIDS research, Abraham Verghese writes that "to say this was a time of unreal and unparalleled confidence, bordering on conceit, in the Western medical world is to understate things" (24) and that "there seemed no reason to believe when AIDS arrived on the scene that we would not transfix it with our divining needles, lyse it with our potions, swallow it and digest it in the great vats of eighties technology" (25). Ironically, Gallo, writing in the *Scientific American* in 1987-well after

the etiology of AIDS had been established-admonishes those who would assume science can solve all human problems. He argues that the discovery of retroviruses and their "capacity to cause extraordinarily complex and devastating disease has exposed that claim [that science has conquered infectious disease] for what it was: hubris. Nature is never truly conquered" (56). While this statement seems to contradict Gallo's earlier optimism, it merely indicates his sensitivity to the shifting ground of rhetoric during a plague. When all seems hopeless in the beginning, perhaps the reassuring voice-as it is expressed in Gallo's early AIDS virus papers-is needed. Later, after some questions have been answered, admonishments related to the community's values and behavior that might have impeded progress, especially when voiced by a recognized authority, are, perhaps, expected. And Gallo is certainly sensitive to his audience's expectations.

In the *Science* paper, Gallo makes several moves-naming a phenomenon, asserting priority, speculating about outcomes of a hypothesis that he has little solid evidence to support-that Myers would classify as those which "assert the privileges of the individual and suspend the absolute authority of the group" (6). However, it is possible to defend this rhetoric as necessary under the circumstances to establish leadership in a disorganized pursuit. According to Gallo, "We were incorrect about HTLV, but we had the right idea, and we were correct to present it the way we did. I have no regrets about our presentation. It was a powerful idea and a powerful presentation" (interview). By "powerful idea," Gallo means one that can serve to reorient the research community toward productivity, and he feels his idea did accomplish just that. By "powerful presentation," he means forceful, no-holds-barred rhetoric that reveals a writer's confidence in his work and that succeeds in creating confidence in the audience. Such rhetoric can, as Kitcher notes, deceive scientists "into overestimating the promise of a program," (20) but it can also protect "ideas from being abandoned before they have had a chance to develop, [and thereby] the cognitive aims of the community may thus be promoted" (20). In the AIDS virus hunt, both results may have occurred-a promising idea helped narrow etiologic explanations to virology while it detracted from other promising ideas, including those of the French team.

The French Rhetoric

In their paper published in the same issue of *Science*, Barre-Sinoussi, et al., the French scientists employ a conservative and cautious rhetoric that relies on prudent specialized readers to make necessary connections and draw conclusions on their own. It is a bald, matter-of-fact approach that resists overt persuasion in the form of appeals to thematic orientations such as the productive idea, technological application, or optimism.

While the Gallo team builds a theoretical foundation for the *idea* about which they are "testing the possibility," the French begin their paper by summarizing principle knowledge about AIDS in carefully hedged prose:

The pronounced depression of cellular immunity ... *suggests* that T cells or a subset of T cells *might* be a preferential target for the putative infectious agent. (1983, 868).

The depressed cellular immunity *may* result in serious opportunistic infections in AIDS patients ... [swollen lymph nodes] ... *may* correspond to an early or milder form of the disease (868).

All this information is meant to prepare the reader for the team's announcement of major finding and conclusion contained in the final sentence of the first paragraph: "We *report* here the isolation of a *novel* retrovirus from a lymph node of a homosexual patient with

multiple lymphadenopathies. The virus *appears* to be a member of the human T-cell leukemia virus (HTLV) family" (868). The French team's verb "report," in contrast to Gallo's "testing the possibility," is indicative of the main differences between their paper and Gallo's. The French paper is bald reportage, unlike the Gallo paper, containing no theoretical discussion, no speculation, no overt attempts to influence readers. Rather than boldly argue that they had discovered a new virus family, the French align their discovery with HTLV, announce that they have a new isolate of HTLV rather than a new virus altogether. But in order to establish the originality of their contribution, they were obligated rhetorically to specify the novelty of the virus they had tried unsuccessfully to grow in their laboratory.

In the second paragraph of their paper, the French emphasize what their virus shares with HTLV-antigen reactivity and similarities in reverse transcriptase-but they do not mention here the most striking evidence of their virus' novelty-that their patient's serum did not react with antibodies to HTLV-I and that unlike HTLV-I, their virus did not immortalize cells. By relegating that information to the methods narrative, rather than emphasizing it in the early sections of the paper, the French miss a rhetorical opportunity to specify the novelty of their virus, for HTLV isolates were known to immortalize cells, not destroy them.

Their main conclusion comes at the end of paragraph two:

We tentatively conclude that this virus, as well as all previous HTLV isolates, belong to a family of T-lymphotropic retroviruses that are horizontally transmitted in humans and may be involved in several pathological syndromes, including AIDS (868).

They offer a new characterization of a virus family and provide a more generic classification, replacing "leukemia" with "lymphotropic." Yet, their cautious articulation of their innovation and their alignment with HTLV are antithetical to the bolder announcement we see in Gallo, et al. which actually reports less evidence. Given their laboratory experience, they might have argued just as tentatively that their virus did not belong to the HTLV family of human retroviruses, which is what they eventually did argue in subsequent papers. They did have enough important evidence to report in this paper to be bold: They had isolated a retrovirus from a patient with pre-AIDs symptoms, cultured it, infected healthy T-cells with it, and observed that it did not immortalize cells. But as Grmek notes, "Despite all this, the writers avoided shouting 'Eureka!'" (66).

Throughout the rest of the French paper, the writers resist exploiting arrangement and language options to emphasize the novelty of their virus the way Gallo emphasizes the possibility of HTLV. Their most striking evidence of novelty becomes reported anomaly as a result of elliptical presentation.

In the methods narrative, key evidence of their virus's unique qualities gets buried organizationally and de-emphasized linguistically. In the middle of paragraph six, for example, they report matter of factly that once they placed the virus on blood cells to encourage growth:

Virus production continued for 15 days and decreased thereafter, in parallel with the decline of lymphocyte proliferation (868-869).

This elliptical expression of a conclusive observation was, according to Françoise Barre-Sinoussi, considered sufficient for knowledgeable readers to grasp the implications. "We expected them to put two and two together, to see that we were reporting

something very new, very exciting. We were very excited about this work" (interview). Yet the French do not exploit redundancy, as does Gallo, and neither emphasize nor comment on the meaning of this interesting finding. Gross notes that "the brute facts of science are stellar positions or test-tube residues *under a certain description*; and it is these descriptions that constitute meaning in the sciences" (emphasis in original, 11). Here, meaningful description of what had been noted as a fact in Barre-Sinoussi's laboratory notebooks is missing and so is the idea, the meaning of this fact—its mystery and novelty.⁹ It is entirely possible that even the most prudent readers did not "put two and two together."

Near the end of the methods narrative, the French place additional important evidence of the novelty of their virus that is mentioned nowhere else in the paper. They report that a protein in virus-infected cells from their patient was not recognized by antiserum to the HTLV core protein and that HTLV protein "was recognized by antibodies to HTLV but not by serum from patient 1" (870). In other words, blood from their patient which was infected with their virus did not react to antibodies to HTLV. This is an extremely important finding that speaks to the issue of antibody testing as well as the novelty of the French virus. However, excluded from the paper's main argumentative framework, this information becomes an unexplained anomaly rather than evidence supporting a bolder claim that a new virus had been discovered.

In their conclusion, the French refrain from influencing readers by emphasizing the novelty of their finding as the Gallo laboratory emphasized possibility and optimism. In the first sentence of their final paragraph, where writers may remind readers of the most important conclusions, Barre-Sinoussi, et al. choose to be cautious rather than to try to direct opinion or attitudes: "The role of this virus in the etiology of AIDS remains to be determined" (870). Although this statement is appropriate and responsible, it is markedly timid compared to the boldness of the summation statement in the Gallo paper.

In the remaining few sentences of their paper, the French finally address their virus's cell-killing effects. The infected lymphocytes "*seemed to* have no increased growth potential in vitro compared to the uninfected cells" (870). "Seemed to?" They had observed this phenomenon directly. In her lab notes, Barre-Sinoussi recorded her observations of the virus' cytolytic (or cell-killing) effects (Grmek 65). So why did they address this issue so passively in their paper? As Barre-Sinoussi explains, "we didn't feel we had enough evidence to make much of the protein differences and the cell-killing effects" (interview). So they do not discuss or rhetorically heighten the implications of that evidence. Instead, they end their paper by suggesting that the cell-killing activity of the virus is the result of "other factors" that may overload the patients' immune defense and cause "irreversible depletion of T cells involved in cellular immunity" (870). This final sentence actually dismisses the possibility that their virus was killing cells. As Lear says to the plain-spoken Cordelia after she says she has nothing to say, "Nothing will come of nothing!" (I, ii, 92). The tacit appeals to relevance and reassurance in the Gallo paper are absent from the French paper which ends cautiously with explanation of anomaly rather than a description of possibility.

The Gallo paper ends by reassuring the audience that while the team doesn't yet have a proven etiologic agent, they have the methods that will ensure eventual discovery. The Barre-Sinoussi paper ends with more questions, more mystery and no rhetorical emphasis on reassurance even though, by some accounts (Grmek 67-68), the French team had more answers than did the Gallo team. Yet, the Gallo team's papers may have had more

immediate influence on readers. Mary Klotman, currently directing clinical research at a Mount Sinai laboratory, was a member of Gallo's lab in 1983. In her view,

the French were more conservative in terms of interpreting their data. I don't know whether it was a confidence issue or just simple conservatism. The going one step further and interpreting the data, making a case, I think, and everyone else thought as well, that the American papers were stronger than the French paper (interview, Mount Sinai Hospital, New York City, July 28, 1995).¹⁰

But wouldn't the French team's conservatism have been prudent under the circumstances? What research team offering a re-characterization of an established phenomenon wouldn't assume that to be scrupulously conservative is to be heard? As Jay Levy, the third scientist to isolate the AIDS virus, said, "When you're going up against mainstream opinion, when you're offering evidence that contradicts some tradition or a very strong consensus, you've got to be very conservative in your prose and in the way you interpret your data, especially if you're a nobody" (interview, University of California at San Francisco, July 24, 1994).¹¹ If we consider that the French team had not yet attracted international acclaim or garnered a reputation like Gallo's in the United States—and they were publishing in an American journal after all—then they might not have been politically or institutionally entitled to be bold.

But not being bold enough when one has evidence that confronts the reigning phenomenon and its discoverer is also to risk being ignored, especially when the situational context demands that the relevance of particular variables be established. In our case, the lack of theoretical orientation that allowed Gallo to suggest the possibility of HTLV may also have provided the French an opportunity to suggest they had a new virus based on a particular variable, the cell-killing effects on their virus, which they eventually named Lymphadenopathy-Associated Virus, (LAV). There are times when "dead" or conventional rhetoric comes to life, as Kitcher notes, such as "when there is debate about whether some variable not on the standard roster might prove relevant" (9). In the AIDS virus hunt, debate eventually surrounded that "variable," that evidence of the cell-killing or cytolytic effects of LAV left under emphasized in the French team's 1983 *Science* paper. A less conservative interpretation of their data and a rhetoric that utilized the resources of organizational and linguistic emphasis without going beyond their data might have persuaded audiences of the relevance of LAV's cytolytic effects much earlier. In a debate about the relevance of some variable, a bolder rhetoric may be necessary in order to "[keep] minority viewpoints alive" (Kitcher 20), and the French team's view that the virus associated with AIDS was unrelated to Gallo's HTLV family was a minority view.

By the fall of 1983, HTLV had entered the discourse. According to Jay Levy, "Everywhere everyone was saying this AIDS virus is in the family of HTLV. Bob wanted to have it that way, and he had great influence at first" (interview). The influence of the HTLV hypothesis was so great, in fact, that in August, 1983, one private laboratory, Biotech, filed a patent on what they called an AIDS blood test which was a modified version of Gallo's blood test for HTLV-I (see Norman 1985).

Meanwhile, the French team found it increasingly difficult to keep their minority viewpoint alive even as they became more confident in their evidence. They began to pronounce their views to journalists with a boldness that they avoided in their published papers. In August, 1983, John Maurice quoted members of the French team as saying, "we believe that LAV, as far as we can tell, is quite different from any retrovirus so far

described," (66) yet, as Gallo notes, "they never made such a claim in any of their papers. They were being indirect" (interview). Meanwhile, the French team was having trouble convincing enough members of their scientific audience that they were on the right track. As Grmek notes, after the first French paper, Barre-Sinoussi, et al., appeared in *Science*, "the way in which the popular and medical press reacted to it, left the Pasteur Institute team with a bitter aftertaste. The specificity of 'their' virus was only secondarily apparent, hidden behind the idea that the cause of AIDS was a type of HTLV" (66). According to Grmek, in both the United States and Europe, there was "Distrust concerning the role of the 'Pasteur virus', poor cousin of rich Uncle Sam" (67) accompanied by the emergence of HTLV as the likely cause and LAV as 'a second' or 'another HTLV-like retrovirus' within the discourse of AIDS research in 1983.

While it was likely that Gallo's emergence was rooted in political and economic pressure in the United States—as Montagnier once complained, "there was terrible pressure in the United States for an American to be the first to discover the virus" (1988, 128)—it is also likely that the emergence of HTLV and Gallo during this early hypothesis-generating period resulted from the Gallo team's hypothesis-generating rhetoric that specified the theoretical reasonableness of HTLV as well as from the French team's failure to rhetorically specify the novelty of LAV. Once *both* teams had linkage evidence, we continue to see boldness on the one hand and passivity on the other.

Patents and Prizes: The Virus Hunt in 1984

By the time the second round of papers from the Gallo and Montagnier laboratories were published in *Science* in the spring of 1984, the stakes were very high with opportunities to game blood test patents, international approbation, a possible Nobel Prize. And there remained another unanswered question about the exact classification of the virus. While both teams had evidence linking their viruses with AIDS, it was unclear whether the two viruses were the same and whether they belonged to the HTLV family or to another class of retroviruses. Establishing the viruses' exact identity, essential in improving diagnosis, treatment, and prevention, involved deliberation about several matters that could not, at first, be established empirically. That is, until genetic sequencing of both viruses was conducted in late 1984 and early 1985, their identity was a matter of deliberation about what constituted membership in a virus family.¹² While Bob Gallo and his colleagues were arguing that the new AIDS virus was an HTLV, Jean Luc Montagnier and his team were arguing that it wasn't. By early 1984, the French had made considerable progress toward characterizing LAV and its similarities with viruses other than HTLV (see Grmek, chapter five) and had the confidence in their data to apply for a US patent on their blood test as early as December, 1983. Disappointed, the French team watched as Gallo received approbation from the medical community, the scientific press, and the lay public. On April 23, 1984, then secretary of Health and Human Services, Margaret Heckler, announced at a press conference that "the probable cause of AIDS has been found" by "our imminent Dr. Robert Gallo" (In Crewdson 2). Gallo eventually won the first blood test patent for the US Department of Health and Human Services in 1985. Eventually, with their patent application going nowhere and their suspicions that the Gallo laboratory had somehow stolen their virus, Montagnier sued the US government for priority rights to the AIDS blood test.

Science, 1984

Margaret Heckler's dramatic announcement that Gallo had discovered the cause of AIDS came a few weeks before *Science* published in May an armada of papers from his laboratory. These papers announce, in a bold and reassuring rhetorical style, that the cause of AIDS is HTLV-III. They report several isolations of the virus from AIDS patients, (Gallo, et al., 1984) the development of a cell system that "was permissive for cytopathic variants of HTLV," (Popovic, et al., 1984) the detection of antibodies to HTLV-III in the serum of patients with AIDS, (Samgadharan, et al, 1984) and serological analysis of the new virus (Schupbach, et al., 1984). The Gallo team also distinguishes their virus from HTLV-I but insists that HTLV-III is still a sub-member because it contained an envelope protein also found in another HTLV sub-member. They also show that HTLV-III attacks immune system helper lymphocytes, OKT4+ cells, which were known to be depleted in AIDS; however, they do not report evidence that HTLV-III selects these cells *only*. The evidence in these papers was deemed overwhelming by the scientific press and by officials with the Department of Health and Human Services (see Crewdson for a historical account).

Later, in July, 1984, as Gallo and his team enjoyed the spotlight for having discovered the cause of AIDS, *Science* published a second paper from the French laboratory (Klatzman, et al, 1984) providing evidence that LAV is different from HTLV-I, that it can be found in the peripheral blood lymphocytes of someone with frank AIDS, and, most importantly, that LAV *selectively* targets T4+ helper lymphocytes-which Gallo's team had not shown for HTLV-III- and that it has cytolytic or cell-killing effects. Their evidence confronts particular points made by the Gallo team, and they boldly reported to sciencejournalists (see Maurice) that they had a new virus, one that was not an HTLV. Once again, the French were in a delicate rhetorical position. Just how boldly would they proclaim their disagreement with Gallo? Given that misunderstandings about the nature of the AIDS virus had already been created by the HTLV nomenclature,¹³ was rhetorical hard ball in order? Klatzmann, et al. chose not to play rhetorical hardball. Their rhetoric is slightly more deliberate and forceful than that presented in Barre-Sinoussi, et al. the year before but still avoids the overtly persuasive tactics employed by the Gallo team.

I will focus the following discussion on a few points of comparison between the American and French rhetorics in these important papers published in *Science* in May and July, 1984 announcing conclusive evidence that the cause of AIDS had been discovered.

The Gallo Team Proclamations vs the Montagnier Team Suggestions

The Gallo laboratory papers in the May 4, 1984 issue of *Science* proclaim and pronounce in linguistically streamlined statements of fact:

These studies of HTLV-III isolates from patients with AIDS and pre-AIDS and from some healthy individuals at risk for AIDS *provide strong evidence* of a causative involvement of the virus in AIDS (Gallo, et al., 1984, 502).

Other bold statements link the new virus causing AIDS with HTLV, as in Sarngadharan, et al. who announce that "the *primary cause of AIDS is another member of the HTLV family*" (Sarngadharan, et al., 1984, 508). Similarly, Schupbach, et al. end their paper by stating that "these immunological and nucleic acid data *clearly indicate* that HTLV-III is a *true* member of the HTLV family. . . ." (505). The linguistic style of these statements-the lack

of hedging modalities, the boldness and certainty-make them appear more factual, as if the cause of AIDS has been sewn up, black boxed, even though several investigators such as V.S. Kalyanaraman and Don Francis at the Centers for Disease Control and Jay Levy at the University of California at San Francisco were convinced that Gallo's team had not demonstrated that HTLV-III belonged to the HTLV family (see Grmek 69-70). Nevertheless, in these papers we have the discourse of proclamation normally found in popular accounts (see Fahnestock 1986) that Gallo employs here and elsewhere (see Reeves, 1992) as a rhetorical resource in an open debate. Such discourse could have produced the aura of consensus, if not among all virologists, at least among institutional bureaucrats, such as Margaret Heckler, who were ready to proclaim that science-American science-had found the cause of AIDS.

The French, in contrast, suggest rather than proclaim. Their paper published two months later, Klatzmann, et al., contains an announcement clothed in carefully hedged language, the bulky and hesitant discourse of hypothesis:

... since LAV infection of T4+ cells impairs their proliferative capacity and causes cytopathogenic effects, the present results support *the hypothesis* that LAV is *involved in* AIDS and point to its *possible interference* with T-cell differentiation (1984, 62).

Although a more aggressive style than we saw in Barre-Sinoussi, this statement does not match the linguistically streamlined effect of announcements in the Gallo papers. "Involved in" is a far cry from the Gallo team's "primary cause of." Given that the French had applied for a patent to an AIDS Blood test using proteins from their virus, this elliptical style does not convey the confidence they obviously felt in their linkage evidence.

Significant competition for recognition and patent rights increases the rhetorical importance of how rivals acknowledge each other, whether a rival is praised or blamed or ignored altogether. The Gallo team employs "non-integral citations" (Swales 114-115) which do not identify interlocutors by name in the text and which can serve, as in this case, to de-emphasize a source. For example, Sarngadharan, et al. cite Barre-Sinoussi, et al. but refuse to name the French virus within the text even though the LAV label was well known by then: "*Another retrovirus* was isolated from a homosexual patient with chronic generalized lymphadenopathy" (506). Likewise, Gallo, et al., in summarizing previously reported evidence of the connection between HTLV and AIDS, nod briefly at the French virus but ignore the French team's evidence that their virus is distinguished from HTLV: "*Another HTLV isolate* was obtained from the lymph nodes of a patient with lymphadenopathy and at risk for AIDS" (Gallo, et al. 501). As Myers notes, "to ignore a claim is to kill it, and it is the omission of a reference rather than outright attack that is the main threat to an individual's positive face" (6). Gallo et al. also remind readers that the French virus had not been kept alive on a cell line and thus could not be fully characterized: "This isolate (the French virus) has been difficult to grow in quantities sufficient to permit its characterization" (501). Another example occurs in Popovic, et al. where near the end of the paper, the French team's evidence that LAV differs from HTLV is questioned. After stating that the French have tried to distinguish LAV from HTLV-III, Popovic, et al. insist that such distinction may be unfounded since the French had not yet found a way to keep the virus alive in culture and so could not

fully characterize it:

These findings suggest that HTLV-III and LAV may be different. However, it is possible that this is due to *insufficient characterization of LAV* because the virus has not yet been transmitted to a permanently growing cell line for true isolation and therefore has been difficult to obtain in quantity (500).

The French team, in Klatzman, et al., in contrast, are infinitely polite, favoring integral citations and avoiding any direct criticisms of the Gallo team's work. Rather than keep HTLV-III and its discoverers unnamed and anonymous in references, as the Americans do with LAV, the French mention Gallo and his virus by name. They report that

Data published by Gallo and co-workers have suggested that HTLV shows a similar tropism in transformed cell lines derived from leukemic patients . . . (Klatzman, et al. 61). Tropism for T4 cells has also been suggested for a new HTLV isolate which resembles LAV, called HTLV-III (61).

In offering their evidence that LAV is different from HTLV and is selective for T4+ lymphocytes, the French delicately remind readers that HTLV has never been found to be selective for T4+ only and can attack not only T4+ but also other types of lymphocytes: "In both situations, most cell lines were T4+, but some others have been described as being T4+ and T8+ or T4- and T8- or even B cells" (61). The inference Klatzmann, et al. expect prudent readers to make is that it is not enough for the Gallo team to show that HTLV goes for T4+ cells; they must show that HTLV is *selective* for T4+ and no other cells in order to argue that HTLV plays a role in AIDS.

Virus nomenclature becomes rhetorical in the battle over whose virus causes AIDS. Here, the Gallo team has changed the HTLV nomenclature from "leukemia" virus to "lymphotropic" virus:

As indicated by Popovic et al., we and others have suggested that specific human *T-lymphotropic* retroviruses (HTLV) cause AIDS. Many properties of HTLV are consistent with this idea (p. 501).

Without crediting the French, who were the first to use the term "lymphotropic" to describe the AIDS virus in Barre-Sinoussi, et al., Gallo makes "a subtle but clever change in meaning of the acronym, HTLV" (Grmek 68). This change provides a more generic category for HTLVs.

The approach to nomenclature in Klatzmann et al. is to call a leukemia virus a leukemia virus. Here is their statement of major finding in the introduction:

We recently discovered a new group of human retroviruses that differ from human T-cell leukemia virus (HTLV-I) (Klatzman, et al. 59).

A forceful linguistic style contributes to the ring of certainty here, but there is a more important rhetorical strategy at work, a polite and suggestive one. The French take a subtle jab at Gallo in the use of "leukemia" in the HTLV nomenclature, reminding the world that HTLV had heretofore been known as a family of viruses that proliferated rather than actively destroyed cells as is the case with AIDS and with their LAV.

As was the case in their 1983 *Science* papers, the 1984 Gallo Laboratory papers also contain generalizations, statements influencing attitudes toward findings or methods,

while the French avoid such generalizing. Near the end of the introduction, for example, Schupbach, et al. state that "Isolation of these viruses was achieved by means of a *novel* system permitting the continuous growth of T-cell clones. . . ." (503). The Popovic, et al. paper ends with a reminder that the cell line containing their AIDS virus would lead to more research opportunities:

The establishment of T-cell populations that continuously grow and produce virus after infection *opens the way* to the routine detection of cytopathic variants of HTLV in AIDS patients and *provides the first opportunity* for detailed immunological and molecular analyses of these viruses (500).

These two statements—bordering on braggadocio—call attention to the ground breaking nature of the methods, insinuating the cultural ethos of discoverers who, through their "novel" approaches, "open the way" and provide opportunities for their community. This kind of appeal to the discoverer ethos is intertwined in community deliberation about who deserves the credit—those who get there first or those who design ways for all of us to get there. Whether or not there is empirical evidence that either laboratory team involved in this debate actually fits neatly into either category is not, perhaps, as interesting from a rhetorical standpoint as the ways in which discoverer status is deliberated and what warrants are at work in this process.

After reading the Gallo team's April, 1984 papers in *Science*, Montagnier and his group were reportedly annoyed at the way their work had been slighted, as Montagnier explained, "I was shocked by the way he [Gallo] presented our data" (In Norman 640). During that spring of 1984, the importance of the French virus seemed hidden behind HTLV. For example, in a review of the latest AIDS research in a May, 1984 issue of *Research News*, Jean Marx states that "a newly discovered subgroup of the human T-cell leukemia virus family, designated HTLV-III, is closely linked to the disease [AIDS]" (475). Marx obviously didn't notice that Gallo had made that subtle switch in nomenclature, from "leukemia" to "lymphotropic," and his article may have perpetuated lingering confusion about the matter among readers. Marx quotes scientists who have read the Gallo papers as well as the French paper that would be published in July, including Jerome Groopman of New England Deaconess Hospital and Harvard Medical School who says, "I think that Dr. Gallo has identified the cause of AIDS . . . and I am a very cautious, skeptical person who has been involved with AIDS from the start" (475). In describing the Pasteur group's work, Marx calls LAV "HTLV-like" and devotes no detailed discussion to its novelty. Gallo's aggressive approach may have contributed to this misunderstanding of the novelty and importance of LAV, but the French team's rhetorical reserve may be blamed as well.

Conclusion

At the end of *King Lear*, practically everyone is dead—perhaps because of bad rhetoric. At the end of the AIDS Virus Hunt, the fortunes of two key investigators are marred, in part, because they refused to "mend their speech a little."

After the first blood test patent was awarded to the National Institutes of Health, in Gallo's name, the Pasteur Institute launched a patent dispute against the US Government, claiming priority for Montagnier's group. Meanwhile, in early 1985, genetic sequencing of HTLV-III, LAV and another virus isolated by Jay Levy from AIDS patients revealed that HTLV-III and LAV were genetically identical, which prompted

accusations by the French that the reason Gallo's HTLV-III was identical to LAV was because Gallo had simply stolen LAV. A team of legislators and scientists representing the Office of Research Integrity found evidence of contamination in the Gallo lab and no evidence of virus theft. They eventually accused Gallo of making a dishonest statement in one of the papers from his laboratory published in *Science*, a charge that was later dropped. After five years fighting these accusations and the defamation of his character and reputation, Gallo was finally vindicated. This virus war, as it has been called, was costly, and the biggest loser may have been AIDS research. Near the end of the ordeal in 1991, Howard Streicher, one of Gallo's colleagues at the National Cancer Institute, claimed that vaccine and other basic research had been significantly delayed by the imbroglio (in Brown 20).

Could better rhetoric on both sides have prevented this imbroglio?

Perhaps if the French team had been bolder, the importance of LAV would have been acknowledged much earlier. In their lawsuit against the US government over patent rights, the group argued that they had reported in their first paper, Barre-Sinoussi, et al., isolating from an AIDS patient a retrovirus different from HTLV-1 or II and that, therefore, "There is a prima fade case that the Montagnier team was 'first'." (In Norman 642). Yet their elliptical presentation did not emphasize those characteristics of their virus that made it novel and different from known isolates of HTLV, namely, its cell-killing effects. Barre-Sinoussi now acknowledges that they might have

done a little more with the cell-killing effects of our virus in the 1983 paper. We could have been a little more aggressive, but I think we were too shy for good reasons. I know I was. We had been working on animal retroviruses, so when we enter the field of human retroviruses, people don't want to accept you because human retroviruses was Gallo's field (interview).¹⁴

In being "a little shy," the French might have failed to transfer the confidence they had in their data to their audiences. As Anthony Fauci, director of the Institute of Allergies and Infectious Diseases at the National Institutes of Health, explains, when one is confident in one's data, one should be assertive, or what he calls being "appropriately definitive. Not being definitive enough in presenting your claim when you should be leaves the reader totally uncertain about what it is that you think which as bad as being definitive when you don't have the evidence" (interview, National Institute for Allergies and Infectious Diseases, Bethesda, August 4, 1995).¹⁵ In other words, it is just as damaging to the community for scientists to behave like Cordelia as it is for them to behave like Goneril and Regan.

In our conversation, Gallo asserts the importance of aggressive claim-staking, "Look at his [Montagnier's] papers in 1983 and 1984. It doesn't claim that he had the AIDS virus. I'm the first person who said this is the cause of AIDS, I'm positive, and I have the blood test to prove it" (interview). Montagnier's indirection, Gallo argues, masks either lack of evidence, a failure in logic, or humility:

It wasn't humility; he's not a humble person. But he was playing at indirection, you know, not making any conclusions. Well, you can make no conclusions for two reasons. Either you don't have the thought to properly make the conclusion or you don't have the data. I mean if the conclusion is there, you're stupid if you don't make it (interview).

Gallo's culpability in the virus hunt imbroglio is also evident. While his bold rhetoric can be reassuring and compelling, it can also be self-aggrandizing and misleading. The Gallo team's 1984 *Science* papers just discussed are examples of massive scientific

achievement. But they also exemplify rhetoric that can inspire conviction as well as create hostility. While those papers convinced many scientists, journalists, and government bureaucrats that Gallo had found the cause of AIDS, they infuriated the slighted Pasteur Institute team and offended members of the Office of Research Integrity committee investigating Gallo as a result of Montagnier's accusation of foul play. Finding no evidence that Gallo stole the French virus, the committee accused the Gallo team of making a dishonest statement in one of those *Science*, 1984 papers whose lead author was Popovic. A landmark in twentieth century virology, this paper describes the cell system developed by Popovic for growing HIV, a system that made genetic sequencing, as well as other important investigations of the virus, possible. However, the committee's scientific members were struck by, according to Klotman, "the attitude in this paper, the infusion of ego and the claiming of proprietary positions. They were rubbed the wrong way, appropriately so" (interview). However, rather than attack the improper rhetoric, "they combed that paper to find misconduct because, you know, they don't know how to talk about rhetoric. And they came up with a really ridiculous accusation-that it was dishonest" (interview). The committee focused on this statement, which I have previously discussed, near the end of Popovic, et al.

These findings suggest that HTLV-III and LAV may be different. However, it is possible that this is due to *insufficient characterization of LAV* because the virus has not yet been transmitted to a permanently growing cell line for true isolation and therefore has been difficult to obtain in quantity.

As a direct hit, an outright criticism, this statement violates the norms of politeness in scientific discourse as Greg Myers has outlined them (1992). With the French blood test patent pending and a priority dispute brewing, this statement focuses on an ostensible weakness in the French team's work that could undermine their blood test as well as their discoverer status.¹⁶ Other scientists understood the rhetorical move but considered it unfair. Lyle Bivens, director of the Office of Research Integrity under whose auspices the Gallo investigation was conducted, saw the statement as "intellectual appropriation" that downplayed the French work (in Anderson, 22). Gallo explains that the sentence means simply this: the viruses may appear different, but since the French could not grow their virus as completely as we did, they couldn't describe it properly. However, the committee interpreted the statement as saying that LAV had never been grown on a cell line and after searching through Gallo's notebooks, found that the Gallo team had, indeed, grown LAV in their laboratory; therefore, the committee deemed the statement that LAV had not been grown untrue. In other words, they saw outright dishonesty where Gallo had intended rhetoric.

Gallo defends the statement as a legitimate rhetorical move in establishing the strength of his position:

That statement doesn't say *we* never cultured the virus; it says *they* never mass produced it so that they couldn't have characterized it. The point I was making was that *they* hadn't grown LAV successfully enough to mass produce it and characterize it, make a blood test from it. That's what we did first, and I felt it was important to say that (interview).

Gallo doesn't see anything unethical about the move: "That statement was my aggressive style.¹⁷ I knew we were right, and I knew they hadn't done what we had done. Maybe it was impolite-and I've learned some lessons about being too offensive-but it wasn't dishonest" (interview). The question of ethics in this case centers on what Gallo believed

to have occurred, or not occurred, in the French laboratory, and we will never know for certain what he actually knew or believed. But the move to slight rivals by pointing out what they either haven't done in the laboratory or haven't sufficiently persuaded their audience that they have done, albeit impolite and even self-aggrandizing, is defensible in this situational context only *if* Gallo was correct about the French team's evidence. There is no time for politeness when the blood test for an infectious virus might be developed from the antigens of an under-characterized virus isolate. Barre-Sinoussi now contends that "we did characterize our virus. We'd been doing the work and felt confident we had a good blood test" (interview). But if their audience lacked confidence in their work because of absences in their papers, then they left themselves vulnerable to a rhetorical opportunist such as Gallo who could fully exploit those absences whether they resulted from inadequate rhetoric or flawed laboratory work.

But Gallo's boldness eventually made him vulnerable to attack. A lawsuit filed by a foreign government and public outcry against the dirty laundry of scientific institutions and their competitive scientists created an opportunity for the attack on Gallo that led to charges of dishonesty, charges that overlooked the rhetorical nuances of the situation he addressed. While perhaps one of the most imaginative and resourceful scientists in the world, Gallo was vilified as a result of the very qualities that compelled him to search for and find a cancer virus in the seventies: a bold, aggressive, tenacious spirit that moves him to become caught up in one massive idea as well as one massive rhetoric. That massive rhetoric, because it violates norms of politeness and reserve, because it resonates with what might be called fervor, marks its owner as either master or megalomaniac. Those who view Gallo as master argue that his reputation for masterful science earned him the privilege of employing that rhetoric. As Anthony Fauci, director of the National Institute of Allergies and Infectious Diseases claims, "Bob writes definitively. He's confident in his data, and he writes his papers with the confidence that he has in his data. It's a less conservative style, but I have no problem with it" (interview). Mary Klotman, a member of Gallo's laboratory during the eighties, believes Gallo is granted some rhetorical latitude because he always has the evidence:

He's given a lot more leeway when he steps over them [rhetorical limits]. Those limits are determined at what level you are at, how senior you are, how much of your work backs up your rhetoric, so some people are complete buffoons when they carry on like that. Bob can get away with a lot more because he's got a high track record {interview}.

But there is a fine line between boldness that compels and boldness that offends, between master and megalomaniac. And Gallo crossed that line in those *Science* papers proclaiming that his team had found the cause of AIDS. As he now admits, he can no longer get away with aggressive rhetoric: "I've been forced to tone down my approach. I've learned to be more polite" (interview).

The French team's reticent rhetoric did not serve them well at first because it underplayed what was most novel and interesting about their work. While offering prudent reports of their investigations and findings, they did not recognize a rhetorical battle in the wind. They were playing the part of the conservative, Cordelia-like scientists who conscientiously offer evidence without attempting to overtly influence attitudes and without understanding the legitimacy of such influence during matters of great import. A conditioned expectation that scientific presentation must be "matter of fact" contributes to the reluctance among many scientists to understand the nuances, the blessings and

banes, of specific rhetorical styles in specific contexts. At the end of *King Lear*, the Duke of Albany sums up a very important lesson that I think applies to our case: "The weight of this sad time we must obey;/ Speak what we feel, not what we ought to say" (V, iii, 332-324). The French felt they had provocative evidence of a novel virus, but, following the "oughts" of matter-of-fact scientific discourse, they didn't emphasize this evidence. As a result, audiences may have missed the significance of their data.

There is a fine line between rhetorical aggression that offends and rhetorical passivity that hides. During a controversy, such as the AIDS virus hunt, a rhetor who crosses that line in either direction may create misunderstanding. Gallo's boldness, while helping lead the community toward a viral etiologic theory, misled the community about the exact identity of the AIDS virus and slighted Montagnier's contribution. Montagnier's indirection, while reasonable, did not accommodate the particular exigencies of the rhetorical situation surrounding the AIDS epidemic—the need for directness, for assurance, for bold ideas, for possibilities.

Notes

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¹Philip Kitcher frames his essay ("Persuasion") with Cordelia's dilemma. Stephen Jay Gould, in his essay ("Cordelia's Dilemma") also employs the opening of *King Lear* to compare Cordelia's silence to the negative result in science. I frame this essay with the same literary allusion because of the parallels between the attitudes toward language held by characters in the play and those held by the key figures in the AIDS virus hunt.

²All of the interview quotes from Gallo have been taken from my interview with him at the National Cancer Institute on August 3, 1995.

³When my own first cousin returned home, very ill, after being diagnosed with AIDS, his family was very concerned about whether they would get infected by kissing or touching him. Because he and his friends had read every scientific paper on AIDS that had been published to that point (1986), he was able to reassure his own family that they were not at risk.

⁴However, as one reviewer of this essay pointed out, Montagnier is more a victim of Cartesianism than a practitioner, for Descartes was highly astute rhetorically, sufficiently artful to create the appearance of demonstration.

⁵All interview quotes from Barre-Sinoussi have been taken from this interview.

⁶The issue contains one paper from the Pasteur Institute, (Barre-Sinoussi, et al., 1983) two from the Gallo laboratory (Germann, et al. and Gallo, et al.) and one paper from Harvard (Essex, et al.). Publication of these papers in *Science* involved a hurried process, with other papers already previously accepted for publication being bumped for later publication dates to make room for these papers. Under normal circumstances, according to Kulstad (see "Publishing AIDS Papers"), Gallo (interview), and Barre-Sinoussi (interview), none of these papers would have been published as quickly as they were. As Kulstad remarks, "Individually, the HTLV and LAV papers were weak; the reviewers knew it, the authors knew it, and so did the editors. But side by side, . . . they supported the view that studies of a possible retroviral etiology were worth pursuing" (14).

⁷Gallo now explains that Essex's data was invalid because he was picking up an antigen that both HIV and HTLV had in common, not HTLV itself because the AIDS patients were doubly infected with HTLV and HIV.

⁸All italicized statements from the texts under analysis represents my emphasis.

⁹Gallo explains that "some leukemia viruses can cause leukemia or they can cause aplasia, lack of proliferation of red blood cells, aplastic anemia" (interview, National Cancer Institute, August, 1995). When I asked if they were actually cytolytic, actually killing cells, he conceded that "the mechanism is usually not cytolytic" (interview, National Cancer Institute, August, 1995) which means that there was no evidence that HTLV isolates actually killed cells which means that the French team's laboratory observations were indeed significant indications of a new virus.

¹⁰All interview quotes from Dr. Klotman are taken from this interview.

¹¹All interview quotes from Dr. Levy are taken from this interview.

¹²The debate between Gallo and Montagnier centered on the difference between membrane or envelope proteins and structural proteins. In the relatively young field of virology, there were disagreements about what qualities two

viruses had to share in order to belong to the same family. In the AIDS virus debate, the Gallo camp believed that antibody reactions to envelope proteins or antigens was enough to establish family relationship while the French team argued, in 1984, that because their virus didn't share HTLV core or structural proteins, it wasn't an HTLV. While in 1983, the Gallo laboratory reported isolation of an HTLV-related virus containing HTLV core proteins and expressing HTLV antigens, in 1984, however, they announce isolation of a retrovirus that did not contain the core HTLV proteins, p19 and p24, but argue, nonetheless, that this virus belongs to the HTLV family because antibodies to cell membrane-envelope-antigens of HTLV were found in AIDS patients' blood. Agreement with Gallo's claim necessitated the belief that antibody reactivity to envelope proteins of the cell, rather than to the core proteins, indicates family relationship. In 1983, the French team at first seemed to agree with this last principle since they classified their virus as a likely HTLV family member because of antigen reactivity even though it did not share HTLV core proteins (Barre-Sinoussi, et al 1983). But in 1984, core proteins become one deciding factor in the French team's claim that their virus was not an HTLV.

¹³Both Jay Levy and Barre-Sinoussi claim that the emphasis on HTLV created misunderstandings among researchers and clinicians. While Gallo carefully distinguished his HTLV-111 from the oncogenic viruses in the HTLV family, Levy and Barre-Sinoussi believe many people were confused. As Levy said, "That was very misleading to many people, to research. If you have it in your mind it's an HTLV, you have a different approach than if you think of this as a brand new virus" (interview). Barre-Sinoussi claims that "many clinicians were confused by the HTLV label, and many assumed, falsely that the AIDS virus was a leukemia virus" (interview).

¹⁴One of her most valuable lessons from the virus hunt, Barre-Sinoussi says, was learning "how to write papers for Americans. Now I am much better at writing papers for Americans" (interview). Evidence of the French team's more aggressive style can be found in a 1991 paper (Wain-Hobson, et al.) that clarifies the origins of the HIV isolates from the Pasteur. In this paper, they argue that the reason the Gallo laboratory was able to grow the AIDS virus back in 1984 was because the Gallo lab was working with a particularly aggressive strain of HIV that had mutated from the French virus and contaminated Gallo's cultures. Meanwhile the weak strain of LAV remained at the Pasteur where it has never thrived on cell lines. This paper includes rhetorical moves similar to those employed by the Gallo team. Acknowledging the more aggressive style in this paper, Barre-Sinoussi said that "one reason it's more aggressive is that the lead writer is English and living in France. He taught us how to be more aggressive" (interview).

¹⁵All interview quotes from Dr. Fauci have been taken from this interview.

¹⁶According to Grmek, the fact that the French were not at first successful in growing their virus in a transformed permanent T-lymphocyte culture was due to the nature of the first isolate of LAV which simply will not grow easily in cell lines. However, the French sent cloned strains of their virus to other laboratories. These strains changed in vitro, making them more adaptable to cultures (73).

¹⁷The French team's difficulty in maintaining a permanent cell line infected with the virus was mentioned by Gallo in several papers (see Reeves, 1992) at a time when the community was deliberating about who first isolated the AIDS virus.

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