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**Owning a Virus:
The Rhetoric of Scientific Discovery Accounts**

Carol Reeves

On April 23, 1984, Margaret Heckler, at that time the secretary of Health and Human services, announced at a press conference that "the probable cause of AIDS has been found." She went on to explain that the cause was a new virus and its discoverer was "our eminent Dr. Robert Gallo," chief of the National Cancer Institute (Crewdson 2). Gallo was known for his discovery of a family of human retroviruses - Human T-cell Leukemia Virus (HTLV)-that caused a rare form of cancer and had received the coveted Lasker prize, the top honor in American biomedicine which often precedes the Nobel Prize. In several papers published in 1984, Gallo claimed to have found antibodies to what he believed was a new member of the Leukemia virus family-HTLV-III-in the blood serum of AIDS patients and that he had several isolates of the virus growing in his lab. He had also published electron micrographs of virus particles. On the day of Heckler's announcement, the American biomedical community was ready to accept Gallo as the first to solve the mystery of AIDS etiology, was ready to view him as "one of the paradigmatic figures of the 20th Century" as Samuel Broder described him (in Crewdson).

However, the Gallo lab was not alone in searching for the cause of AIDS and developing a blood test. At the time of Heckler's announcement, the American patent office had two applications for a patent to the new AIDS blood test, the ELISSA (Enzyme-Linked Immuno-Absorbent Assay), one from the American Government and one from the Pasteur Institute in Paris. Gallo's chief rivals in the search for the cause of AIDS, a team headed by Luc Montagnier of the Pasteur Institute, had also applied for a patent on their ELISSA. They had argued at various conferences and in publications in 1983 and 1984 that they had found no antibodies to Gallo's leukemia virus in the blood of their AIDS patients, that the virus they had identified, Lymphadenopathy-Associated Virus, was unrelated to HTLV and that they had discovered it many months before Gallo detected any retrovirus activity in blood samples from AIDS patients. Moreover, they were convinced that the American blood test had been made from the virus isolate the French had sent Gallo in the spirit of scientific collaboration. By 1985 a storm was brewing over who had first isolated the AIDS virus and who deserved the patent rights to the AIDS blood test. In May 1985 the US patent office issued a patent to the US

government on the ELISSA made with Gallo's AIDS virus. From May to December 1985, representatives from the Pasteur Institute confronted senior officials at the Department of Health and Human Services (HHS) with evidence that they, not Gallo, had first isolated the virus and developed the ELISSA and that the American patent was invalid. The HHS rejected their claim. On December 12, 1985, the French filed suit against the US government, challenging the patent.²

From 1985 to 1986, while the controversy surrounding the question of who first discovered the AIDS virus and developed the ELISSA heated up, Gallo published a flurry of review articles about research linking retroviruses to AIDS. Three of these five papers (*Cancer*, *Cancer Research*, and *Scientific American*) were written by Robert C. Gallo alone; two of them (*The New England Journal of Medicine* and *Annals of Internal Medicine*) were written by Gallo and a cowriter from his laboratory. In these accounts, Gallo and his cowriters use the review or discovery account genre to promote Gallo's role in AIDS research and his virus-

HTLV-11 as the cause of AIDS. These five discovery accounts represent an intriguing opportunity to examine how one scientist and two members of his laboratory team demonstrate and attempt to maintain rhetorical authority during a period that represents a significant shift in knowledge about a devastating human problem.

The necessity for rhetorical discourse during a shift in knowledge has been noted by several theorists. Sociologist Robert Merton notes that there is a rhetorical dimension involved when collectives compete to be the first to produce a public interpretation of a phenomenon and naturally want their interpretations to prevail over other interpretations (110-11). Likewise, Thomas Kuhn identifies rhetorical persuasion as essential to scientific revolutions: "To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists" (Structure 94). It is impossible to determine here whether the discovery of retroviruses or the cause of AIDS had anything like the revolutionary impact upon whole scientific paradigms as did the identification of the double helix model of DNA. Nevertheless, the fact that these papers are directed at several different communities during a time when there was considerable competition for what Joseph Gusfield calls "ownership" of a public problem necessitates a close examination of the strategies Gallo and his cowriters use to maintain that ownership.

Gusfield explains that ownership "indicates the power to define and describe the problem" (13). People or institutions who "own" a public problem "can make claims and assertions. They are looked at and reported to by others anxious for definitions and solutions to the problem" (10). These papers demonstrate Gallo's determination to maintain the privileges of ownership—to be viewed as the first to discover the cause of AIDS and to have the authority to name, define, and describe the phenomenon and control its public interpretation. These accounts

serve not only to disseminate knowledge but also to defend Gallo's primary role in generating that knowledge.

Gallo claims "ownership" in AIDS research by crediting the divergent course his research has taken and his team's contributions to what is now routine laboratory practice and by discrediting the early publications of the French team. Another way that Gallo gains rhetorical authority in these papers is to construct contexts that accommodate his intended audience's interests and beliefs. Gallo's readers range from biomedical specialists who read *Cancer*, *Cancer Research*, and *Annals of Internal Medicine* to medical practitioners who read *The New England Journal of Medicine* to a wider, perhaps less specialized, audience for the more popular publication, *Scientific American*. While there are obvious differences in the style and format of these papers due to editorial policy and convention, there are some contrasting features that I believe go beyond the necessities of convention and serve as flexible argumentative resources in Gallo's attempt to gain rhetorical ownership in AIDS research. Gallo demonstrates an acute understanding that different audiences have different epistemological assumptions and therefore need different conceptual constructions of the scientific enterprise as well as the phenomena under investigation.

Steve Woolgar describes discovery accounts as reviews or "popular articles" that "present summary accounts of research" on a particular phenomenon (396). Woolgar finds a discrepancy between formal, published accounts and informal accounts gleaned from conversations and interviews. Published accounts "tended to give the impression of a relatively straightforward progression through a series of logical steps . . . leading to the discovery" (415) whereas personal interviews revealed that the discovery process is often marked by uncertainty, error, confusion, and surprise. In formal accounts, moreover, the debatable issues—such as the date of discovery, for example—are assumed to be closed and consensus achieved. Woolgar's findings imply that the valedictory occasion for the formal accounts may require writers to give the impression that their discovery process was unproblematic and that the issues have been black-boxed. I am concerned here neither with comparing a scientist's formal and informal accounts nor with discovering whether Gallo's accounts are scientifically or historically accurate. My program is simply to identify the ways that Gallo uses the valedictory occasion as an active resource in an open battle for ownership in AIDS research.

Crediting the Gallo Lab

One way that Gallo establishes his "ownership" in AIDS research is to credit himself and his lab team and to discredit the French team. In two papers for which he was the sole writer, (*Cancer* and *Cancer Research*), Gallo emphasizes the role his lab played in retrovirus and AIDS research in the introductions. In the *Scientific American*, *Annals*, and *New England Journal of Medicine*, discussion of the primary role of the Gallo team appears in subsequent body paragraphs. Given the

nature of the introductions to *Scientific American* and *The New England Journal of Medicine*, which emphasize the disease itself rather than the Gallo team's role in investigating the problem, it could be argued that staking territory in AIDS research for the benefit of his specialist peers in biomedicine is a high rhetorical priority for Gallo. Highlighting his lab's investigative role in AIDS research in the introductions to papers for specialists could be his way of tacitly reminding them of his ownership.

In the introductions to the *Cancer* and *Cancer Research* papers, Gallo discusses how the work done in his lab was the result of a readiness to travel outside mainstream cancer research and take an approach that differed from routine methodology. In *Cancer*, after he briefly summarizes the mainstream work on cellular one genes, he writes:

However, these studies do not generally deal with primary causes. My co-workers and I are also interested in the molecular mechanisms involved in the pathogenesis of neoplasms, but our approach has been different. (2317)

He then explains that his group has "focused on particular types of cells and particular forms of growth abnormalities" and that they "are also interested in the primary causes of these diseases" (2317). Also, he insists that he and his coworkers used the discoveries of others in ways that mainstream investigation did not. In both *Cancer* and *Cancer Research*, he describes how his lab creatively utilized the discovery of reverse transcriptase in ways the rest of the community did not. In *Cancer*, he writes that

(w)hat stimulated us to work, rather than just to think about these questions, was the discovery of reverse transcriptase in retroviruses, which opened up new areas to molecular biology and paved the way to the understanding of the life cycle of these viruses. To us it also offered something more."(2317)

In *Cancer Research*, he writes that "reverse transcriptase paved the way for the beginning of biochemical understanding of retrovirus replication . . . ; for me it was also a powerful and sensitive new tool for detecting low levels of retroviruses in human leukemias" (4524). Gallo knows the scientific community values the kind of flexible, divergent thinking that leads to the solution of major technical problems. Kuhn argues that the sciences "do demand just that flexibility and open-mindedness that characterize, or indeed define, the divergent thinker. . . . Unless many scientists possessed them to a marked degree, there would be no scientific revolutions and very little scientific advance" (Kuhn 227). Whether or not Gallo's work was flexible and divergent is a question for sociologists. I am

concerned here with how Gallo's introductions go further than the perfunctory assertion that the work about to be described represents a "significant advance" in knowledge. Gallo is doing more than building a context for the information he is about to provide. He is reminding his audience that his work represents a creative and divergent approach.

Gallo also reminds his readers of the skepticism and dogma that his team opposed. In *Cancer Research* he notes in the introduction that

[o]ur idea that retroviruses might be present in small amounts in some human leukemias received considerable resistance. . . . Our premise was therefore contrary to the experience and dogma current at that time, and our attempts were considered futile. (4524)

Likewise, in the introduction to the *Cancer* paper, he writes that "when we set out to do these experiments, there were very strong doubts expressed by many investigators about the existence of any human retroviruses." Because these viruses had not been detected by electron microscopy, "it was assumed that no human retroviruses existed" (2317).

In the midsection of the *Scientific American* paper, Gallo asserts that "the prologue to the discovery of the first human retrovirus is a history of skepticism" (88), and later, he places himself in the position of countering common wisdom:

Under the influence of Termin's ideas I decided to search for reverse transcriptase in human leukemic cells, hoping to find a retrovirus there. In so doing, I was gainsaying acceptable wisdom. (91)

The premise evoked in these examples is that it is often necessary to consider the impossible, to go outside mainstream assumptions and dogma in order to conduct research having an impact on the growth of scientific knowledge. He is implying that without his willingness to consider the possibility of a human retrovirus science would be without the benefits resulting from such efforts, namely the information about the relationship among human retroviruses, T-cell leukemia, and acquired immunodeficiency. Though it is uncertain whether retrovirology had a revolutionary impact on whole paradigms within several scientific communities, it is clear that Gallo wants his audience to think of his work as having a singularly important impact.

While he refers to other laboratory teams doing research on retroviruses and AIDS, Gallo insists that by being the first to ask that all important initial question about the role of retroviruses in AIDS and by developing technology with which to isolate the virus, his lab actually reached "the top" first. Though he gives credit to the collaborative efforts of labs across the country, his references to his chief competitors, the Pasteur Institute team, who first published findings of isolation of

retroviruses from AIDS patients, traverses a fine line between credit and discredit: He concedes that they were the first to publish findings of retrovirus activity in AIDS patients, but dismisses those findings as "inconclusive." In so doing, Gallo discredits the work of the French team and their claim that the AIDS virus is a newly discovered virus unrelated to Gallo's HTLV.

According to Gallo, the obstacles involved in growing and propagating the virus prevented the French team from definitively characterizing the virus, and, in effect, from actually being "the first," to make the discovery. In the *Annals* paper, just after they establish that the problem of losing virus isolates "initially prevented characterization of the virus, development of specific reagents and clear-cut linkage of the virus to the cause of AIDS," Gallo and Wang-Staal write, "However, in 1983, the group at the Pasteur Institute headed by Luc Montagnier reported their first description of the virus associated with AIDS. . . . These investigators also had difficulty in getting adequate amounts of this virus to grow; they could not keep the primary cells in culture indefinitely. . . . Therefore, the virus was not characterized, nor clearly linked to the cause of AIDS in 1983, and specific reagents for the virus could not be developed" (683).

In *Cancer Research*, he explains that the French team could "not report evidence indicating that any two isolates [of the virus] were the same" but published their findings anyway because they were "sufficiently convinced of its interest to publish the first identification of the virus" (4528). However, as Gallo points out, his team had also isolated the virus, but rather than publish prematurely, they decided to wait because they "did not feel these results were sufficiently clear to publish at that time" (4528). In explaining that his team did not publish their findings until they had found a way to grow the virus and thus definitively characterize it, Gallo obviously wants to demonstrate that although the other team might have reached the top first, they took a short cut. Gallo implies that the other team lacked the patience and tenacity that contributed to the conclusive work done in his lab.

In addition to reminding his audience that the French team's first published data were inconclusive, Gallo also reminds his readers that he and his colleagues first proposed the notion that AIDS was caused by a retrovirus and thereby initiated investigation of that hypothesis. In the *Cancer Research* paper, he states, "I proposed that AIDS was likely to be caused by a human T-lymphotropic retrovirus in February, 1982 at a Cold Spring Harbor Conference on AIDS" (4527). Likewise, in *Cancer*, he writes that "[w]e reasoned that AIDS was a viral disease and proposed that it was due to a human T-lymphotropic retrovirus in February 1982 at a Cold Springs Harbor Meeting on AIDS" (2321). He and Wong-Staal remind their readers in the *Annals* paper, that "the genes of HTLV-III were first molecularly cloned in our laboratory" (684). Like the explorers who mark their territory with a flag and want the world to acknowledge their symbolic ownership of a place,

Gallo wants the scientific community to acknowledge his ownership of the territory of AIDS research.

Gallo also promotes his work as resulting in the kind of new knowledge and technology which initiates major shifts in methodology. In all his accounts, Gallo describes the technology and methods developed in his lab that enabled investigators to first identify human retroviruses and then to isolate the retrovirus causing AIDS. He also reminds readers that these methods have become indispensable to routine practice. In the introduction to the *Cancer* paper, for example, he points out that his discovery of T-cell growth factor or interleukin-2 and the techniques he and his lab team developed to use interleukin-2 to stimulate T-cell growth *in vitro* "became the approach to isolate human retroviruses." Moreover, he reminds his readers that "selection of the right patient, use of sensitive assays for virus, and proper growth of the cells *in vitro* has made isolation of these viruses now a routine" (2318). It is important to keep in mind that there is no clinical necessity for descriptions of these methods since they have become so much a part of routine; however, rhetorical necessity requires that Gallo remind his audience of what team is responsible for developing these methods.

Gallo's emphasis on the role his work played in the discovery of the cause of AIDS likely undergirded his claim that his virus, HTLV-III, was the cause of AIDS and helped to market him as an "owner" in AIDS research. In all the papers, the claim that HTLV-III causes AIDS and that it is the same virus isolated by other researchers seems unproblematic, black-boxed. In the *New England Journal of Medicine*, for example, while he concedes that "there is a noteworthy diversity in the . . . patterns seen among HTLV-III isolates from different patients," he still claims that the French LAV is not another virus but one of the forms of HTLV-III: ". . . we believe that HTLV-III really represents a set of closely related but varying genetic forms, and that lymphadenopathy-associated virus [LAV] is one of these forms" (1295).

In the *Annals* paper, Gallo and Wong-Staal enumerate the evidence pointing to HTLV-III as "clearly" the cause of AIDS:

There are now more than 100 isolates of HTLV-III in our laboratory. The virus was isolated from all risk groups: homosexuals, drug abusers, blood product recipients, mothers and fathers of infected children, promiscuous heterosexual men, prostitutes, Haitians, and the wives of men with AIDS. We have never been able to detect the virus in a normal donor. *These findings alone clearly link the virus to the cause of AIDS.* (684, emphasis mine)

In their classification of statement types, Latour and Woolgar define a claim as a statement which contains "modalities which draw attention to the generality of available evidence (or lack of it)" (79). The modality "clearly" draws attention

away from what could be problematic in Gallo's claim. Other labs had reported isolations of retroviruses from AIDS patients, but no one had confirmed that these isolates represented the same virus or that they were all isolates of Gallo's retrovirus. In fact, a research report written by Luc Montagnier follows the Gallo and Wong-Staal paper in *Annals*. Montagnier claims that the primary causative agent of the acquired immunodeficiency syndrome "is a type of retrovirus that has not been previously recognized" (689). At least as far as the French were concerned, consensus had not reached the point that a definitive statement about the link between HTLV and AIDS was even possible. Yet Gallo's ostensibly unproblematic statement is made after he has narrated the history of research on retroviruses and AIDS and made himself and his lab team chief protagonists in that story. By establishing the primary role of his lab in AIDS research, Gallo lends credibility to his claim that *his* virus is *the* virus causing AIDS.

Building Contexts

Another way that Gallo gains rhetorical authority in these papers is to construct contexts that accommodate his intended audience's interests and beliefs. In his introductions and conclusions, he focuses on issues indicative of his intended audience's assumptions about such matters as AIDS, science, and knowledge.

The main contextual difference between Gallo's introductions in *Cancer Research* and *Cancer* and his introductions in *The New England Journal of Medicine* and *Scientific American* centers around the issue of *agency*. For specialists, Gallo emphasizes his laboratory team's agency in AIDS research, an emphasis that accommodates the communal ritual of identifying the discoverer and the belief that such an endeavor is even possible given the collaboration that occurs in science. For readers of *The New England Journal of Medicine* and *Scientific American*, however, Gallo emphasizes the disease itself as an agent in causing human suffering and public catastrophe and then counters the grim outlook with the consolation of new knowledge.

In the NEJM paper, Gallo and Broder demonstrate a clear intent to communicate with those who will actually treat AIDS patients in their office or emergency rooms. Throughout the paper the authors counter the bad news—the devastating and frustrating course of the new disease—with the good news—the growing knowledge about AIDS and the medical benefits that might be reaped from this knowledge. In the introduction they claim that "one of the major frustrations for practicing physicians and the public alike has been the inability of the scientific community to define the origin of this disease" (1292). They liken this frustration to the sentiments expressed by John Donne: "Perhaps John Donne, in 1624, in reference to his own life-threatening illness, was able to capture some of the frustrating emotions felt by patients with AIDS, persons thought to be at risk of AIDS, and their physicians" (1292). They then quote Donne:

'I observe the Phisician, with the same diligence, as hee the disease; I see hee feares, and I feare with him, I overrun him in his feare, and I go the faster, because he makes his path slow; I feare the more, because he disguises his fear, and I see it with more sharpnesse, because hee would not have me see it.' (1292)

The reference to the frustrations felt by patients and doctors alike and the Donne quote embody the text with a highly personal tone. The writers establish an intimate relationship with their readers as members of the practitioner community—a membership of individuals who have witnessed the AIDS epidemic first hand. Gallo and Broder want their readers to know they understand the plight of the practitioner facing patients who have a debilitating and overwhelming disease. But they also want to console their readers with the hope of new knowledge about the cause of the disease, as they say in their introduction:

But recently, several converging lines of research have linked a human T-cell lymphotropic retrovirus (HTLV-III) to the pathogenesis of AIDS, and this knowledge has brought us closer to understanding the disease. (1292)

In the introduction to the paper in *Scientific American*, Gallo employs metaphors that Susan Sontag has identified in the language of AIDS, the metaphors of plague and apocalypse. Gallo's first sentence contains the plague metaphor: "It is a modern plague: the first great pandemic of the second half of the 20th Century" (47). Sontag says that plague "has long been used metaphorically as the highest standard of collective calamity, evil, scourge" (89). Gallo goes on to add that "by now as many as two million people in the U.S. may be infected" and that "in some areas it may be too late to prevent a disturbingly high number of people from dying" (47). The "bad news" outlined in the first paragraph is followed by the "good news" outlined in the second: "In sharp contrast to the bleak epidemiological picture of AIDS, the accumulation of knowledge about its cause has been remarkably quick. Only three years after the disease was described its cause was conclusively shown to be the third human retrovirus" (47). In this introduction Gallo accommodates his readers by evoking the metaphors they likely associate with AIDS and by reassuring them that though infection has reached catastrophic proportions, the disease is no longer shrouded in mystery. Gallo allows himself some dramatic touches that would not be appropriate for more specialized audiences.

In the conclusions to the papers in *Annals*, *CanCfI*, *Cancer Research*, and *The New England Journal of Medicine*, Gallo appeals to the premise that scientific explanation of puzzling phenomena is always a desirable end in itself. Though, as Gallo points out, the retrovirus may pose serious psychological and physical

problems for its victims, it is a "remarkable" biological entity for scientists to study. Moreover, he explains that not only has research identified the human retrovirus causing AIDS but also promises to curtail its destructive capacity. The consolation of new knowledge *about* AIDS is placed against the morbidity and mortality caused *by* AIDS.

He concludes the *Cancer* and *Cancer Research* papers by reminding his readers of the optimistic results of the contributions he outlined in the introductions and throughout the papers. In concluding the *Cancer* paper, he avoids mentioning the clinical difficulties involved in disease management and vaccine development for a virus that has a great capacity to mutate and states that "preventive measures to control the spread of AIDS with the help of chemotherapeutic agents or a vaccine are currently being actively pursued" (2321). He ends the paper by reminding his readers of the rapid progress in research:

Thus, in the past 5 years, the first human retroviruses have been discovered and two types have been linked to the cause of two different human diseases. (2321)

He also reminds readers of the interesting knowledge that has been discovered:

Remarkably, one disease involves an over-proliferation of the T4 lymphocyte, while the other involves premature death of the same cell. (2321)

Finally, he alludes to the technological innovations developed in his laboratory and their effects on basic research:

The capacity to mimic the disease *in vitro* by infecting target normal T4 cells provides the most powerful system available for the study of altered growth of a human cell. (2321)

In concluding the *Cancer Research* paper, Gallo says:

The past 5 years have witnessed the beginning of a new era in retrovirology, the era of human retroviruses. Within this short period the causes of two human fatal diseases have been worked out. In both instances *in vitro* systems have developed which mimic the diseases so that a reasonable amount of information on the involved molecular mechanisms has become available. The future promises much more such information, but what is much less certain is how soon we will learn to correct and/or prevent these diseases. (4530)

Though this statement is not wholly optimistic, it is important to note that Gallo does not ask *whether* we will learn to prevent these diseases but *how soon*.

In *The New England Journal of Medicine*, after detailing the new knowledge about retroviruses and about the history of their discovery, the writers conclude the paper by enumerating what they believe is optimistic about their findings. Knowledge of the virus "may affect many phases of basic research with clinical implications" (1295): "a stimulus to re-examine endemic forms of cancer that pose special public-health problems for some nations in the Third World" and the possibility of developing "a vaccine for persons who belong to certain risk groups and thereby prevent the disease" (1296). Whereas he emphasizes the implications for future research at the end of the specialized research journals, here Gallo and Broder emphasize the implications for treatment of serious medical problems caused by the virus.

In the *Scientific American*, he concludes by painting a very different picture of the scientific community. Gallo warns his readers of the "hubris" of scientists in thinking that infectious disease has been conquered. In the final paragraph, Gallo discusses the "moral" of "this terrible tale." He says that the discovery of retroviruses and their "capacity to cause extraordinarily complex and devastating disease has exposed the claim [that science has conquered infectious disease] for what it was: hubris. Nature is never truly conquered" (65). It is interesting that he ventures a moral attack on the "hubris" of modern science in this paper while in the other papers he applauds the capacity of basic science to explain puzzling phenomena and develop new technology.

In light of the recent negative publicity surrounding Gallo, it would seem *prima facie* that Gallo was using base rhetoric, was playing to the gallery. However, a more benign interpretation is that Gallo was using two contrasting conceptions of science as flexible tools for arguing in different contexts. Karl Popper suggests that the primary aim of science is "to find *satisfactory explanations* of whatever strikes us as being in need of explanation" and that ". . . scientific explanation, whenever it is a discovery, will be the explanation of the known by the unknown" (191). For scientific culture, *explanation* of puzzling phenomena, rather than *control* of phenomena, is a desirable end in itself. In popular culture, the Frankenstein or mad scientist myth, as well as such highly publicized cases as the development of the atom bomb and human genetic engineering, combine to build a popular image of science as aiming to control nature with often disastrous consequences. The discrepancy between Gallo's celebration of scientific explanation in *Cancer* and *Cancer Research* and his admonishment of scientific hubris in *Scientific American* would seem hypocritical to a positivist but merely ironic to anyone who understands the relationship between rhetoric, culture, and reality.

Will the Real Virus Please Stand Up?

Description of the phenomenon itself offers another flexible argumentative resource for Gallo. Rhetorical ownership implies the authority to name, define, and describe the phenomenon and control its public interpretation. Gallo's descriptions of the AIDS virus itself indicate his sensitivity to the needs and expectations of his specialized and lay audiences as well as his understanding that his descriptions influence their perceptions of the virus and his role in its discovery. In these descriptions two metaphorical conceptions of the virus construct two very different public interpretations of the disease.

Jeanne Fahnestock has traced the "rhetorical life" of scientific facts (1986) as they move from specialized to lay publications. Fahnestock illuminates the ways journalists inscribe scientific findings in language that is more certain and authoritative than the language in scientific research reports and how the translation from scientific to lay communication belies the different epistemological assumptions and rhetorical practices of both communities. But what happens when *one* writer or one team of writers disseminates new knowledge and the story behind its discovery to different audiences, to specialized and unspecialized readers?

The star of the show, the AIDS virus itself, offers an interesting view of the way one writer or a team of writers may very subtly "change" the phenomenon at hand to accommodate different audiences. In both the *Annals* and the *Scientific American* papers, the virus and its cytopathic effects are described in detail, but both descriptions offer different conceptions of the virus. In the *Annals* paper, the human retrovirus causing AIDS is a "remarkable" natural phenomenon with "unique" biological capacities. In the *Scientific American*, the virus becomes an "invader," a "culprit" that violently attacks its host. Although Gallo cowrote the *Annals* paper with Wong-Staal and although it is impossible to tell which writer composed the descriptions of the virus, the difference in the language used to describe retrovirus activity in these two papers indicates the different needs and attitudes of the two audiences. The biomedical specialist is interested in the virus as a clinical entity and needs to know how the virus differs from other viruses. The *Scientific American* readers likely knew the virus not as a clinical entity but as a killer; Gallo's descriptions of the virus accommodate their expectations and allow them to visualize and imagine intercellular activities.

For readers of *Annals*, Gallo and Wong-Staal choose terms which depict the virus as "agent" and the host cell as "scene" but do not necessarily invoke visual images of aggression:

This enzyme [reverse transcriptase] *converts* viral RNA into double stranded DNA. For virtually all retroviruses, the DNA form *moves* from the cytoplasm to the nucleus where it *integrates* into the host cell DNA. (679, emphasis mine)

In contrast, the description of the same activity in *Scientific American* uses stronger verbs connoting aggressive agency:

When the virus enters its host cell, a viral enzyme called reverse transcriptase *exploits* the viral RNA as a template to *assemble* a corresponding molecule of DNA. The DNA *travels* to the cell nucleus and *inserts itself* among the host's chromosomes, where it provides the basis for viral replication. (47, emphasis mine)

In describing this activity, Gallo is sensitive to his nonspecialized readers' need for more graphic visual images to help them *imagine* what they will probably never get the chance to *see*. The key differences are in the verbs: "converts" becomes "exploits," "moves" becomes "travels," and "integrates" becomes "inserts itself." The first set of verbs implies that what is occurring is a process in nature, a complex relationship between viruses and human T-cells. But the second set of terms builds a metaphoric theme centered on the retrovirus as an aggressive invader, a model likely to be recognized by his readers.

Mary Hess explains how metaphor works in scientific knowledge:

...metaphor works by transforming the associated ideas and implications of the secondary [which involves language] to the primary [involving observation] system. These select, emphasize, or suppress features of the primary [which]. . . is 'seen through' the frame of the secondary. (114)

In other words, through language we provide versions of the empirical world. The version of retrovirus activity in the paper for virologists suppresses features of aggression while his version prepared for unspecialized readers emphasizes these features.

Descriptions of how the virus transforms cells and infiltrates the bloodstream are also different. In *Annals* the writers explain that once infection occurs,

integrated viral genes are duplicated with the normal cellular genes so all progeny of the originally infected cell will contain the viral genes. The cell may become a cancer cell by the expression of one or more viral genes. In other instances, integration of the viral DNA form without expression of the viral genes may lead to a change in the expression of nearby cellular genes, leading to the pathologic effects of the virus. (679)

In *Scientific American* the story goes like this: Once infection occurs, and the T4 cells gets activated,

instead of yielding 1,000 progeny [as is a healthy system] the infected T cell proliferates into a stunted clone with perhaps as few as 10 members. When those 10 reach the blood stream and are stimulated by antigen, they begin producing virus and die. (50)

Obviously, Gallo constructed the second account so that visual imagery might accommodate the unspecialized reader and the context of a more "popular" publication. We can "see" the "stunted clone" and imagine the pathetic army of only ten fighters entering the blood stream, unaware that they carry within themselves the very enemy they seek.

In the *Annals* paper, Gallo seems intent on bringing the uniqueness of the HTLV family of viruses to light. This uniqueness is defined in terms of such things as the "indirect association with B-cell leukemia and lymphoma," and "a *trans*-acting transcriptional activation of viral long terminal repeat sequences" (686). Gallo explicitly labels these and other features as "unique family traits." Thus, in this paper, Gallo is concerned with showing how the human retroviruses differ from other viruses in their biological, biochemical, and epidemiologic uniqueness.

The retrovirus' uniqueness in comparison with other viruses plays a lesser role in the *Scientific American* paper. It is more important to gain the readers' understanding by helping them visualize through language of destruction and violence. Gallo depicts the retrovirus as a furious, violent agent that plays havoc with T4 cells:

The virus bursts into action, reproducing itself so furiously that the new virus particles escaping from the cell riddle the cellular membrane with holes and the lymphocyte dies. (49)

We may well imagine Audie Murphy bursting from a blown-up tank and riddling the German invaders with bullet holes.

Gallo proposes a hypothetical model, as he calls it, of the process of cell death:

[the cell's] death may depend on an interaction between the viral envelope and the cell membrane. Perhaps that interaction . . . punches a hole in the membrane. Because the virus buds in a mass of particles, the cell cannot repair the holes as fast as they are made; its contents leak out and it dies. (52)

The above description, "for the moment only a model" employs an analogy from our everyday experience. Terms such as "punch out" and "leaks out" suggest a visual model of puncturing a hole in a bag full of water. This modeling is actually both an hypothesis and a tool for explanation.

It is physically impossible for Gallo to directly observe the mechanisms contributing to cell death; thus Gallo employs analogy as a heuristic device for his own problem solving. This analogy also serves rhetorical purposes. The vivid and graphic qualities of his description enable the reader not only to visualize a biochemical process but also to picture a battleground which scientists like Gallo have spent countless hours investigating. Gallo wants the readers of *Scientific American* to know the formidable odds in fighting such an enemy and to understand that "the progress made in only three years" (55) represents the fruits of the valid scientific investigation done in his lab.

Given the exigencies that could have contributed to the rhetorical situation—the lawsuit against Gallo's patent claim as well as the laboratory evidence that the virus isolated by the French was unrelated to HTLV—we can assume that these papers represent Gallo's attempt to reclaim rhetorical ownership in AIDS research, to reassert the primary role of his laboratory in discovering human retroviruses and linking them to AIDS. However, I believe these accounts represent something more than one scientist's attempts to gain rhetorical ownership. These accounts actually tell us more about the social forces surrounding the acceptance of a theory than about the actual real world of the laboratory. Steve Woolgar has argued that "the practical expression of, or reference to, a phenomenon both recreates and establishes anew the existence of the phenomenon. In describing a phenomenon, participants [that is, writers and readers] simultaneously render its out-there-ness" (246). Gallo's depiction of the virus itself recreates the phenomenon, the retrovirus, which is a different critter for different audiences. And his depiction of his lab's role in human retrovirology and AIDS research not only recreates that role but rekindles communal expectations about the kind of laboratory investigation that leads to significant discoveries. Though certainly more research of the discovery account genre is needed before we can draw definitive conclusions about them, I suggest that discovery accounts, a recognized form of valedictory address which usually occurs after a community has settled issues and reached consensus, may actually be employed by writers to settle those issues and establish their rhetorical ownership in the field. Gallo adopts the discourse appropriate for a closed debate as an active argumentative resource in an open battle.

Notes

¹According to Gallo, "A retrovirus carries within its core an enzyme that synthesizes DNA, reverse transcriptase. This enzyme converts viral RNA into double stranded DNA. For virtually all retroviruses, the DNA form moves from the cytoplasm to the nucleus where it integrates into the host cell DNA" (*Annals* 679).

²For a thorough account of the controversy between Gallo and the French, see "Science Under the Microscope" by John Crewdson. *The Chicago Tribune*, November 18, 1989.

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