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# Establishing a Phenomenon

## The Rhetoric of Early Medical Reports on AIDS

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### Abstract

In the first three medical reports on AIDS which were published in 1981 in the *New England Journal of Medicine*, the writers' primary rhetorical agenda was to argue that a new medical discovery had been made. A secondary agenda was to offer etiological explanations for the new problem. To establish the new disease entity as deserving serious attention, the writers built a sense of mystery by confronting established medical knowledge about immunodeficiency and emphasizing the inability of modern medicine to diagnose and treat the problem. When they explained the phenomenon in etiological terms, rather than confronting the disciplinary matrix, the writers relied on established medical knowledge of infection rates in homosexual males as well as prevailing social views about the dangerous nature of male homosexual activity; consequently, they were able to imply that nothing was mysterious or surprising about immunodeficiency in homosexual males.

In December 1981, the *New England Journal of Medicine* published three reports (Gottlieb et al.; Masur et al.; Siegal et al.) announcing the discovery of immune system failure in young, previously healthy patients. In making such an announcement, the three medical teams were attempting to establish a *new* medical problem and thus had to characterize it for the sake of future diagnosis, treatment, and research. But since most of the patients were homosexual males, the writers were also recounting a story of morbidity and mortality among the "wrong patients," as Gallo (1987, p. 47) later termed them. Thus explaining the presence of the new condition in homosexual males was another component of the rhetorical agenda. The writers' strategies in *establishing* and *explaining* the new syndrome for the benefit of practitioners and researchers who would later treat and investigate the problem are the focus of this analysis. I will argue through an analysis of the introductions, patient histories, and discus-

sions in these reports that establishing and explaining the new phenomenon required two distinct rhetorical strategies. In establishing a phenomenon, the writers *confronted* disciplinary knowledge whereas in explaining a phenomenon, they *relied* on such knowledge in making etiological claims.

How a given community or discipline establishes and validates a phenomenon as appropriate for inquiry is an important question for rhetorical studies. Sociologist Robert Merton (1987) distinguished the cognitive and social patterns of "establishing the phenomenon" and "explaining the phenomenon" (p. 1). The former pattern aims at definitive characterization while the latter pattern concerns identification of cause and effect relationships. Merton argued that unscientific and invalid theories result from attempts to explain a phenomenon that has not been characterized or shown to exist. Given Merton's warning, the fact that two of these three reports on AIDS attempts to explain the new phenomenon before there was consensus about its existence proves an interesting problem for rhetorical inquiry.

As Merton (1973) noted, the competition between collectives to be the first to characterize a problem gives a "political dimension" to the activity of establishing the phenomenon. Collectives compete to "capture what Heidegger called the 'public interpretation of reality.' With varying degrees of intent, groups in conflict want to make their interpretations the prevailing one of how things were and are and will be" (pp. 110-111). In 1981, those first announcing a new form of immunodeficiency naturally wanted their interpretations to prevail over other possible interpretations. That year, four reports were published on the new immunodeficiency: the three cited here and an earlier one published in June in the *Morbidity and Mortality Weekly Report* ("Pneumocystis," 1981). The writers of these reports had to characterize the disorder in such a way that the medical community recognized a threat to public health and took measures to solve the problem. These characterizations had to be compelling enough to contribute to a public understanding of the condition as a new medical mystery deserving attention.

Unfortunately, clinical difficulties with characterizing and interpreting the devastating symptoms led to significant rhetorical problems for the writers who first attempted to establish and explain the problem. Because of the bizarre constellation of the patients' symptoms, disagreement about the writers' interpretations of the problem was inevitable. Modern technology could provide no definitive explanation for loss of immune functions in previously healthy patients.

The most sophisticated assays revealed that the patients had no helper T-lymphocytes, which are cells serving important immune and regulatory functions and which are responsible for cell-mediated immunity. Without helper cells, the immune system simply does not work. The findings that the patients lacked these cells were surprising; however, the assays could not reveal an underlying condition that might have led to the problem. In addition, there was no objective way to determine which of the various infections that accompanied the syndrome was the cause and which the effects of immunodeficiency. Typically present were a rare pneumonia (*pneumocystis carinii*), unusual parasitic infections, a rare skin cancer that normally occurred in older men (Kaposi's sarcoma), and severe cases of normally treatable viruses such as cytomegalovirus (CMV) and herpes simplex virus (HSV). Siegal (1983) explained that the lack of historical precedent made attempts to characterize the problem almost impossible. He distinguished AIDS from other scourges such as cholera, polio, influenza, and the Black Plague: "AIDS has introduced a new dimension: instead of merely eluding, it directly attacks the sophisticated structure of immunity that evolution and individual experience have built . . ." Such a "remarkable experiment of nature," (p. 1) as he termed it, would necessarily offer specific rhetorical challenges for those first describing it.

Besides the difficulty with definitive characterization, another component in the rhetorical problem faced by these writers in 1981 was that no agreement existed among clinicians and practitioners that this new problem warranted massive research efforts. Despite the June 1981 *Morbidity and Mortality Weekly Report* of *pneumocystis carinii* pneumonia in homosexual males, the problem of immunodeficiency had not become a significant research problem by December. As Shilts (1987) reported, during a September 1981 conference on Kaposi's sarcoma and opportunistic infections at the National Cancer Institute (NCI), experts ignored the outbreak of immunodeficiency among homosexual males in the United States. Instead, they debated the intricacies of Kaposi's sarcoma and how it had been treated in Africa without addressing its possible relation to the outbreak of immunodeficiency in the United States. Shilts noted that except for a paper presenting epidemiological work by a doctor who had treated such patients, no discussion of the problem of immunodeficiency in homosexual males occurred at the conference (pp. 93-95). Yet by September 1981, 120 cases of immunodeficiency in previously healthy persons had been reported (p. 94). According to Shilts, the lack of urgency with

respect to the issue at the NCI conference suggested to clinicians who had treated these patients that "no one cared because it was homosexuals who were dying" (p. 95). Shilts also contended that "people died while scientists did not at first devote appropriate attention to the epidemic because they perceived little prestige to be gained in studying a homosexual affliction" (p. xxii).

Another way to view the situation would be to argue that because only one report had been published on an outbreak of opportunistic infections previous to the three discussed here, scientists simply were not aware of the problem. The writers who first treated and reported the problem not only had to convince the medical community that a problem existed but also had to build into their characterization of the disease the social necessity for additional research.

The sexual orientation of the first patients to be diagnosed with acquired immunodeficiency played a significant role in the early characterization of the disease as well as the development of attitudes toward it. An editorial in the same issue of the *New England Journal of Medicine* in which the three reports under discussion appeared stated that the new syndrome had appeared in a "liberated sub-group" (Durack, 1981, p. 1466). In early 1982, Gilkey referred to the disease as "homosexuality-related" (p. 933). The assumption that the disease was related to male homosexual activity was fueled by knowledge of disease rates in homosexual groups as well as prevailing social attitudes about homosexual activity. Clinical studies in the 1970s had demonstrated high rates of sexually transmitted disease, including unusual parasitic infections, among homosexual males. Certain infections, such as the gay bowel syndrome, were linked exclusively to the male homosexual population. This documented knowledge of disease in such patients prepared many doctors to think "that a new 'gay cancer' had surfaced in the United States" (Siegal, 1983, pp. 1-2); hence the problem was not viewed as a threat to the wider heterosexual population. Although there was growing awareness of the problem as a potential threat to homosexual males, there was no consensus among scientists that it represented a medical mystery that required significant funding and research.

In building research incentives, writers also had to make significant decisions about the language they used in identifying risk groups and characterizing the disease. This language often contained plague metaphors. Although Siegal insisted that AIDS should not be clinically compared to previous incidences of plague, the language of AIDS was replete with the "end of the world rhetoric" that Sontag

(1988, p. 86) identified as a "numbing contemplation of catastrophe." As Sontag argued, public discourse is often well intentioned but dangerously unconscious of the implications of metaphor. The plague metaphor evokes images of "collective calamity, evil, [or] scourge" (p. 44), of illness that is "inflicted, not just endured" (p. 45). Moreover, according to Sontag, "there is a link between imagining disease and imagining foreignness. It lies perhaps in the very concept of wrong, which is archaically identical with the non-us, the alien" (p. 48). The metaphors of plague combined easily with popular notions of homosexuals as foreign, as "a community of pariahs" (p. 25). In addition, such metaphors contributed to a conception of AIDS as inevitably fatal and patients as helpless victims. These conceptions evoked the inevitability of "generic defeat" and made AIDS the "rebuke to life and hope" (p. 24), a certain death sentence. As Burke (1966) taught us, "Much that we take as observations about 'reality' may be but the spinning out of possibilities implicit in our particular choice of terms" (p. 46). Sontag (1988) argued that these "implicit possibilities," the ideological and social implications in any given terminology, must be "exposed, criticized, belabored, used up" (p. 94).

The details of the rhetorical situation outlined here contributed to the necessity that writers of early reports on the new immunodeficiency construct arguments with warrants that reached beyond the empirical data and called on premises about the nature of compelling medical mysteries. Although the data indicated that the patients' immune systems were depressed or nonexistent, they could not indicate that the condition was new, that it was infectious, and that it deserved the attention of researchers. The writers had to argue what the data implied: that a highly complex medical mystery deserving of aggressive research had been uncovered. They needed to establish a sense of urgency by demonstrating that this phenomenon represented the kind of problem that could not be solved by the then-current understanding and practice in biomedicine.

Holton's (1965) theory that a thematic imagination in science plays a significant role in building scientific theory and establishing research problems is applicable here. Holton contended that "the process of building up an actual scientific theory requires explicit or implicit decisions, such as the adoption of certain hypotheses and criteria of preselection that are not at all scientifically 'valid' in most cases" (p. 90). Communal acceptance of a research problem or a new theory involves decisions based on "presuppositions, notions, terms, methodological judgments and decisions" (p. 98) embedded in

interpretive networks. One theme embedded in the cognitive network of scientific investigation is the concept of mystery. In medical science, morbidity and mortality are warrants for considering a medical problem worthy of attention, but thematic conceptions of a problem as unsolvable and unexplained, as a medical mystery, also drive research in biomedicine. In order to attract the attention of the research community, the writers of the early reports on the new problem of acquired immunodeficiency were faced with the rhetorical task of building a context of surprise and mystery.

Previous investigation into the ways that problems come to be viewed by scientific communities as significant provides insight into the task faced by the medical teams discussed here. Myers (1984) showed how grant writers "persuade without seeming to persuade" (p. 220) by projecting an authoritative persona and attempting to redefine a field in order to clarify the connection between their new idea or problem and the needs of the disciplinary matrix. He contended that announcing new research problems poses special rhetorical problems, and in such cases, "one must either present a persona as an established member of one of the fields, or redraw the fields around the work" (p. 221). Myers's findings implied that the rhetorical strategies that writers employ in grant proposals eventually influence the shape of a field by offering new problems or new approaches to old problems.

Prevailing social attitudes may also influence community attitudes toward the selection of research problems. In his analysis of syphilis research, Fleck (1979) illustrated how research objectives could be shaped by the "habits of thought" informed by the discursive practices, the interactions and negotiations, within "thought collectives," which he defined as a "community of persons mutually exchanging ideas or maintaining intellectual interaction" (p. 39). In tracing the history of syphilis research, Fleck demonstrated how social impetus, identified in the terminology used by collectives to describe the disease, influenced research objectives. As long as syphilis was seen as a "carnal scourge," serious investigation did not occur. A massive research effort began only after syphilis came to be viewed as an "empirical-therapeutic disease," a conception fueled in part by a prevailing social attitude that research ought to be done (p. 10). For Fleck, "cognition is the most socially-conditioned activity of man, and knowledge is the paramount social creation" (p. 42).

Similarly, Latour and Woolgar (1979) traced the social construction of a scientific fact and the formation of a new field of study by

observing the negotiations and the inscriptive practices in a Pasteur Institute laboratory. The authors stated that they were "concerned with the *social* construction of scientific knowledge in so far as this draws attention to the *process* by which scientists make sense of their observations" (p. 32). As they demonstrated, the process of fact construction is not as well ordered as scientists' reconstructions and rationalizations in published literature might suggest but often "entails the confrontation and negation of utter confusion" (p. 36). Fact construction also involves persuasion that

enables them [scientists] to convince others that what they do is important, that what they say is true, and that their proposals are worth funding. They are so skillful, indeed, that they manage to convince others not that they are being convinced but that they are simply following a consistent line of interpretation of available evidence. (p. 70)

These previous investigations into the social and rhetorical nature of building theory, constructing facts, and identifying research problems are germane to the present discussion. In the three early reports on AIDS, the most compelling social imperative was to announce findings of a new disease threatening the health of some groups and to attract the attention of researchers; the writers had to *establish* the phenomenon as new, as a medical mystery, and as a challenging research problem. In establishing the condition as a *new* phenomenon, their primary rhetorical agenda was to *reject* documented cases and knowledge of immunodeficiency states as suitable explanations of the morbidity and mortality they had observed. They built incentives for research by emphasizing the tragic course of the disease and their failure in treating it. These two rhetorical strategies constructed the disease as a surprising medical problem worthy of aggressive investigation.

*Explaining* the phenomenon is another pattern found in the 1981 reports by Gottlieb et al. and Siegal et al., whose patients were all homosexual males. To explain the problem, the writers connected male homosexual activity with disease etiology. Rather than *confronting* disciplinary knowledge as they had in establishing the problem as a new medical mystery, Gottlieb et al. and Siegal et al. *relied* on documented knowledge of disease in homosexual males, and on tacit understanding of male homosexual behavior to explain the problem. The resulting implication was that the occurrence of such profound immunodeficiency in homosexual males was not so surprising after all.



## CONFRONTING THE DISCIPLINE

Throughout these three reports, the writers established the phenomenon of AIDS by confronting disciplinary knowledge about immune deficiency states. Such a strategy served two purposes. First, by enumerating such knowledge, the writers demonstrated their membership in the disciplinary matrix. They demonstrated that they had thoroughly considered all possible explanations. Enumeration of the *known* directed them in their struggle for rhetorical control over the *unknown*. Moreover, the lists of possible explanations, of definitions of what the problem reported was *not*, amplified the mysterious nature of a problem that asked to be solved.

Ziman (1968) argued that the element of surprise plays a highly rhetorical role in scientific discourse because it gives a proposition "weight" as a contribution to knowledge (p. 50). The unexpected announcement, the unanticipated finding, the unusual evidence—all are solid commodities in scientific argument. The surprise element in science comes into play when an observation cannot be explained by existing knowledge. The titles of all three reports emphasized the surprising fact that a condition previously associated with transplantation and chemotherapy was suddenly afflicting previously healthy young adult men:

Gottlieb et al. (1981, p. 1426): "Pneumocystis Carinii Pneumonia and Mucosal Candidiasis in Previously Healthy Homosexual Men"

Masur et al. (1981, p. 1431): "An Outbreak of Community-Acquired Pneumocystis Carinii Pneumonia"

Siegel et al. (1981, p. 1439): "Severe Acquired Immunodeficiency in Male Homosexuals, Manifested by Chronic Perianal Ulcerative Herpes Simplex Lesions"

Although the titles emphasized the empirical evidence, they also hinted at the implications of this evidence; such a tendency in titles has been noted previously (Bazerman, 1984, p. 182). Terms such as "homosexual," "outbreak," and "community" suggested possible risks to certain groups.

In the introductions to their reports, the writers constructed the context of surprise by delineating existing knowledge and then announcing findings that threatened to modify that knowledge. For example, in their introduction, Gottlieb et al. (1981) outlined the known conditions that involve acquired T-cell defects—"untreated Hodgkin's disease, sarcoidosis, and viral infections" as well as

immunosuppressive therapy (p. 1426). The authors also added that "opportunistic infections rarely occur in the absence of immunosuppressive therapy" (p. 1426). Masur et al. (1981) described *pneumocystis carinii* in their introduction as a "ubiquitous organism" that "rarely if ever causes disease in immunologically competent persons" (p. 1431). Siegal et al. (1981) emphasized that severe, chronic lesions "are unusual even in patients with severe immunologic defects" (p. 1499).

All three introductions then followed a similar pattern. After indicating the rare and unusual nature of the conditions described, the writers included statements of principal findings that threatened to modify existing knowledge and assumptions. The active constructions used by the writers in these declarative statements evoked the quality of personal testimony and the authority of first-hand observation. Gottlieb et al. stated:

We recently treated several young previously healthy homosexual men for multiple mucosa! candidiasis, and severe viral infections. (p. 1426)

Masur et al. stated:

We recently recognized 11 cases of this disease in young men with no previous history to suggest immunologic dysfunction. (p. 1431)

Siegal et al. stated:

In four previously healthy homosexual men we found chronic perianal ulcers infected with HSV. (p. 1439)

Following the statements of what was known about immunodeficiency states, these statements juxtaposed the doctors' first-hand experience to the disciplinary consensus. These statements also served as ethical appeals to powerful commodities in medicine—doctors' observations of their patients. The ethical appeal, as Halloran (1984, p. 79) argued in his analysis of the ethos in Crick and Watson's report on their model of DNA, is often crucial to a report's acceptance in a community. In these introductions to the first reports on AIDS, active constructions and a declarative tone created the ethos of the careful practitioner whose observations could be trusted. The writers' message was clear: Although the data are surprising and although there are no precedents in the literature for such a phenomenon, *we* did treat these patients and did *observe* their unusual conditions.

The apodictic tone helped substantiate the writers' claims that existing knowledge did not explain the new disease. When they addressed alternative hypotheses in the discussion sections, they used the same tone to advance the argument. Gottlieb et al. (1981) countered the hypothesis that the problem was actually mononucleosis. They argued that this "distinct and unusual clinical syndrome is clearly unrelated to cytomegalovirus-induced mononucleosis" because "the persistence of fever for more than three months and the occurrence of leukopenia, lymphopenia, and opportunistic infection are not features of the cytomegalovirus-monomucleosis syndrome in the normal host" (p. 1428). Moreover, as they pointed out, in normal subjects, cytomegalovirus infection is not associated with "depression of T-cell numbers to the degree observed in our patients and . . . proliferative responses to the degree observed in our patients have not been reported to occur in cytomegalovirus induced syndrome in normal persons" (p. 1429). These statements contributed to the central argument that the condition was new, that it was not related to a known and well characterized disease. The premise appealed to was that what was unexplained by current knowledge and theory proves an interesting and challenging problem for research.

The focus in Masur et al.'s (1981) discussion of their findings was that the *pneumocystis pneumonia* found in patients could not be explained by the findings from previous reports of the pneumonia in infants with congenital problems or protein malnutrition or in homosexual males with cytomegalovirus. None of these cases applied, the writers argued, because their own patients were well nourished adults who had no history of congenital problems and because only two of the five homosexual patients and none of the drug users had CMV (p. 1437). They even dismissed reports with findings ostensibly similar to their own: "Three adults and two children have been reported to have *p. carinii* pneumonia and two have had no immunosuppressive disease discovered at autopsy. However, these patients had only limited immunologic studies performed" (p. 1437). This statement not only discounted the previous report as having any bearing on the present cases but also reminded readers that the present findings were based on thorough investigation.

Siegel et al. (1981) stressed that existing knowledge did not explain the presence of such severe and unusual symptoms in previously healthy adults. Their discussion began with a restatement of what had been affirmed in the introduction—that "ulcerative lesions caused by

HSV are usually observed only in patients with severe deficits of cellular immunity associated with another underlying disease" (p. 1441). Illustrating the severity of the lesions were photographs of two patients' conditions. The writers argued that the HSV lesions suggested "dysfunction in cellular immunity" (p. 1441) and insisted that "these cases are rare, even among homosexuals" (p. 1443).

Confronting disciplinary knowledge of immunodeficiency states was the major rhetorical strategy used by these writers to convince their readers that the problem they described represented a new disease entity and thus a new research problem. This confrontation took several forms. In their introductions, the writers juxtaposed their direct observation of patients with the disciplinary consensus; in the discussion of findings, they disproved alternative hypotheses connecting the problem to some well characterized disease and discounted the possible relevance of previously reported cases of acquired immunodeficiency. Word choice and sentence constructions undergirded this confrontation. Adjectives such as "unusual" and "rare" helped to build the context of mystery, and the active constructions that emphasized the doctors' role in the cases contributed to an ethical appeal to the legitimacy of first-hand observation and experience. Clearly, these writers wanted to establish this problem as something more lethal and baffling than the diseases commonly associated with known immunodeficiency states or with diseases known to occur in homosexual males.

#### CHARACTERIZING THE PROBLEM IN PATIENT HISTORIES

The patient histories presented in these three 1981 reports played a crucial role in the construction of the new disease as a baffling mystery. Descriptions of the symptoms and progression of a disease are necessary in any work reporting a medical problem, but they become even more so when writers are presenting what they believe to be a new phenomenon. It is important to remember that in December 1981, there was no standard definition of the disease and no coordinated institutional disease management plan; there was not even a name for the disease. It was not until July 1982 that officials at the Centers for Disease Control (CDC), agreed to use the acronym AIDS to denote acquired immunodeficiency syndrome (Shilts, 1987,

p. 171). The CDC's surveillance definition of AIDS did not begin to appear in medical reports until late 1982 and early 1983. The characterizations of symptoms and disease progression in the early reports on AIDS were cited in subsequent reports and thus played an important role in the process of definition. In addition, these patient histories served those who would eventually treat such patients by outlining key symptoms. The writers were among the first to describe the problem, and their descriptions were crucial to diagnosis, treatment, and definition.

A conventional case history must narrate the important details of disease progression, medical treatment, and outcome. Patients are described in objective terms, usually identified by a number, and only pertinent physical details about the patient are included. Passive constructions are conventional. The purpose of such histories in the office or hospital setting is obvious—without thorough history, diagnosis and treatment would be impossible. However, the case history as it appears in medical journals serves purposes other than those directly related to the treatment of particular patients. Published patient histories serve writers' rhetorical agendas by offering support for the claims being made about the effectiveness or ineffectiveness of some course of treatment. The rhetorical force of the case history becomes, in some measure, a function of narrative—the story itself must be true-to-experience (or else test notions of valid experience) and compelling.

The case histories presented in these reports were all conventional: They were written in passive voice and contained objective descriptions of patients and narrations of the course of disease and treatment. They also functioned as compelling "stories" of mysterious, unexplained morbidity and mortality, and the patients served as characters, as examples of the condition being established. These stories were not only compelling but also interesting because what they described violated notions about the capability of modern medicine to treat common problems, such as high fever and herpes lesions, and to identify an underlying condition contributing to opportunistic infections. Rather than depicting *success* in treatment, the histories highlighted *failure*—the failure of modern medical knowledge and technology.

Gottlieb et al. and Siegal et al. included conventional case histories that followed the course of treatment for individual patients. The strategy of Masur et al. was to include relevant demographic information in a chart rather than in a narrative account and to emphasize

in the text what was most pertinent to the claim that the immunodeficiency was acquired. Both strategies highlighted the severity and unrelenting nature of the condition as well as the failure of the medical teams to treat it. In Masur et al. (1981), autopsy accounts served to reiterate the lethal and mysterious nature of the condition. These accounts emphasized the finding of no underlying condition that could explain the patients' crippled immune systems:

[Patient 1] died suddenly at home; at autopsy he was found to have extensive *pneumocystis* pneumonia but no evidence of an immunosuppressive disease. Patient 2 died 10 months after lung biopsy because of a progressive febrile disease . . . but no underlying disease was found . . . at laparotomy. . . . Patient 7 died seven months after lung biopsy because of another episode of *p. carinii* pneumonia; autopsy did not reveal an underlying disease. (p. 1434)

The repetition of the phrase "no underlying condition" reinforced the fatal, mysterious nature of a disease that defied definitive diagnosis.

Fleck (1979) noted that "no matter how a given case may be described, the description is always a simplification permeated with apodictic and graphic elements" (p. 114). He also suggested that the "pictorial quality" of scientific description is chosen "to render an idea intelligible to others for mnemonic reasons" and that "certainty, simplicity, and vividness" are aims of most description in scientific discourse (p. 117). The narratives in Gottlieb et al. and Siegal et al. may be viewed as apodictic and graphic illustrations of a mysterious and frustrating disease and its debilitating effect on the patient.

Because doctors have been trained to heal and to expect results, they become frustrated when patients' conditions do not improve and when illness cannot be explained. These writers appealed to a communitywide sense of frustration by carefully detailing the successive events of suffering and failure in the tragic course of the disease. There were first the unexplained, normally treatable conditions such as "recurrent fever," "severe lesions," "pain on swallowing," and "lymphadenopathy" (enlarged lymph nodes). Symptoms were "recurrent," "atypical," "severe," "arid," "continuing" (Gottlieb et al., 1981, p. 1426). References to the failures in diagnosis and treatment of these symptoms became key rhetorical features of patient histories. For example, Siegal et al. (1981) reported that in one patient, "parenteral nutritional supplements, transfusions and antibiotics were given, but without

benefit" and that despite later treatment with "human-leukocyte interferon," the patient died (p. 1439). Another general feature of these histories was the information about the mysterious progression of symptoms; some symptoms were reported to have improved while others worsened. Gottlieb et al. (1981) reported that there was "marked improvement of the gluteal and fingertip lesions [after drug therapy]; however, daily episodes of fever continued" (p.1426). Often, as the histories revealed, treatment only caused additional problems. Gottlieb et al. reported that in one patient, prednisone was prescribed for fever after which the patient returned with "severe oral candidiasis," a yeast infection commonly known as thrush. Furthermore, patients wasted away, becoming overpowered by the disease. "The patient's condition deteriorated despite five days of pentamidine" (p. 1427); Siegal et al. (1981) stated that "by August, the patient had lost approximately half of his original weight" (p. 1349). Ultimately, modern technological innovations failed; biopsies, colostomies, chest X rays, and blood studies either did not reveal anything significant or found something of interest only after nothing could be done about the condition. The inevitability of death pervaded these accounts.

These histories were designed to tell the story of a baffling mystery; the impact they may have had on the reader was probably due as much to the way they were written as to the empirical evidence they related. Although passive constructions traditionally create the objective tone that writers try to achieve in such descriptions, the bare, chronological ordering of these patients' symptoms offered a subliminal appeal to emotion. Polanyi (1964) argued that "the quickest impression on the scientific world may be made not by publishing the whole truth and nothing but the truth, but rather by serving up an interesting and plausible story composed of parts of the truth" (p.53). These histories contained "parts" of the truth, the parts that the writers felt were most crucial to their argument that a compelling new condition had been identified. These "parts" were arranged in order to make the greatest impact on the reader. Each history began with statements about a patient suffering from a supposedly treatable ailment, followed by statements indicating the deteriorating condition of the patient, and ended with statements about the patient's inevitable death and autopsy report.

Undoubtedly, the narrative histories in Gottlieb et al. and Siegal et al. excluded "parts" of the truth. The months of treatment entailed countless interviews with patients, yet, other than a statement about

their sexual orientation and age, specific information gleaned from those interviews about the patients as persons, as unique individuals, was excluded. Naturally, in their attempt to focus on symptoms found in a group of patients, the writers were not concerned with describing the individual in a capacity other than that of a body invaded by disease. As Grinnell (1987) noted: "The scientific attitude applied to people is potentially de-individualizing because it deals with unique individuals-like other subject matter-as typical examples of a class rather than as individuals" (p. 27). The "de-individualization" of patients in histories is conventional, of course, but in these early reports on AIDS, designating patients as belonging to certain "populations" became rhetorically significant because Gottlieb et al. and Siegal et al. would later attempt to explain the existence of the disorder by referring to the behavior of homosexual males. The patients in these histories became *examples* of the wider population at risk, and their suffering became the defining characteristics of a future medical crisis.

These patient histories displayed past events-disease, treatment, failure, and death-but also created incentives for future action. All three medical reports can be seen as bridging the past and the future and thus should not be viewed as wholly forensic discourse. Fahnestock (1986) classified scientific reports as forensic discourse because they argue for the validity of past observations (p. 278). While the writers of these reports argued for the validity of their past observations and experience, they also wanted to warn the medical community of an impending medical emergency and to stimulate investigation. Aristotle (1960) argued that "examples are best suited to deliberative speeches, since we judge of things to come by divining from things that have gone before" (p. 54). The histories worked to illustrate "things to come" and built social imperatives driving research. The writers' focus on failure, their own failures, worked as examples of "things to come"-morbidity and mortality in certain groups-and thus built social imperatives driving research. They also offered more specific recommendations for future treatment and diagnosis. Gottlieb et al. (1981) stated:

We therefore believe that long-term TMP-SMZ prophylaxis should be initiated in such patients after the first episode of *pneumocystis*. (p. 1430)

Masur et al. (1981) stated:



An awareness of the association [between opportunistic infection and abnormal cellular immune responses] should prompt further study of the epidemiology of *pneumocystis* infection and an aggressive diagnostic approach to diffuse pneumonias that occur in these groups of patients. (p. 1437)

Siegal et al. (1981) stated:

It seems possible that earlier recognition and prospective study of such patients will reveal an anomaly in host defense that could illuminate the pathogenesis of this disorder. (p. 1443)

Because these recommendations implied that future action *must* be taken, it is possible to view these reports as having both forensic and deliberative discourse features.

#### EXPLAINING THE PROBLEM

We have seen how these writers characterized the problem of unexplained immunodeficiency in patients as new and mysterious by confronting existing knowledge and offering examples of disease manifestations in patients. Although, as Merton (1987) contended, establishing the phenomenon should always come before attempts to explain or interpret it, both Gottlieb et al. and Siegal et al. attempted to explain the etiology of the new syndrome. Masur et al., however, avoided such explanations altogether. Why Gottlieb et al. and Siegal et al. chose to posit explanations of the problem before a consensus about the problem as a new disease entity had been established is not clear. Perhaps both of these medical teams wanted their etiological propositions to have "explanative power." Popper (1962) argued that scientific propositions must not only report findings but present conclusions and discuss implications of findings: "Although we seek theories with a high degree of corroboration, as scientists we do not seek highly probable theories but explanations; that is to say, powerful and improbable theories" (p. 58). Possibly, the writers also hoped to be the first to solve the mystery of disease etiology. In explaining a new phenomenon, being the first to trace causes and to distinguish effects has a clear rhetorical dimension. However, the more probable explanation of the scientists' willingness to attempt explanation is the

fact that all of their patients were homosexual males. Specialized knowledge of sexually transmitted disease among such patients, as well as less specialized presuppositions about sexual practices associated with homosexuality, were very likely warrants behind speculations that the new disease was somehow related to an environmental factor in the homosexual community. In building their etiological claims, Gottlieb et al. and Siegal et al. cited documented knowledge of high rates of certain infections in homosexual males and used language that implied agreement in the medical community about the dangerous nature of homosexual behavior. Rather than creating surprise, as they had when establishing the new syndrome, the writers suggested in their explanations that the existence of such a phenomenon in male homosexual patients was not so surprising after all.

Gottlieb et al. and Siegal et al. argued that the disease was the result of repeated exposure to infectious agents via sexual contact. Repeated exposure allowed for reinfection, which compromised the person's immune system. Part of the warrant for this conclusion was the documented high rates of cytomegalovirus and herpes simplex virus in homosexual males. Gottlieb et al. (1981) cited three studies revealing that "sexually transmitted infections, including cytomegalovirus, are highly prevalent in the male homosexual community" (p. 1429). Siegal et al. (1981) cited the June 1981 *Morbidity and Mortality Weekly Report* article on *pneumocystis carinii* in homosexuals and a study documenting "a 94 percent prevalence" of cytomegalovirus infection in "the homosexual community" (p. 1443).

These references to documented knowledge of disease in male homosexuals were accompanied by more implicitly worded references to the assumed promiscuous nature of male homosexuals. As Holton (1965) maintained, the themes that scientists use are "largely left implicit rather than explicit" (p. 104). The implicitness of the attitudes about the dangerous nature of homosexual activity was evident in the language of the etiological claims made in these two papers. Gottlieb et al. (1981) carefully avoided explicit references to particular sexual behaviors; however, the connection between the homosexual and disease was made to seem unsurprising:

The fact that this illness was first observed in homosexuals is probably not due to coincidence. It suggests that a sexually transmitted infectious agent or exposure to a common environment has a critical role in the pathogenesis of the immunodeficient state.

. . . [T]he shedding of virus for prolonged periods in many secretions, including semen, facilitates sexual transmission. (p.1429)

The implication was that a sexually active person could become infected and then infect others:

[A]fter infection with cytomegalovirus, very high titers of the virus may be shed in the semen of asymptomatic subjects for more than a year [and] it is therefore likely that sexually active, young homosexual men are frequently reinfected. (p. 1430)

The writers also suggested that "a new strain" of cytomegalovirus "transmitted initially within the male homosexual population" (p.1430) could have caused the problem initially.

Likewise, in Siegal et al. (1981), what was assumed to be heavy sexual activity among homosexual males was linked to the new problem:

Viral infection, especially in unusually heavy inoculum transmitted by enteric routes, may be an important initiating factor.

Exposure to cytomegalovirus is known to be particularly heavy within the homosexual community.

Heavy exposure to Herpes Simplex Virus could lead to chronic infection, and secondary immunodeficiency could result.

. . . [A]mong men who are homosexual, some have a latent, broad-based cellular immunodeficiency that becomes clinically manifest only because of heavy exposure to certain pathogens in particular combinations. (p. 1443)

The phrases "enteric routes," "heavy exposure," and "frequent exposure to a common environment" suggested factors that might distinguish homosexuals from heterosexuals and place them at higher risk. However, it would be logical to assume that under the conditions described, *anyone* who was promiscuous or engaged in anal sex could be at risk; however, the writers never suggested risks to heterosexuals or distinguished heterosexual and homosexual promiscuity. Shilts's (1987) book contains straightforward accounts of the sexual behavior of the early AIDS patients. Though not all of these patients frequented bathhouses or engaged in numerous anonymous sexual encounters,

they had known people who did. Knowledge of these habits was reflected in the reports by Gottlieb et al. and Siegal et al. by such terms as "enteric routes" and "heavy inoculum." These terms are tactful references to practices that the writers viewed as dangerous and common among homosexual males.

The implicitness of this knowledge of homosexual behavior is revealed in the fact that direct discussion and evidence of the patients' sexual behavior played a very insignificant role in the etiological claim. For example, Gottlieb et al. (1981) stated that "the four patients in this report did not know each other. In-depth interviews did not reveal common contacts or knowledge of sexual partners who had been ill"; one was involved in a long-term monogamous relationship, one had two lovers, and only one was "highly sexually active and frequented homosexual bars and bath houses" (p. 1429). Siegal et al. (1981) noted only that "there was no obvious contact between the four men" (p. 1443). This lack of a lengthy discussion of specific sexual behaviors was likely the result of the writers' sense of propriety as well as their sense of their audience's awareness of these behaviors. Their own presuppositions about the risks linked to such behavior as well as the belief that they shared these presuppositions with their audience might have led the writers to consider direct references and documentation unnecessary.

In his discussion of common arguments in sociology, Overington (1977) claimed that the most familiar form of the causal argument is that which "tries to establish a connection of some event to what is recognized as a reality" (p. 159). He explained that "a 'real' effect is shown to be related to a cause that may then plausibly be taken as equally 'real'" (p. 159). By linking male homosexual activity and immunodeficiency, the writers connected the "real" effect-disease-with the plausibly "real" cause-"dangerous" sexual activity within the male homosexual population. Their argument appears to be inductive: After observing these cases of immunodeficiency in homosexual men, the doctors arrived at the theory that something in the sexual behavior of homosexual males was causing the problem. However, as Popper (1959) argued in his criticism of Hume, most of the time what appears to be induction is actually deduction: "Without waiting, passively, for repetitions to impress or impose regularities upon us, we actively impose regularities upon the world" (p. 46). Much of this active imposition is a matter of guessing based on belief.

In an interview conducted by the author on October 6, 1988, Grinnell, Professor of Anatomy and Cell Biology at the University of

Texas Health Science Center /Southwestern Medical School in Dallas, commented on the deductive processes involved in the early etiological theories about AIDS. He said it was possible that "in 1981, most doctors had treated very few patients they knew were homosexual and probably were predisposed to viewing homosexual behavior as different, an implicit assumption that might have affected the way they viewed the problem." Perhaps it was not so much doctors' experience in treating homosexuals and direct knowledge of their patients' behavior as their beliefs about this behavior that guided their etiological theories. Fleck's (1979) notion of "thought style" is applicable in this case. He argued that a "thought style" that has formed around a subject within a community is a "definite constraint on thought; and even more, it is the entirety of intellectual preparedness or readiness for one particular way of seeing and acting and no other" (p.64). It is possible that a "thought style" created the thematic context of the "dangerous, promiscuous homosexual," a context that played a crucial role in claims linking the sexual behavior of homosexual males with etiology of the disease.

It is highly significant that Masur et al. (1981), whose patients were not exclusively homosexual, avoided referring, even implicitly, to sexual habits. The writers stated in the discussion that "a common environmental substance that might have been a factor in the immunosuppression in this group of young men could not be identified" (p. 1437). They claimed that their data "suggest that immunologic evaluation of homosexuals and drug users should be reassessed in conjunction with epidemiologic investigations of factors that could subject subpopulations to unusual risks for neoplastic disease and opportunistic infections" (p. 1443). However, no references were made to the type or frequency of sexual behaviors. Other than what was implied by referring to a person as a homosexual or a drug user, the report by Masur et al. contained no other references to what might be habits or behaviors contributing to immunodeficiency.

## CONCLUSION

Merton's (1987) suggestion that establishing a phenomenon should precede explaining a phenomenon seems moot when applied to the discourse on medical problems involving significant social controver-

sies. In the case of AIDS, the possibility of disease transmission, the magnitude of the suffering, and the necessity of defining risks and risk groups in order to predict, detect, and prevent the disease were more than likely exigencies that forced Gottlieb et al. and Siegal et al. to explain the problem before there was a consensus about its existence. It is possible that in biomedical discourse in general, explaining a problem is actually part of the process of its establishment. In epidemiology, for example, determining causes of disease, which helps to establish risks and risk groups, might naturally coincide with the characterization of the effects. However, because of the socially volatile nature of AIDS, it would be wrong to generalize that the patterns of establishing and explaining the phenomenon are combined in most scientific papers.

The findings presented here, however, suggest that the two patterns of establishing and explaining the phenomenon require very different rhetorical strategies. In characterizing the immunodeficiency observed in their patients as a new medical problem, the writers of these early reports on AIDS demonstrated that *old* knowledge was inapplicable and that *old* diagnostic and treatment procedures resulted in failure; through descriptions of disease progression, they constructed a context of mystery and surprise. But in their efforts to explain the condition, the writers applied disciplinary knowledge of infection rates in homosexual males as well as more implicit assumptions about the dangerous nature of male homosexual behavior to make their propositions seem self-evident.

The explanations of disease etiology in these reports derived from a complex interplay of prevailing social assumptions, disciplinary knowledge, and first-hand experience. The presumed link between what was considered specifically male homosexual behaviors and AIDS helps to explain why early researchers failed to recognize the danger to blood transfusion and blood product recipients, sexual partners of AIDS patients, and babies born to infected mothers. However, the etiological claim linking homosexual activity and disease can be viewed as the result of both cognitive and social processes that are common in theory construction. Though found later to be inaccurate, the hypothesis that acquired immunodeficiency was caused by some factor in homosexual behavior reveals the connection between beliefs about male homosexual behavior, on one hand, and knowledge and experience of homosexual diseases, on the other. According to Fleck (1979), scientists usually begin with prevailing social attitudes and

assumptions: "From false assumptions and irreproducible initial experiments an important discovery has resulted after many errors and detours" (p. 76). The implication is that discovery would be impossible without the errors and false starts that result from mistaken beliefs.

Rather than inhibiting progress in AIDS research, false starts, such as these early hypotheses about AIDS etiology, may have actually stimulated the conflict necessary for more substantive explanations. As Fleck argued: "When two ideas conflict with each other, all the forces of demagogy are activated. And it is almost always a third idea that emerges triumphant: one woven from exoteric ... and controversial strands" (p. 120). Rather than placing blame on the practitioners who first treated patients or researchers who carried out the first investigations, we should view the early explanations of AIDS as illustrations of how belief, experience, and documented knowledge are linked in scientists' efforts to construct theories and then argue the validity of those theories.

Likewise, it would be injudicious to argue that these writers were unconscious of the implications of the language they employed. Sontag (1988) implied that those who characterized AIDS and its risk groups used language without being fully aware of its ideological and social ramifications. However, it is also possible to argue that those first announcing the discovery of a new form of immunodeficiency consciously chose language that underscored the magnitude of the problem. In these early medical reports, the writers faced the rhetorical problem of establishing the new phenomenon so that the medical community would comprehend the full potential of the disease as a deadly public threat. The depiction of AIDS as inevitably fatal and mysterious worked rhetorically to recruit the research community to solve an urgent problem.

Perhaps with diseases like AIDS, writers are, in fact, more concerned with, as Sontag (1988) suggested, gaining "rhetorical control over the illness," in gaining control over "how it is possessed, assimilated in argument and in cliché" (p. 94). Sontag's purpose is to challenge this process. However, our purpose as rhetoricians should be to try to *understand* the process. We need a thorough understanding of how rhetorical patterns contribute to the identification of research problems and the creation of disciplinary consensus. We also need to identify generic as well as discipline-specific patterns in order to lay the groundwork for a truly cross-disciplinary rhetorical theory.

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