

Studies in the rhetoric of science have tended to focus on classic scientific texts and on the history of drafts and the interaction surrounding them up until the moment when the drafts are accepted for publication by a journal. Similarly, research on disasters resulting from failed communication has tended to focus on the history of drafts and the interaction surrounding them up until the moment of the disaster. The authors argue that overattention to the moment skews understanding of what makes scientific discourse successful and neglects other valuable sources of evidence. After reviewing the promises and limitations of studies from historical, observational, and text-analytic approaches, the authors call for studies of responses to research articles from disciplinary readers and argue for studies using a variety of qualitative and quantitative methodologies that will explore the real-time responses of readers to scientific texts, test the effects of rhetorical strategies on readers, and track the course of acceptance or rejection over time.

Moving beyond the Moment Reception Studies in the Rhetoric of Science

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Rhetoricians of science are fascinated with the moment. The explosion of interest in the rhetoric of science in the mid-1980s began with historical studies of great moments in science: important scientific discoveries or the writings of renowned scientists. Although some scholars have examined what Thomas Kuhn calls “normal science” (5), or the discourse of day-to-day scientific work, even these studies tend to focus on the moment—the moment when an article is accepted for publication by a journal and the history that leads up to that acceptance.

Although this focus has led to important and interesting insights into the writer’s intentions and choices as a text is produced, it begs the question of how the text is received by its readers. Few rhetoricians these days would assume that scientists accept whatever is published in journals as unvarnished truth, but our research gives us little insight into the nature of acceptance. Does acceptance occur in grad-

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tions? Does acceptance change over time? How is acceptance related to rhetorical strategies in the text? And how do accepted texts influence the course of science? We are not the first to raise questions such as these. In 1993, Dilip Gaonkar revived charges that Trevor Melia had made, more than 10 years earlier, that rhetoricians have yet to show any clear relationship between rhetoric and the course of science. Melia noted a contradiction in Kuhn's view of how converts are won to new paradigms during scientific revolutions. Although Kuhn describes scientific revolutions as times of persuasion, he also contends that converts are won through a shift in gestalt rather than through rhetorical arguments. To find out more about whether rhetoric changes the course of science, Melia called for case studies. Yet, despite more than 10 years of case studies, most of which have focused on scientific revolutions, Gaonkar is still not satisfied that any effect of rhetoric on science has been demonstrated. (For a more complete discussion, see Gross and Keith.)

Similarly, Steve Fuller calls for more attention to audience reception when he argues that many rhetorical analyses of science are self-serving, actually demonstrating how cleverly rhetoricians can read texts rather than how scientists read them. Therefore, in Fuller's view, the analyses serve to legitimize rhetoric rather than increase our understanding of science. Unless rhetoricians reform their practices, Fuller argues, the rhetoric of science will become "a field whose practitioners trade intuitions about the efficacy of devices rather than, say, the validity of arguments" (310).

We contend that focusing on the moment devalues or circumvents investigations of the effects of rhetorical strategies. Despite the availability of various methodologies, few studies give more than a cursory glance at the audience's response to a text at any point in time. The dominance of a moment-based focus is clear in Randy Harris's 1997 collection, *Landmark Essays on Rhetoric of Science: Case Studies*. Only one of the 11 articles deals substantially with responses from audience members. Ironically, we focus on audience when we advise students in writing classes and when we promote user-friendly document design to industry, but when we act as scholars, we focus primarily on the text or the writer and ignore the audience's response.

When we do consider the audience's response, such consideration is often only for the sake of choosing which texts are worthy of analysis. Too frequently, texts are chosen because they have been acclaimed for their contributions to the world's knowledge; they are deemed great works in the canon of scientific literature. Their contemporary

status as revolutionary texts is attributed backward into their histories without sufficient examination of their initial effect on their original audiences and how that effect may have changed over time. In such acclaimed cases, the contemporary status of the texts serves as a self-fulfilling prophecy for the effectiveness of particular rhetorical strategies: If a work is successful, then any aspect of it may be assumed to be effective a priori. The converse is perversely similar. Scholars who study disasters, such as the meltdown at Three Mile Island or the explosion of the shuttle *Challenger*, would seem to need to focus on the effects of particular rhetorical acts on readers (Arnold and Mailey; Dombrowski; Farrell and Goodnight; Gouran, Hirokawa, and Martz; Herndl, Fennell, and Miller; Pace; Winsor, "Communication"). However, studies of disasters are largely the flip side of studies of scientific revolutions, focusing on the catastrophe rather than on the moment of utterance. Scholars who study disasters know that the rhetoric failed and deliberately search for causes of that failure. The problems with this kind of reconstruction are examined by Dorothy Winsor ("Construction") and by Alan Gross and Arthur Walzer. In either case, the audience's response acts as a confirmation of the textual analysis rather than as a measure of the text's rhetorical efficacy.

To move beyond the moment, we need to find ways to gauge the effects of normal scientific texts on readers when they are first published, watch acceptance or rejection unfold over time, and associate those effects reliably with rhetorical strategies in the texts. This kind of research is essential for critiquing the power and uses of scientific rhetoric in any serious way. Otherwise, we may fall prey to radical critical theories that derive scientists' power solely from complicity with unjust social structures (Charney, "Empiricism"). Furthermore, only by understanding the complex mechanisms of scientific rhetoric and all its available means of persuasion can we help citizens become active participants in public debates bound up with the discourses of science.

To argue for more research on the reception of contemporary science over time, we begin by outlining the problem with relying wholly on the current historical status of the scientific texts as a measure of audience reception or success. Then we consider why the recent addition of observational and formalist studies has not alleviated the problem. Finally, we look at the prospects for new approaches that triangulate traditional methods with experimental and diachronic studies. We review a small set of promising studies in

this vein and end by sketching the kinds of research methods that can and should be used.

THE PROBLEM WITH THE CURRENT HISTORICAL STUDIES OF TEXT

Close analyses of specific scientific texts over the past 20 years have yielded valuable insights into the array of rhetorical strategies that scientists can and do employ. In looking toward future prospects for research, we argue that textual analyses of historically important texts are not enough. Such analyses can tell us what rhetorical strategies have been employed but not why scientists use them or what effects they have on readers.

The texts most often studied by rhetoricians of science are those that foment scientific revolutions, such as those by Charles Darwin, Isaac Newton, and James Watson and Francis Crick. Treating these texts as best practices may seem reasonable because revolutionary arguments not only overcome great resistance but also prove to be of lasting validity. Many studies of revolutionary rhetoric use the current historical status of texts not only as a method for selecting the text but also as a measure of its success. The effectiveness of the discourse is not often judged or measured. It is assumed. Instead of looking at the audience's reception of the text, scholars focus on the role of the text or the scientist in bringing about the revolution. Given this focus on production, discourse analyses of historically important texts (i.e., historical studies) cannot examine the long-term effect of the rhetorical features discussed or the contribution of these features to the success of the articles because these analyses provide no clear standard for success or effectiveness, leaving them vulnerable to the risks of relativism, as well as no clear standard for failure.

No Clear Standard for Success

Analyses that rely on the current historical status of a text as a measure of success do not provide a clear standard for success. First, the status of a text is never static. Second, a text is considered to be a success in science if it has achieved communal acceptance, which is demonstrated by the accrual of prizes, citations, research funding, or references in textbooks. Although these measures indicate a historical

status of sorts, acceptance is frequently gradual. Because the status of a particular text evolves over time, pinning down when success occurs or gauging relative success is difficult. The histories of the most obviously canonical scientific texts—the works of Darwin and Newton, for example—make that clear. The current scientific acceptance of, say, the theory of natural selection is not the same as contemporary acceptance of Darwin's *Origin*. John Campbell has argued that Darwin's rhetoric was key to his success. However, as Fuller points out, if we ask whether Darwin persuaded his readers, considering "the British response to *Origin* in its first decade of publication, the answer is a resounding, no" (308). In a similar vein, comparing Newton's 1672 article on optics to his 1704 *Opticks*, Alan Gross argues that Newton over time developed a rhetoric of continuity that was more effective than the rhetoric of revolution that he used in the earlier article ("Shoulders"). However, Gross's account does not consider that in the 30 years between the two publications, the earlier article had endured close scrutiny, Newton had established himself by other means as the expert in optics, and many of his detractors had died.

How confidently may we attribute the more favorable reception of *Opticks* to Newton's changes in rhetorical strategies? How confidently may we say that the later rhetorical strategies caused a more favorable reception rather than were the product themselves of a more favorable climate? Unless we agree on how to measure contemporary communal acceptance and when acceptance is to be measured, we cannot use the current historical status of a text as a clear standard of success.

No Clear Standard for Effective Rhetoric

Analyses that rely on the current historical status of a text as a measure of success do not provide a clear standard for effective rhetoric. Reasoning about textual features—such as distinguishing which rhetorical strategies are effective and which are not—is too often colored by prior assumptions about the success or failure of the text. The problem is that scholars too easily assume that all the features of successful texts are effective and that none of the features of unsuccessful texts are.

The problem is illustrated in Michael Halloran's pioneering rhetorical analysis of Watson and Crick's 1953 article on the structure of DNA. Halloran chose to date "the birth of molecular biology" from the publication of Watson and Crick's article rather than from Oswald

Avery, Colin MacLeod, and Maclyn McCarty's 1944 article that showed that DNA is the genetic substance. Halloran based his decision on the fact that Avery, MacLeod, and McCarty's work did not have an immediate revolutionary effect, whereas Watson and Crick's did. For Halloran, the styles of the two articles illustrate two kinds of scientific ethos, one brash and confident (Watson and Crick) and one painstaking, impersonal, and tentative (Avery, MacLeod, and McCarty). Although Halloran acknowledges that Avery, MacLeod, and McCarty's article may have been "premature" (76), he also argues that Watson and Crick's ethos intensified the acceptance of their work. He even speculates that their success inspired a distinctive new ethos among molecular biologists. The strong implication is that Watson and Crick's rhetoric helped to create the moment, or *kairos*, in which their text influenced the world and that Avery, MacLeod, and McCarty's might have done the same.

Halloran implies that Avery, MacLeod, and McCarty's work had less immediate impact because of the scientific ethos that they adopted. By implication, a cautious, reserved style is deemed ineffective. But is that a fair judgment? Whatever the merits of their style, Avery, MacLeod, and McCarty's article was not overlooked; in fact, it was considered highly controversial at the time (Lederberg). To be deemed controversial is not a bad fate for a scientific article; a worse fate is to be disregarded. And although they were never awarded a Nobel Prize, Avery, MacLeod, and McCarty did receive due recognition. They received the Copley Medal from the Royal Society in 1945. Their contribution to the understanding of DNA is now routinely mentioned in popular histories as the immediate precursor to Watson and Crick's findings (Eckhardt; Shreeve). If Avery, MacLeod, and McCarty's rhetoric were ineffective, we would not expect it to have received such recognition. If this recognition came despite defective rhetoric, then we might be forced to conclude that in the long run, the rhetoric was largely irrelevant. Alternatively, we might reconsider whether anything is distinctive about Avery, MacLeod, and McCarty's style. Perhaps it seems cautious only in relation to Watson and Crick's excessive breeziness; what Halloran sees as cautious might seem normal in relation to other scientific articles. If in fact Avery, MacLeod, and McCarty's style seems cautious even for scientists, then whether this style was inappropriate for their situation is worth considering. Considering that Avery, MacLeod, and McCarty's

article was taken as controversial despite its cautious style, we speculate that the article may not have been considered publishable if it had been written in a bolder style.

Lawrence Prelli makes similar judgments of the effectiveness of Watson and Crick's article in a more detailed analysis of their line of argument. Prelli praises Watson and Crick for choosing rhetorical strategies so "wisely" and "astutely":

My rhetorical analysis has shown that the influence of the article was not simply the result of its bold ideas. Astute rhetorical decisions entered in, several times very consciously. Watson and Crick displayed consummate rhetorical skill: it was a virtuoso performance in scientific rhetoric with power to compel an authorizing audience's immediate, appreciative understanding, if not their immediate full conviction. (249)

Does any evidence exist that readers of Watson and Crick's article actually felt compelled to give immediate, appreciative understanding? Had Watson and Crick been less astute and made different rhetorical choices, would the double-helix model have been rejected or disputed? Did all of Watson and Crick's choices contribute equally to the overall effectiveness of the text? Did they miss any opportunities to be even more effective? Neither Halloran nor Prelli considers such questions. Clearly, studies that rely on the current historical status of a text as a measure of success give little basis for measuring the effect of rhetorical strategies.

Risks of Relativism

Without independent means to measure either success or effectiveness, discourse analyses of historical texts may devolve into a form of relativism that endorses whatever choices a scientist makes as appropriate to the unique situation.

Prelli acknowledges that scientists have an array of rhetorical strategies at their disposal and that their choices are constrained both by the historical situation and by their methodologies. When comparing Watson and Crick's article on DNA with Rosalind Franklin and R. G. Gosling's article in the same issue, Prelli attributes differences in their choices to constraints imposed by methodologies. As experimentalists, Franklin and Gosling chose an evidential framework, whereas theorists Watson and Crick chose an interpretive one. Both frames are deemed appropriate. However, Prelli is noticeably

less enthusiastic about Franklin and Gosling's choices than Watson and Crick's. Does he have a basis for judging Watson and Crick to be more astute rhetoricians?

Carolyn Miller considers this issue in her reanalysis of the DNA case by introducing more clearly the external disciplinary situation in which Watson and Crick's and Avery, MacLeod, and McCarty's articles were written. Miller argues that neither Avery, MacLeod, and McCarty nor Watson and Crick fully controlled their "kairos." Avery, MacLeod, and McCarty faced a situation in which promoting the importance of DNA was "premature," but Watson and Crick faced a situation in which the question of DNA was "overripe"; scientists were actively seeking this particular piece of the puzzle. Miller argues that in their situation, Avery, MacLeod, and McCarty's cautious tone was more appropriate to their time, when "anomalies were just beginning to be noticed against a framework of fairly solid expectations," whereas Watson and Crick's confident tone was appropriate for their time, when an "explanatory synthesis was awaited" (318). If each tone is fully appropriate to the situation, then judging effectiveness becomes even more difficult. If every writing situation is unique, can a published scientific article ever be judged as rhetorically deficient?

Miller's analysis points to some external disciplinary conditions that might be considered in judging a scientist's choices. We might draw from Miller's analysis two hypotheses: (1) Brash confidence is effective in synthesis-quest situations but not in anomaly-introduction situations, and (2) cautious reserve is effective in anomaly-introduction situations but not in synthesis-quest situations. The articles concerning DNA might be used as a partial test of these hypotheses. Avery, MacLeod, and McCarty wrote in a period of anomaly introduction. Were any brash articles written about DNA from that period? If brashness is really ineffective in the anomaly-introduction period, then such manuscripts might never have been published, one reason why confining scholarly attention to published articles is a serious limitation. At the later point of synthesis quest, however, more than one article on DNA is available. Watson and Crick's article has been characterized as brash. What about Franklin and Gosling's article? Is it bolder than normal? The key comparison would be of Avery, MacLeod, and McCarty's text with that of Franklin and Gosling. Both texts are constrained by methodology, in Prelli's sense, to an evidential framework, but they differ in disciplinary situation, in Miller's sense. Do Franklin and Gosling take advantage of the synthesis-quest situation as an opportunity to display greater boldness than Avery,

MacLeod, and McCarty display within the bounds of an evidentiary style? If they do, then we might have some basis for judging that all three articles employ some astute rhetorical strategies. We hope further research closely compares these texts. But would that research have strong grounds for claiming that these strategies influenced readers?

Claims about rhetorical effectiveness are hollow without more precise definitions of what succeeding, falling short, or failing means for a scientific text. Although the situational nature of rhetoric always leaves it open to charges of relativism, finding and applying guiding principles are also central to the study and practices of rhetoric. Without considering the readers' reception of a text, we risk our research being dismissed as contradictory and/or ineffectual—such as the charges made by Gaonker—and we risk our ability to provide principles for practice for our students.

No Clear Standard for Failure

The same kinds of critique that we have just been making for articles in the scientific canon also apply to historical studies of disasters. Several analyses of the meltdown at Three Mile Island or the explosion of the shuttle *Challenger* (Arnold and Mailey; Dombrowski; Farrell and Goodnight; Gouran, Hirokawa, and Martz; Herndl, Fennell, and Miller; Pace; Winsor, "Communication") attributed the disasters to ineffective rhetorical features in the memos and reports that led up to the incidents. The rhetorical strategies must be deficient because they failed to prevent the disaster. But how confident can we be that the outcome would have been different if the texts had been different? These studies provide no clear standard for failure.

In her thoughtful second take on the *Challenger* accident, Dorothy Winsor ("Construction") reconsiders whether the texts or even their readers were to blame. Winsor argues that even though engineers saw the evidence that we now know to be critical and passed it on to their managers, neither the engineers nor the managers seemed to know that it meant the launch would fail: "Data do not automatically produce knowledge, even for the persons originating the data" (15) because "knowledge is always shaped by both empirical evidence and social, contingent factors" (12-13). As plausible as this analysis is,

it leaves us in an uncomfortable position. If the engineers were not bad rhetoricians and the managers were not necessarily venal or stupid—in other words, if both engineers and managers were seeing the world and reasoning and communicating about it as their social environment predisposed them to do—then how can rhetorical strategies ever have an effect?

The problem here, as in success stories, is that specific rhetorical strategies have only been examined in the context of texts relating to the disaster. To investigate whether rhetorical strategies have a predictable effect, their occurrence in other situations must be examined. If strategies identified as possible causes of communication problems in the *Challenger* incident are found to be ineffective in general for convincing a reader that a problem is serious, then these strategies should also be associated with other situations in which recommendations are rejected. We would like to see researchers analyze a wide array of memos and reports used in day-to-day noncatastrophic decision making in which some recommendations are accepted and others rejected. In this kind of study, researchers might investigate whether certain rhetorical features show up more often in texts leading to rejected recommendations than they do in accepted ones.

Discourse analyses of important historical texts have been extremely valuable in demonstrating that scientists employ a wide array of rhetorical strategies. Many of these historical studies appeared at a time when that fact in itself was considered revolutionary. However, analyses that focus on historical texts alone cannot help us gauge rhetorical effects. They do not provide a clear standard of success. They cannot answer important questions about the effects of individual rhetorical strategies. They give us little basis for teasing apart the choices of the writer from the situational constraints. Although careful historical reconstruction can help us to understand more about the receptions of historical texts, in the remainder of our article, we focus on contemporary scientific texts to avoid post hoc assumptions and to have access to actual readers.

THE LIMITS OF OBSERVATION

When close analysis of textual strategies is combined with observational and ethnographic studies of scientists as they produce texts, more valuable insights often result. Studies of scientists as they pro-

duce articles (e.g., Berkenkotter and Huckin; Blakeslee; Law and Williams; Myers; Rymer) analyze a sequence of drafts and note the changes that authors make on those drafts both in response to their own rereading and to feedback from peers. These studies illustrate how scientists lay the groundwork for written discourse through informal venues; make decisions about organization, use of citations, and terminology; respond to feedback from colleagues and reviewers; and so on. Because these studies have often focused on scientists in the midst of ongoing projects, they provide some corrective to the post hoc analysis of revolutionary successes and dramatic disasters. And because these studies include a greater variety of discourses than does the published text (including conversations between coauthors and responses from reviewers), these studies also provide insight into the array of choices that scientific writers consider. Overall, these studies provide strong evidence that many scientists are acutely aware of their audience as they write and call on an array of strategies that they think will appeal to their readers. In looking toward future prospects for research, however, we argue that observation of individual scientists is not enough. Such analyses can tell us more about why scientists decide to use certain rhetorical strategies but cannot tell us what effects these strategies eventually have on readers in the discipline at large. These studies focus on the production up to the moment and not the subsequent reception of the text.

In his influential studies, Greg Myers analyzes the draft-by-draft histories of proposals, journal articles, and popularizations. He observed two biologists writing research-funding proposals. The two biologists, Dr. Crews and Dr. Bloch, displayed similar concern for engaging their readers but faced quite different challenges in justifying the importance of their work. Crews, whose proposal was eventually funded, requested an extension for an ongoing project and needed to convince readers that it was more than just the same old thing. Bloch, whose proposal was not funded, was starting up research in a new domain in which he had not previously published and needed to convince readers that he was not an upstart interloper. Myers notes that their situations called for different rhetorical strategies: "The cautious tone adopted by Dr. Bloch, appropriate for his situation as a newcomer, would be disastrous for Dr. Crews, who is well established in his specialized field" (240). Myers finds both approaches appropriate, a conclusion that he supports with evidence that the developing argumentative approaches lessened the resis-

tance of the reviewers. In fact, Myers believes that Bloch's caution was "the only strategy that would have had a chance" (236).

Myers's approach provides an important corrective to the historical standard of success. He recognizes that a writer's choices may have been rhetorically astute even if the proposal is rejected for now. He also notes that scientists can take other steps to improve their next textual gambit, such as giving talks at conferences and extending their research to address objections. But Myers also leans toward a relativism that makes gauging degrees of appropriateness difficult:

If the rhetoric of the proposal is not given by some ideal list of persuasive or communicative techniques, or by an ideal scientific persona, or by the characteristics of the project itself, but instead depends on a complex process involving both the researcher and the discipline, then it will vary with each discipline and with the writer's relation to the discipline. (240)

Surely we can say more than "be a good rhetorician" and still stop short of promoting fixed formulas or guidelines for effective rhetoric. For instance, analyzing and comparing some of the situational variables that have been identified in case studies and historical analyses would be useful. The term *caution*, for example, has been used to describe the rhetoric of texts both in Myers's description of Bloch's "situation as a newcomer" and in Halloran's depiction of Avery, MacLeod, and McCarty's introduction of an anomaly. Does caution look the same in both situations? What other situations arise that might lead to a common set of rhetorical strategies?

We should also press further for some means to gauge whether a scientific text is appropriate and whether it falls short or fails. Although Myers certainly would characterize the biologists' early drafts as less effective than the later ones, he provides no independent means of assessing the final proposals against other proposals. Despite the fact that, in our role as writing teachers in technical writing classes, we routinely assign grades to papers about science and engineering, scholars in the rhetoric of science seem unwilling to pass negative judgments against the writing in published articles. Few scholars have considered what happens after an article is published, as if publication is a sufficient sign of rhetorical success—a problem we discuss further in the next section.

Carol Berkenkotter and Thomas Huckin's observational study of a biologist does provide an analysis that is somewhat critical of the biol-

ogist's rhetorical skills. But even they focus their criticism on early drafts of an article and not the final, published draft. Berkenkotter and Huckin observed and interviewed June Davis, a biologist who researched how *Candida albicans* (a yeast involved in toxic shock syndrome) affects a clotting agent in the blood and whether it increases the tumor necrosis factor (TNF). As Berkenkotter and Huckin point out, in her first submission to a journal, Davis motivates her research in the introduction only with references to her own previous work:

We have previously found that a small dose of *Candida albicans* which had little adverse effect by itself, acted synergistically with *Staphylococcus aureus* to cause shock and death in mice (3 [self citation]). While attempting to identify the role of *C. albicans* in the *C. albicans/S. aureus* synergism, we had found that *C. albicans* alone at low doses which have no effect on a variety of blood parameters tested did elevate plasma fibrinogen levels (unpublished). In this study the ability of small doses of *C. albicans* to induce changes on blood chemistry and hematology and the role of tumor necrosis factor (TNF) in these changes were examined. (Berkenkotter and Huckin 51)

Davis seems to have assumed that describing her local experiences was sufficient, but the journal reviewers asked her to set her findings into the context of related studies. Davis rewrote the introduction, commenting that she would have to add in a "phony story" to accommodate the fact that "reviewers always expect you to say certain things" (Berkenkotter and Huckin 55-56). Davis seems to assume that the occasion for writing and the moment of discovery in the lab are one and the same—that the audience receives rather than participates in the production and reception of the text. However, she did follow the reviewers' suggestions, and she revised the introduction. Berkenkotter and Huckin see Davis's reaction to the reviewers' suggestions as anomalous compared with other scientists whose grasp of the rhetorical situation is more savvy (such as the scientists who read Davis's draft and scientists observed by Myers and others). For rhetorically savvy scientists, the world extends beyond the lab. It begins with a community of research and ends by reemerging individual experiences back into that community: It situates the microcosm of lab findings in the macrocosm of the discipline and its accumulated knowledge. Berkenkotter and Huckin agree; the revisions made Davis's text stronger rhetorically, as can be seen in her fifth, and final, draft:

We have previously found that a small dose of *Candida albicans* which had little adverse effect by itself, acted synergistically with *Staphylococcus aureus* to cause shock and death in mice (3 [self citation]). The present study was undertaken to determine how *C. albicans* contributes to this lethal shock synergism. It has been reported that induced tumor necrosis factor (TNF) is responsible for endotoxic shock in the mouse (2). Because *C. albicans* and endotoxin share a number of characteristics (for a review see 15) candidal-infected mice were examined for induced TNF. It is reasonable to suspect that *C. albicans* could induce TNF *in vivo* because it has been reported recently that *C. albicans* induced TNF production *in vitro* by human monocytes and natural killer cells (9). TNF has also been shown to potentiate the fungicidal activity of human neutrophils *in vitro* against *C. albicans* (10, 11). As exogenously administered TNF is known to induce acute phase proteins such as fibrinogen (12), plasma fibrinogen in infected mice was also measured and the role of TNF in the fibrinogen increase investigated. (Berkenkotter and Huckin 56)

Berkenkotter and Huckin consider Davis's final draft successful because it tells more of a story—incorporates more of a chronology—about her research. Compared with her earlier draft, the final draft had improved and seems to have satisfied the next set of journal reviewers. But is the fact that it was published sufficient warrant to consider it successful? In the end, did Davis approximate the strategies of a skillful rhetorician?

We think not. In fact, Davis seems to have made the minimal number of changes necessary to appease the reviewers, without altering the aim of her first draft—namely, to recount and justify her experience in the lab. The final draft is still rather myopic—it still begins with Davis's own timetable: first what she did in previous experiments and then what she did in the current experiment. It still contains very little by way of a research history outside of Davis's own previous work. It makes no reference to the current state of knowledge in the community or the importance of the additional information that her study will offer. In essence, she retains the spirit of her first draft, if not the letter. But on what basis can we critique Davis's final product? In their critique, Berkenkotter and Huckin may have been constrained by their observational approach, which aims to recount accurately a specific case. They focus on the arc of Davis's progress rather than on where she ends up relative to her peers. Although descriptive studies of the production of the text up to the moment of acceptance or rejection from a journal can tell us about the development of the process, they cannot tell us about the quality of

the product because they do not gauge the ultimate impact of a text on its readers. They simply do not provide a basis for judging a text.

THE LIMITS OF FORMALIST STUDIES

One independent basis for judging a text is by comparing it to practice that is considered either normal or ideal. We have been reluctant in composition studies to hold up certain kinds of standards, perhaps out of an unwarranted fear of reductivism or perhaps out of experience with how norms can be turned into rigid rules. We believe that no approach in pedagogy or in research is ideologically safe from misuse; to avoid the limitations of one approach, supplementing it with another approach that is limited in its own different way is often productive. Studying normal practice can provide valuable historical insights as well as tools for gauging individual efforts.

Charles Bazerman examines Newton's development of a methodology section in his early optics reports and then traces changes in methods sections over the 300-year history of the research article genre. In Bazerman's formalist analysis of more than 100 articles, textual choices are placed in social, historical, and rhetorical contexts. More recently, Dwight Atkinson has taken on a similar project of examining the history of the Royal Society, by examining the changes in rhetorical strategies, linguistic features, and content of the *Philosophical Transactions of the Royal Society* over time. These studies focus on the development and acceptance of the form rather than the textual features of arguments in particular research articles. But they are valuable for showing that conventions grow from the needs of a community and are not imposed arbitrarily.

The important formalist work of John Swales shows how structural conventions fulfill a community's need. Because of this work, the introduction section of research articles has become one of the best-known structural features in scientific discourse. Swales's Create A Research Space (CARS) model is based on reviews of hundreds of research articles across several disciplines. Swales found that writers typically make three moves:

Move 1. Establish a territory by claiming the issue is central; by describing the state of existing knowledge on the issue; and by detailing some particularly relevant research.

Move 2. Establish a niche by raising questions, uncovering a gap, counter-claiming, or extending a tradition.

Move 3. Occupy the niche by overviewing the goals and procedures of the current project and how it answers questions, fills gaps, tests conflicting claims, or provides a next step. (141)

Although Swales's moves may seem formulaic, using them effectively requires considerable rhetorical skill. Establishing the territory serves to interpret and instruct as well as to remind; writers draw selectively on published work to create a suitable history for their project. These histories are shaped by responses to peer reviewers and a variety of other communal pressures (McRoberts and McRoberts; Law and Williams). The review of relevant literature must strike knowledgeable readers as fair; it must characterize the goals and status of the established work accurately. But it must also promote some aspect of the previous work that will be central in the new work. The uncovering of the gap also requires some skill. Writers must identify themselves with the previous work while critiquing its comprehensiveness or quality strongly enough that readers will believe the gap is worth filling.¹

If we reconsider biologist Davis's final introduction (quoted earlier) in light of Swales's moves, we can see where she departs from the conventions. Davis seems to use these moves but not precisely. Her first three sentences qualify as a discussion of previous research but provide neither generalizations about the current state of knowledge nor centrality claims about the importance of the topic. In these sentences, she mentions only two earlier studies—one of which is her own past work—thereby establishing an extraordinarily small territory in which to interject her research. Then Davis explains what she chose to examine next and why this step is "reasonable." This material resembles Move 3, describing how the current work occupies the niche. But at no point does Davis use one of Swales's options for creating a niche. We suspect that as a result of Davis's departures from the norm, scientists accustomed to reading more conventional introductions might well overlook the potential importance of her contribution. However, without studying readers' responses or citation studies, we cannot gauge these effects. To their credit, Berkenkotter and Huckin do employ what they call "penumbral readers" (48), scientists who read and comment on Davis's text. Although the insights of these readers are valuable (and generally consistent with our view that Davis's

introduction does little to spark interest in her work), a larger-scale look at readers' responses is necessary.

Formalist approaches by themselves cannot address a writer's motives and only allow one to judge a text in terms of whether it follows the conventions. So combining formalist insights with observational studies would be valuable. Berkenkotter and Huckin's observations of Davis's drafting process show us that the final version took haggling and negotiating; we know that the end product was a compromise, of sorts. But neither method gives us insights into whether Davis's departures from the norm ultimately make a difference. Both formalist and observational studies provide insight in understanding the development of scientific texts, in terms of the production of either a particular text or of particular textual features. However, neither gives insight into their reception. In the next section, we discuss how reception studies can transcend this particular limitation.

TRANSCENDING THE LIMITS: STUDIES OF RECEPTION

Over the past 20 years, textual analyses of historically important texts and of developing scientific genres as well as observational studies of writers have provided great insights into the rhetoric of scientific discourse. Although we have suggested ways in which these kinds of methods might be taken further and combined, we believe that they do not address the question of rhetorical effect. Only methods that actually consider and measure communal acceptance can provide more information.

Given the complex nature of communal acceptance in science, we can understand why few have as yet explored the relationship between rhetoric and revolutions that Melia first questioned. Many methods exist for measuring communal acceptance, and each method reveals different aspects of that acceptance. For these reasons, we believe that the best approach is to combine methods. We also believe that examining instances of contemporary science is important—to have access to actual community members and to avoid post hoc evaluations of texts. A multimethodological approach to contemporary texts is essential to investigate textual changes and their reception on various levels—both by individual scientists and by the community as a whole—over time. We see a particularly urgent need for several methods that are underused: correlational studies, textual responses,

observational studies of readers, diachronic studies, and experimental studies.

Correlational Studies

Correlational studies can be used to associate the intensities of any variables that can be described with frequencies or scales. These methods have been widely used in more quantitative areas of communication studies. Rhetorician David Kaufer teamed up with sociologist Kathleen Carley to explore, in effect, what a scientist can do within a journal article to boost its potential impact on a community. For a set of research articles in sociology, Kaufer and Carley showed that an article's "reach" (as measured by the number of times it is cited in later articles) can be predicted in part from the number of articles it cites, the prominence of the cited authors, and the amount of elaboration about the cited articles (392). Other important factors in their model are the prominence of the authors of the article itself and of the journal in which it appears. The specific features that Kaufer and Carley used seem related to those in Swales's introductory moves for establishing a territory. Kaufer and Carley's model does not attempt to account for the essential quality of either the scientific work or the structure of the arguments, but their findings do suggest that rhetorical strategies make a difference to readers. Further research comparing the features of highly cited and not-so-highly cited articles is needed. The Institute for Scientific Information, which publishes citation indexes, has a Web site that provides information that may be useful in such studies, including pointers to particularly popular articles and rankings of influential journals in different scientific disciplines.

Textual Responses

Novel forms of direct response from readers have been evolving in the scientific literature that have not thus far been investigated by rhetoricians. For example, the journal *Behavioral and Brain Sciences* uses a system called open-peer commentary in addition to traditional blind peer review. Important interdisciplinary articles on a variety of topics related to psychology and neurology are selected through blind peer review and then are circulated to a large number of scientists who are invited to write 1,000-word commentaries (critiques, elaborations, cross-specialty syntheses, supplementary data, etc.).

The original target article appears in print along with 20 to 30 commentaries and the target author's response. A similar approach is used for major articles in the journal *Current Anthropology*. More open-ended forums for response from readers are also developing. For example, the Department of Philosophy at Brown University maintains an electronic journal called *BEARS* devoted to publishing brief reviews of recent articles on moral and political philosophy (see <http://www.brown.edu/Departments/Philosophy/bears/menu.html>). These new forms of exchange make certain kinds of academic debate accessible for analysis. We hope scholars will soon begin analyzing these commentaries as new and important sources of insight into the responses of readers, although responses composed for the purposes of publication are, of course, likely to differ from the moment-to-moment responses of ordinary readers.

Observational Studies of Readers

The effect of individual textual strategies on readers may be traced with observational studies, using think-aloud methods that may be combined with attitude surveys or comprehension and recall tests. Many of us who teach technical writing and consult in industry use techniques like these when user testing functional documents, but we have rarely used them to see how readers respond to argumentative texts.

Process-tracing methods can reveal the effects of specific textual features as well as the reactions of different kinds of readers. These methods can be used to test claims from rhetorical discourse analyses about the probable effects of specific textual features. For example, Gay Gragson and Jack Selzer wrote a reader-response analysis of Stephen Gould and Richard Lewontin's well-known article that critiques the adaptationist program in evolutionary biology. Gragson and Selzer argue that by means of an array of rhetorical strategies that were unlike those typically found in scientific articles, Gould and Lewontin had invited biologists to read unconventionally themselves. The rhetorical features included an unusual structural organization, use of nonscientific metaphors, references to art and literature, and an informal, irreverent style. To test whether scientists would actually accept this invitation, Davida Charney observed biologists reading the article and collected their responses by means of think-aloud protocols ("Study"). Concluding that these readers had not accepted Gould and Lewontin's invitation, Charney argued that

Gould and Lewontin's strategy assumed scientists would read linearly; however, the biologists, as per their usual habits, read nonlinearly, previewing the article and skipping around to check out hunches about Gould and Lewontin's points. Many of the readers were deliberately wary about Gould's well-known stylistic proclivities. Charney also observed that graduate students used different, less evaluative reading strategies than faculty members used. Charney's observations do not invalidate Gragson and Selzer's textual analysis, but they do remind us that the reader constructed by a scientific author may not correspond to actual readers.

Diachronic Studies

Although observational studies allow us to understand the effects of individual textual strategies at a particular time on a particular group of readers, diachronic studies can help us move beyond one particular moment of reception to explore how a community's perception of a text changes over time. As with Avery, MacLeod, and McCarty's article on DNA, which started out as controversial and ended up as a key discovery in our understanding of genetics, scientific research articles remain available for reconsideration and reevaluation. They may well change in importance and relevance over time as other studies become available and as theories develop. Diachronic studies that combine textual analysis with measures of communal acceptance—such as observation of readers and measures of reception—can explore how textual strategies that were crafted to meet one moment of *kairos* may affect readers at other times.

Diachronic studies are particularly useful for exploring the evolving nature of knowledge within a community and for examining the contemporary rhetoric of scientific revolutions. Most scientists today are well aware of Kuhn's concepts of normal and revolutionary science, and some seek to make deliberate use of the notion of revolutions and paradigm shifts. Those who would foment revolution, however, face the challenge of writing in genres adapted to the rhetoric of continuity. As described earlier, Swales's CARS model for research article introductions helps scientists to locate their research within the current scientific discourse, particularly with Move 1, which establishes the territory by pointing out the significance of the topic and reviews the current consensus. But revolutions break with the past. So how can scientists making revolutionary claims use the standard

strategies for opening an article? In one example of the use of diachronic methods, Danette Paul and Davida Charney explore how chaos scientists set up their introductions in early articles, trace how their introductory strategies changed over time, and examine how these strategies influenced scientific readers.

In the text analysis stage of their study, Paul and Charney analyzed the introductions of four articles written by two prominent scientists working in nonlinear dynamics, or chaos theory. They included an early seminal article and a later article by each scientist. The two early articles varied significantly from the CARS model. Move 1 was particularly different both in approach and in space. Both scientists dedicated more than half of their introductions to Move 1, with one article taking a much more brash approach than the other. However, these variations did not inaugurate a characteristic style for future chaos articles. The introductions of the later articles, written 10 to 15 years later, followed the CARS model, with only about one-third of the introductions devoted to Move 1.²

How did the generic pattern—and departures from it—affect readers? In the second stage of the study, Paul and Charney asked 12 scientists to think aloud while reading two of the articles. Generally, across articles, readers commented more on the shared information in Move 1 than on the new ideas being introduced in the later moves of the introduction. Readers also commented more while reading the later articles than while reading the earlier ones, commenting most and most favorably on the later article that most closely followed the CARS model. Both early articles had some features that struck readers as dated. When asked directly to compare the two early articles, the scientists liked the one with the brash approach, an article that was familiar by reputation to all the readers. They were somewhat dismissive of the other early article.

Recognizing that timing and the current status of the early articles might have affected their evaluations of the texts, Paul went on to trace the citation histories of these four articles and nine additional articles by the same scientists (“Citing”). She looked at both the number of citations and the content of the referring statements. Although all nine articles were somewhat successful (as compared with average rates of citations), the two early articles were phenomenally successful, even the one with a somewhat lukewarm reception from our readers. Moreover, the scientists who cited these articles seemed to be well aware of their importance, citing them in ways that maximized their rhetorical capital. Although these results do not indicate that our

readers' responses were wrong, they do suggest that individual readers' responses do not tell the whole story. Ideally, readers' responses might be solicited at various points after publication of an article. Triangulating a variety of measures using diachronic studies can provide a richer picture of success over time and how success is related to rhetorical strategies.

Experimental Studies

Until now, most studies of scientific discourse have focused on naturally occurring texts. Textual analyses focus on published scientific texts and other artifacts of the scientific process. Observational studies have focused on scientists writing their own texts or reading texts that might come to them as a matter of course. These studies have much to tell us about scientific rhetoric. But to learn more about the effects of different rhetorical strategies on readers, we should also conduct experiments that compare the effects of texts that differ in systematic ways.

Within the document design community, experiments have been used to test the effects of various textual features on readers, usually for the sake of making a text clearer and more comprehensible. Document design researchers have investigated whether some textual structures are inherently less clear, or harder to understand, than are others. They also investigated alternatives that they hoped would reliably make texts easier to read, understand, use, and remember. With methods drawn from psycholinguistics and reading development, researchers investigated the effects of textual features such as double negatives, hedges, and implicatures; typographic features such as sans serif fonts and upper case; and organizational structures such as conditional sequencing and headings (e.g., Crismore and Vande Kopple; Hartley and Sydes; Hartley and Trueman). For the most part, differences between readers and their purposes for reading (beyond literal comprehension) were not explored.

Such methods might be used productively to investigate the effects of rhetorical strategies on readers—on their willingness to keep reading, on their judgments of persuasiveness or interest, and so on. Experimental studies fell out of favor in the 1990s, partly because they led to guidelines that were considered either vague or officious and partly because of misplaced ideological concerns, as Charney has argued (“Empiricism”). Charney and Aimee Kendall are currently preparing a study that will follow up on Paul’s observation that arti-

cles with introductions that closely follow the CARS model may win over more readers than may articles that do not. We hope with this study, and others like it, to explore the limits of poetic license. The CARS model allows room for considerable variation, but some scientists, such as Davis, whom we described earlier, may not see the conventional practice as either real or necessary. By comparing readers' reactions to introductions that violate the moves to those introductions within the bounds, we may be able to see the added value for scientists of appreciating the rhetoric of their community.

CONCLUSION

In this article, we call for a more complex treatment of rhetorical success. Too often in rhetorical studies, *success* is defined as an absolute and immediate response. Yet, in the deliberative realms of government agencies, law courts, and corporations, decisions are continually revisited. A specific proposal for solving a problem may be rejected, but enough attention may have been drawn to the problem that some other action is taken. An attorney may lose a case in the first court of record but win on appeal—or even inspire legislation that ensures a different outcome for future cases. In scientific discourse too, a writer need not win every point in the line of argument to succeed. A scientist need not win over converts immediately or start a revolution to have an effect.

In a review of Jack Selzer's collection of analyses of the Gould and Lewontin article described earlier, Allan Gross commented on Charney's finding that scientists declined the text's invitation to read in unconventional ways ("Science"). For Gross, if rhetorically active readers do not follow the rhetorical strategies of the writers, then "those latter strategies must be failures," and any rhetorical analyses that "assume sequential reading are utterly beside the point" (182). We disagree. The readers whom Charney observed did not read linearly or give up a scientific outlook, but they certainly saw the point. A few noted the power of the unconventional strategies for making the critique. Most readers accepted most of Gould and Lewontin's points as valid but responded that the problems were not as pervasive or as harmful as Gould and Lewontin suggested. So although Gould and Lewontin did not convince biologists to abandon either their normal reading processes or their confidence in the adaptationist program, their article cannot be considered a failure. It is widely read and

widely cited. As Winsor noted in a citation analysis (“Constructing”), the article may well have heightened awareness of the dangers of certain kinds of biological explanations. And this article certainly was not Gould’s last chance to influence the course of evolutionary theory.

We believe that the only way to find out if rhetorical strategies matter to the course of science is to move beyond the moment and to study readers’ reactions over time. Readers are resilient. Writers do not succeed by turning readers into passive acquiescers—they succeed by advancing enough major claims that stand up against active readers’ challenges. To maximize the chances that their claims will hold up to the scrutiny of widely varying readers, rhetorically savvy writers are responsible for choosing as best they can from the available means of persuasion. Readers are responsible for assessing, as best they can, the merits of the case, which does not mean suspending critical faculties and following sheep-like in the writer’s train. Some of a writer’s gambits will succeed with some readers; some will fail.

Only through a combination of methods, rhetorical analyses, observations of readers, and citation analyses can we see an article’s actual effect. To be sure, these approaches we have recommended all have their own limitations. But so do all research methods. Historical methods are apt to overemphasize the moment, observational methods to overemphasize a particular author or reader, and formal textual analyses to overemphasize norms. But the limitations of one method may be overcome by complementary analysis with other methods. In practical terms, no one researcher can master all these methods. And no one study can include them all. Although we have critiqued a number of studies, our complaint is not that they fail to account comprehensively for all the factors of interest. Rather, our complaint is that as a research community, we have not followed up on the intriguing questions raised by these studies. Although every individual study may provide only a partial answer, as researchers, we can gain a more comprehensive understanding by using a wider array of methods. We encourage researchers of scientific discourse to employ multiple means to move beyond the moment.

NOTES

1. We believe that these moves do not constitute a phony story but rather an opportunity to do science. In the course of interviewing scientists engaged in chaos research,

Danette Paul heard a telling anecdote. One scientist, after hearing about the CARS (Create A Research Space) model, told Paul that he and his colleagues, who were trying to place a manuscript in a physics journal, had tried the "trick" of connecting their mathematical model with a physical system in the introduction. But when they engaged in the "trick," they discovered something new and valuable about their model. The trick actually produced science. This incident is reminiscent of an episode reported by Jone Rymer in which a scientist who had insisted that all the new ideas occurred before the writing began suddenly observed himself having a good new idea while writing.

2. Both Paul and Paul and Charney followed Swales's original model in which the establishment of the territory (what may be considered old information) was broken into separate moves for demonstrating interest and reviewing the literature. However, although they coded these moves separately, they often combined the results for these two moves in their discussion of results. So their approaches and Swales's have no practical differences.

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