

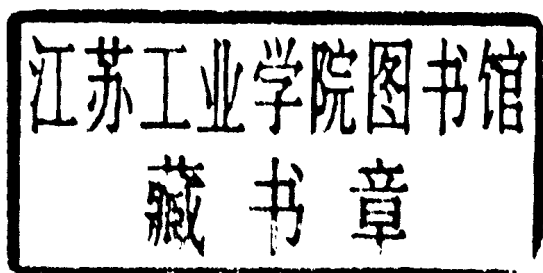
*the
rhetoric
of science*

ALAN G. GROSS

*with a new preface
by the author*

The Rhetoric of Science

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The Rhetoric of Science

To Suzanne

Preface: The Rhetoric of Science 1996

In 1962 Max Perutz won the Nobel prize in chemistry; in 1995, in the *New York Review of Books*, in a review of Gerald Geison's *The Private Science of Louis Pasteur*, he remarks in passing about the worth of rhetoric of science:

According to Geison, it is now a commonplace among historians and sociologists of science that science, no less than any other form of culture, depends on rhetorical skills. I have known scientists who possessed great rhetorical skills which failed to conceal the shallowness of their research from their peers. On the other hand, Alexander Fleming's lectures put everyone to sleep, while his discovery of penicillin made him one of this century's most famous scientists. Good research needs no rhetoric, only clarity. The entire approach emphasizing "relative" truth seems to me a piece of humbug masquerading as an academic discipline; it pretends that its practitioners can set themselves up as judges over scientists whose science they fail to understand. (p. 58).

This passage is interesting for two reasons. The first is the remarkable news of the commonplace status of a rhetorical perspective in historical studies of science. This represents a sea-change from the situation a decade ago. In recent years, historian of science Peter Dear edited a collection of essays entitled *The Literary Structure of Scientific Argument*, and philosopher Marcello Pera collaborated with historian of science William Shea on a similar collection entitled *Persuading Science*. Geison's reference just reinforces an existing trend. The second point of interest is Perutz's evident contempt for rhetoric of science as mere window-dressing. But rhetoric is more

than window-dressing; it concerns the necessary and sufficient conditions for the creation of persuasive discourse in *any* field. Science cannot be excluded by fiat.

In an *obiter dictum* incidental to a review of Marcello Pera's *Discourses of Science*, rhetorician Randy Allen Harris says: "I blame one of our own, Alan Gross—and especially his *The Rhetoric of Science*—for the lack of awareness most other scholars of science have of the work rhetoricians do in the analysis of science. It came with an authoritative title, it came from a large and respected publisher, and it purported to represent our field. If Gross had been more careful and insistent about incorporating the work of other rhetoricians, then philosophers and historians and sociologists who use it to peek in on us would have far less excuse for their ignorance." (Harris, p. 209)

The crucial phrase is "our field": in my opinion, it reverses the causal arrow. *The Rhetoric of Science* is not a consequence of the existence of a field; rather, the impression that there is a field is in part a consequence of this book. In 1990, what was needed was precisely the book Harris describes, a book with an authoritative title by a major press that makes bold claims for the place of rhetoric in the understanding of science and its philosophy. What was needed was not a monograph, but a manifesto. We can easily see this difference in intellectual style by comparing the beginning of this book with the beginnings of Charles Bazerman's *Shaping Written Knowledge* and Greg Myers's *Writing Biology*. While Bazerman and Myers place themselves within existing fields, I announce the presence of a field. The question is not one of quality, but of purpose. The job of this book is to alter the state of the question. To create a disciplinary space, *What contribution can rhetoric of science make to other fields?* had to become, *Is rhetoric of science itself a field?*

The Rhetoric of Science had not only to announce the existence of a field; it had to demonstrate its possibility. To do so, it had to range widely within science; to grapple successfully with such sciences as physics, hitherto thought least amenable to rhetorical analysis. To capture the day-to-day quality of what Thomas Kuhn called "normal science," it had to deal not only with famous scientists, but also with the scientists who do the bulk of scientific work: not only with the Newtons and the Boyles but with the Listers and the Halleys. To reflect the variety of scientific texts, it had to deal with books and articles, but also with first drafts and notebooks.

But there was a goal other than comprehensiveness: in areas traditionally the subject of sociological and historical analysis—for example, peer review and priority disputes—it had to establish itself as an *independent* source of evidence and insight. That these tasks were adequately accomplished is the testimony of reviewers who otherwise found a great deal to criticize: “Gross makes a good job,” says historian John Durant, “of demonstrating that even the dryasdust content of technical reports richly repays rhetorical analysis” (p. 19). “If *The Rhetoric of Science* covers issues and cases already familiar to historians and philosophers of science,” says rhetorician Trevor Melia, “it does so from a distinguishably different point of view. It also displays a range of linguistic sensitivity and virtuosity not typical of work in the sociology of science” (p. 102).

Whether rhetoric of science eventually attains disciplinary status will depend in part on whether the next generation of practitioners can create a unique professional identity by founding and transforming scholarly societies and by building and improving doctoral programs such as those at the University of Minnesota and the University of Pittsburgh. In the cases of history, philosophy, and sociology of science, these developments have already taken place. In rhetoric of science they have just begun. But the disciplinary status of rhetoric of science also depends on whether its intellectual achievements reach critical mass. While the recent burst of activity in rhetoric of science is certainly impressive in its own right, the enterprise still suffers from its character as an aggregate of individual interests rather than a united effort of a group of scholars with a set of common goals. Thus, although a review of the literature is now appropriate, such a survey will reveal not only the strengths of individual achievements, but the weaknesses that are a consequence of uncoordinated efforts.

This survey of the literature will reveal a continuing interest in the epistemic status of rhetoric, a crucial issue if rhetoric of science is to achieve disciplinary status beside its siblings in philosophy, history, and sociology. Unfortunately, this interest has not always been matched by a firm grasp of the philosophical issues involved. This survey will also reveal that a considerable amount of activity in rhetoric of science has been devoted to extensions of current practice. For all their strengths, these studies have the general defects of maintaining the parochial limits of the founders, focusing almost exclusively on the English language and on English and American science in the

twentieth century. In matters of method, these studies have also generally neglected sampling procedures, and, in the analysis of case studies, have ignored the rich sociological and political scientific literature on their interpretation.

To explain the rhetorical features of texts and to illuminate the important rhetorical site of public controversies in which science plays a part, rhetoricians of science have also focused on issues of theory. They have found explanations in fields other than rhetorical; in the cognitive and communicative imperatives of science, for example. But, as this survey reveals, the effect of these numerous theoretical insights is diminished because no one has attempted to synthesize them into a single formulation; nor has anyone attempted to mobilize what has been learned in order to address a major problem in the field. My current project, discussed in greater length at the end of this Preface, is designed to accomplish the latter task: to describe and explain the rhetorical development of the scientific article.

Rhetoric as Epistemic

There are two good reasons for rhetoricians to enter into philosophical quarrels concerning the epistemic status of rhetoric. The first is to rehabilitate the fortunes of rhetoric and to improve its status vis-à-vis established disciplines. This improvement is not a matter of mere prestige: to do its proper work, rhetoric must have earned an academic status equal to other disciplines that focus on scientific texts: for example, history, philosophy, and sociology. The second reason to engage in these quarrels is specifically rhetorical. The issue in this case is heuristic: what view of rhetoric is most likely to foster the growth of the field?

I remain committed to the view that rhetoric has a crucial epistemic role in science, that science is constituted through interactions that are essentially rhetorical. I have made my arguments for this role not only in the Epilogue of this book, but in both philosophical and rhetorical forums ("Philosophy vs. Science"; "Reinventing Certainty"; "Rhetoric of Science without Constraints"). This, however, is the place for me to characterize a view that has been so crudely caricatured. It needs to be reiterated that the relativism I have espoused has been an integral part of the philosophical tradition since the pre-Socratics. My only innovation is to argue my case, not from within the tradition of relativism that stretches from Protagoras and Gorgias to Vico and Nietzsche, but from

within the analytic tradition. "I will build my case," I say, "from within the stronghold of analytical philosophy, constructing a position in which rhetorical interaction is constitutive of knowledge. I make two claims: first, that the philosophical positions of contemporary realists do not entail the realism they espouse; second, that a position consonant with the rhetorical construction of [scientific] reality may be plausibly derived from their work" (p. 194).

The views of philosopher Donald Campbell are representative of the contemporary relativism I endorse. Without any hint that his point of view might be radical, Campbell espouses the relativism I have espoused: "Whatever 'objectivity' is achieved," he asserts, "is based on socially shared plausibility judgments rather than proof" (p. 99). Although he agrees with sociologist Harry Collins that the natural world plays only a small role in the construction of scientific knowledge, he dissents from Collins's more radical conjecture that that role may be nonexistent. Because the advance of science is vital, and because the natural world is, definitionally, the object of science, Campbell feels that those interested in the social construction of scientific knowledge should "look for plausible scenarios as to which types of social customs and structures would maximize that small role" (p. 100). Collins's position is not mistaken; it is just less likely to lead to advancement. Campbell's is a position with which I am in complete agreement: not to address the metaphysical question of access of science to the causal structure of the material world; rather, to impute that access to science, and address the question of the best methods for using that imputed access to create new knowledge.

Those interested in pursuing such problems would best stick to philosophers like Campbell. In "Rhetoric and Reality in the Process of Scientific Inquiry," Heather Brodie Graves is just wrong when she asserts that the philosophical problem can be finessed by rhetorical means: "One way to . . . avoid the impasse," she says, "is to sidestep the philosophical issues about final outcomes and explore the kinds of roles that rhetoric plays in the research practices of human inquiry" (p. 112). She then goes on to study the conversations of a group of physicists as they carry on their research and prepare an article for publication. From these conversations one may discover the views of these physicists concerning the reality of the objects of their investigations. Graves does so in a sensitive manner. She fails to see, however, that no empirical investigation of this sort can resolve the philosophical issue of reference.

But it is not sufficient to understand problems as philosophical ones if you cannot solve them according to the current standards of philosophical discourse. This is Richard Cherwitz's problem. Having previously espoused realism, Cherwitz now espouses "a relational theory of meaning" (p. 326). Though these positions differ, they are apparently alike in their dependence on overturning the relativism characteristic of many in the rhetoric of inquiry movement, and of many prominent sociologists of science. Cherwitz challenges relativism by objecting to those who "[assume] that there is a fundamental distinction between language and objects" (p. 327n). But this distinction is fundamental: language is the repository of meaning, and objects are not. If I say, *This is a ball*, it is I who mean and *ball* that has a semantic dimension, not the apparently spherical object I perceive as out there. Cherwitz and his co-author, Thomas Darwin, also speak of those who "[relegate] objects to an incidental if not meaningless status" (p. 317), who insist that "meaning is reduced entirely to language" (p. 322). But there is no relegation (objects are meaningless), and no reduction is involved (meaning does reside entirely in language).

To understand the value of philosophical relativism, we need to adapt to rhetoric of science the argument that Campbell makes for science itself. There is heuristic merit in robust relativism; there is more to be gained in having a rhetoric of science without constraints because such an enterprise is best-positioned to motivate a theory that provides a full account of textual features, including a rhetorical account of reference. Those who argue against this position are arguing analytical caution. The crucial issue is their attitude, not the quality of their arguments. Theirs is not a good position from which to develop rhetoric of science any more than the Inquisition's was a good position from which to develop physical astronomy.

Extending Current Practice *

In the years since *The Rhetoric of Science* was published, the scope of rhetoric of science has been usefully extended and its claims strengthened. In "Rigorous Discipline: Oliver Heaviside Versus the Mathematicians," historian Bruce J. Hunt extends the scope of

* Important work in rhetoric of science has been omitted because of constraints of space. Those interested in this work should consult the list of references under Harmon, Harris, Krips, Locke, Lyne, Moss, and Vande Kopple. They should also consult the journal *Science, Technology, and Human Values*.

rhetoric of science into the realm of mathematics. Hunt chronicles a remarkable event, the rejection of one of Heaviside's papers by the *Proceedings* of the Royal Society. As a Fellow, Heaviside was by long custom entitled to acceptance of his papers without the formality of refereeing. In this particular case, however, strong opposition from the mathematicians muscled civility aside. The paper was sent for review to William Burnside, professor of mathematics at the Royal Naval College in Greenwich. "Detailed criticism of results obtained in this way seems out of place," Burnside said in his acid summary; "they may or may not be true, but the way in which they are arrived at makes them absolutely valueless" (Hunt, p. 84).

So devastating a critique can have only three sources: thoroughgoing incompetence on the part of the author, personal vindictiveness on the part of the referee, or profound ideological differences between the two. The actual source was the last. Heaviside was a physicist first. As a mathematician, he believed not in the rigor of mathematical proof, but in its usefulness for physics; for him, mathematics was an empirical discipline. Burnside, however, was a representative of a new breed, the English pure mathematicians, men trying to elevate the prestige of their discipline through an exclusive emphasis on deductive rigor. This emphasis becomes, in Hunt's telling, not an essential fact of mathematics, pure or otherwise, but a contingent fact of its history. In Hunt's able hands, rigor is transformed from a mathematical given to a rhetorical device, one means of persuading a particularly constituted audience.

Rhetoric of science's generalizations have also been strengthened. In his paper on the logics of discovery, sociologist Richard Harvey Brown extends the analysis of *Narratio Prima* given in this book. The avowed purpose of Brown's essay is "to define narratives of conversion as a literary/scientific genre" (p. 26). In building his case that "logics of discovery are narratives of conversion generally, and not just for science" (p. 4), Brown first analyzes fictional narratives such as *Moby Dick*. According to Brown, these are stories of conversion designed to draw the reader into the *mores* of an alien culture; in the case of *Moby Dick*, the culture of whaling. Brown then moves from such novels to ethnographic narratives. He establishes a commonality between fictions like *Moby Dick* and ethnographies: in both, the understanding of an alien culture is the goal; in both, the systems of belief of the narrator and reader are altered as a consequence of a double journey—an actual journey into an alien culture, and a metaphorical journey into the self. At this point in his argument, Brown shifts from ethnography to

philosophy. In *Discourse on Method*, he contends, Descartes is taking two journeys, a real European journey and metaphorical journey into the self. This double journey has a radical impact on Descartes's system of belief. In the last part of his argument, Brown turns from philosophy to science. In *Narratio Prima*, he contends, Rheticus's journey is also a double one: an actual journey to Copernicus and a metaphorical journey to Copernicanism.

Recent work has also given us an improved notion of the development of style, arrangement, and argument in scientific prose. The scholarship of Carol Berkenkotter and Thomas Huckin on the news value of scientific articles provides genuine insight in its explanatory synthesis of such apparently disparate genre features of scientific articles as the character of their titles, abstracts, subheadings, and the structure of their introductory and discussion sections. In the view of these two scholars, the development of this suite of features in the twentieth century has the single explanation that each has evolved to increase the news value of the scientific article: "Our analysis of the 350 journal articles in our corpus supports this hypothesis," they say. "The 12 scientific journals that we examined have all modified their genre conventions in a way that foregrounds the most important findings of an investigation, that is to say, promotes news value" (p. 33). For example, they note a significant change in title syntax. A typical title of thirty years ago was: "On the Specificity of DNA Polymerase." As of 1989, however, a title might well encapsulate the claim in a full sentence in the manner of a newspaper headline: "Tumor Necrosis Factor (TNF) Is Induced by *Candida albicans*: The Role of TNF in Fibrinogen Increase" (pp. 33, 54). Although modestly disclaiming originality in declaring their dependence on Charles Bazerman and John Swales, Berkenkotter and Huckin nevertheless move rhetoric of science a step further by their combination of statistical work with ethnography, a way of scholarship that, as we shall soon see, has become important in its own right. The authors' claim concerning news value is supported both by the texts they analyze and by a group of scientists whose reading habits they investigate through interviews: "All seven of these scientists," they assert, "displayed a scanning and reading pattern dominated by the search for interesting new information" (p. 30).

By far the most important article on the rhetorical features of scientific texts is not by a professor of rhetoric but by three professors of library science; it appeared not in a rhetoric journal, but in *Social Studies*

of *Science*. Its title, "Persuasive Communities: A Longitudinal Analysis of References in the *Philosophical Transactions* of the Royal Society," gives scant indication of its broad significance. In it, its authors trace both the pattern and the content of the citations in this long-lived international scientific journal. From their abundance of data, they are able plausibly to infer a multiplicity of conclusions. Of persuasive communities in science, they surmise that they shifted from small to large and from personal to impersonal: "Perhaps nowhere is it easier to observe," they say, "what persuasive communities are in the seventeenth and eighteenth centuries than in the brevity of many of the bibliographic references from that period. It seems that . . . the authors and works cited were like good friends, easily identifiable by shortened names" (p. 291).

The authors infer from citation analysis that the primary medium of scientific communication shifted from books in the seventeenth and eighteenth centuries to journal articles in the nineteenth, and to journal articles, conference proceedings, and technical reports in the twentieth. The authors also trace the shift in scientific productivity from Europe to America, and track the rise of Soviet science. In addition, they follow the eighteenth-century shift in the language of science from Latin to the various vernaculars. In the latter part of the twentieth century, they note a further shift to English as the international language of science. Finally, and perhaps most important, by treating citations as rhetorical features, they are able to "measure" the "rate of change" of persuasive communities; slow in the seventeenth and eighteenth centuries and rapid in the nineteenth and twentieth. In connection with this trend, they venture a cautious prediction concerning the increased tendency toward obsolescence in scientific publications: "if the present trend continues, the median age of the persuasive community may overtake the time required for review and publication of traditional printed communications media. This would lead to increased pressure to adopt speedier means of formal communication in science" (p. 304).

Rhetoricians of science have also illuminated their chosen texts by the methods of ethnographers: close textual analysis, often of early as well as final drafts; interviews with the authors of these texts; protocols in which scientists read and, at the same time, share their thoughts about their reading strategies. In studies focusing on the development and reception of scientific texts, such researchers have given us a "thick description" of the activities of small numbers of scientists in the manner of anthropologists, Greg Myers relying mostly on textual analysis, Charles

Bazerman, on interviews. Unlike Bazerman, Myers has made this method his own, exploiting its possibilities to the fullest. In *Writing Biology*, Myers analyzes peer review as an arena for negotiating the significance of scientific claims. In the instance studied, biologist David Crews sees his "big idea" (p. 69) cut down to size: his paper, rejected by the prestigious *Science*, wends its way ignominiously down to publication in the merely respectable *Hormones and Behavior*. In parallel fashion, the significance of Crews's claim is negotiated downward, narrowed to a claim appropriate to a specialized area. But the story has an ironic reversal. An editor of *Science* reads the article in *Hormones and Behavior*. Intrigued by the issues it raises, he invites Crews to submit a review article on the topic. As a result, Crews's claim moves up a decided notch: the same paper that did not persuade in peer review persuades in publication.

It is an instructive story and Myers tells it well. Moreover, he continues to extend and explore the uses of close textual analysis coupled with interviews. In a 1995 study focusing on the writing of scientific patents, he compares this genre and the genre of the scientific article, illuminating the nature of both: "In each case . . . the writers made changes in response both to textual comments and to their own ongoing experimental findings. These changes imply different ideal readers for articles and patents: biologists or surgeons steeped in the tacit knowledge of their specialisms, as opposed to the generalist, literal-minded 'person skilled in the art.' Both are fictions, but both shape and justify the conventions of a text type" (p. 98). Thus Myers shows how the social forces of science shape the characteristics of its prose.

Such methods are extremely labor-intensive; it is no surprise, therefore, that among rhetoricians of science, only Carol Berkenkotter and Davida Charney have followed in the footsteps of Bazerman and Myers. Charney's analysis of a famous paper by Stephen Gould and Richard Lewontin makes a new point about the implications of the opportunistic reading habits of scientists. Gould and Lewontin's paper is a literary performance, with a title to match: "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptionist Program." But, Charney points out, scientist readers resist the sequential reading the paper invites; they read opportunistically, as we might expect from her study and a similar one by Bazerman in *Shaping Written Knowledge*. From this empirical result, however, Charney mounts a criticism of some of her fellow contributors: "While Gragson and Selzer suggested that the literary and cultural allusions in the

introduction and conclusion encouraged scientists to read as intellectual humanists rather than as 'mere biologists,' there is little evidence that these readers accepted the invitation" (p. 225). Thus the careful unraveling of the verbal microstructure of scientific texts, whatever it tells us about the intent of authors, can say little about its effect on readers. Inferences of the latter sort are unreasonable; they are undermined by a failure to understand science as a *distinctive* cultural practice with a distinctive way of approaching texts. Only empirical studies can tell us about actual rhetorical effects (for another view of the limits of "literary" analysis, see Gross, "The Experiment as Text").

Berkenkotter, like Myers, focuses on peer review as a process of scientific knowledge construction. In her particular case, however, the problem is not the breadth of a claim by a scientist who feels, for the first time, the pressure of a first-rate idea. Rather, the problem is a scientist of the second rank with limited intellectual horizons. In the first, rejected draft of a paper, this scientist reacts negatively to referees who ask her to place her work in the context of a research front broader than her laboratory. She calls this a "phony story." But, Berkenkotter points out, "far from being a phony story . . . the larger narrative is, in a sense, the *real* story" (p. 59). In her conclusion, I object only to the nervous hedge: "in a sense." There is no need to hold back on a conclusion that supports and extends Myers's work, one that illustrates so pointedly the superordinate narratives that frame and govern scientific arguments.

In these admirable studies, I object only to the limitations of scope and can point only to some problems of method. It has been a general problem of science studies that they place undue emphasis on the seventeenth and the latter half of the twentieth centuries to the neglect of the eighteenth and nineteenth, and that they have almost invariably dealt with scientists writing in English. This has created a skewed view of the history of science, and a parochial view of what started as a European and became a global phenomenon. Ethnographic studies, of course, are not subject to criticism for diachronic insufficiency. But they suffer, as do many studies of rhetorical features, from methodological inadequacy. Myers is deeply worried about the sampling problem in ethnomethodology—he calls it the $n = 2$ problem. Given a statistical model, he cannot make even cautious generalizations. But Myers is apparently unfamiliar with the rich methodological and practical literature on case studies in theory building in comparative

political science and in sociology. He does not mention the work of George, Eckstein, Lijphart, Ragin, and Becker. Were he familiar with this work, his own might enter a new phase.

Alternatives to Classical Theory

Despite the usefulness of extending current practice, the issue most important to rhetoric of science is the role of theory. Without an adequate theory of its own, rhetoric of science becomes just another descriptive enterprise: because it can explain nothing, it cannot command serious attention. Several reviewers referred to *The Rhetoric of Science* as a collection of essays rather than a book. I think they were mistaken if what they meant was that it does not present a sustained argument. But if they meant that it was not animated throughout by a coherent theory of rhetoric, then they were correct. This is not to say that the perspective of the book was not that of classical rhetoric. In Dilip Gaonkar's words, this perspective consists of five "views": "1) a view of speaker as the seat of origin rather than a point of articulation, 2) a view of strategy as identifiable under an intentional description, 3) a view of discourse as constitutive of character and community, 4) a view of the audience positioned simultaneously as "spectator" and "participant," and finally, 5) a view of "ends" that binds speaker, stratagem, discourse, and audience in a web of purposive actions" (p. 263; numbering added).

Although I have defended myself as an Aristotelian under this description (Gross, "What if We're Not Producing Knowledge?" Gross and Keith, eds., *Rhetorical Hermeneutics*, 1996), the problem for me remains one that Gaonkar so perceptively uncovered: it stems from the nature of rhetorical theory; a theory developed, after all, not to analyze texts but to produce them. As a consequence of this productive orientation, "in its current form, rhetoric is a language of criticism so thin and abstract that it is virtually invulnerable to falsification" (p. 263). Although my perspective throughout *The Rhetoric of Science* was that of classical rhetoric, the tools that I inherited from that tradition were not designed for the job of rhetorical criticism; they fell far short of the precision necessary for adequate discrimination. At one point, for example, I linked science and *stasis* theory: "In the sciences, what entities really exist? Does phlogiston? Do quarks? Before Einstein's papers on