This article reviews the recent work of Alan G. Gross (individually, and in collaboration with Joseph Harmon and Michael Reidy), with prominent notice, as well, of works by Leah Ceccarelli, Celeste Condit, and Jeanne Fahnestock, among others, in order to sketch out developments in the rhetoric of science.

Books Reviewed in this Essay:


“The rhetoric of science? C’est moi.” That’s what many of us took Alan G. Gross to be proclaiming with the title of his 1990 book, The Rhetoric of Science, when the subfield was barely underway (Harris 1991; Myers 2003, p. 374). Sure, Gross had published a flurry of significant articles in the late 80s, exploring the suasive dimensions of scientific discourse, but several other rhetoricians had as well—Charles Bazerman, Greg Myers, Lawrence Prelli—and their resulting books had considerably more modest titles. Prelli’s (1989) A Rhetoric of Science made a particularly instructive contrast (see also Bazerman 1989; Myers 1990). And some of the more influential scholars—John Lyne, Carolyn Miller, John Angus Campbell—had not published books at all. Most gallingly to others in the field, Gross barely noticed any of this parallel work in his book, attending rather more
closely to the work of philosophers, sociologists, and historians. But now, nearly twenty, highly productive years later, it’s almost true. Alan G. Gross? C’est rhétorique de la science,—presque.

Many scientists, and not a few philosophers, sociologists, and historians feel the very phrase, rhetoric of science, “undermines the claims of science” (Booth 2004, p. 155), and the rhetorical is a perspective on science marginally visible on the science studies landscape, even in so ecumenical a journal as Perspectives on Science.¹ So, I will use this close identification of the field with a single scholar as an opportunity, in reviewing his three most recent books, to chart the rhetoric of science: where it is now, and how it got here.

Gross’s (1990a) The Rhetoric of Science is easily the best known book in the field—widely reviewed, widely cited, widely taught, widely attacked. Its first iteration (1996a) included a long preface, in equal parts feisty and inclusive, responding to criticisms and broadening Gross’s notice of other rhetorical research, but was otherwise unchanged. Now there is the major reconfiguration under review here, Starring the Text, altered not just in content and structure (by about fifty percent) but in creed as well; one of the more radical early theorists, Gross is now among the more reactionary. In the prolific interim between the first and third renderings of his signature book, Gross engineered a watershed event, for his own work and for the field generally, including known provocateur Dilip Parameshwar Gaonkar in a session (co-organized with John Lyne) on rhetoric of science at a major conference. Gaonkar’s long, unremittingly censorious essay on the hermeneutic assumptions underlying rhetorical analyses of science was quickly published, with several similarly polemical responses, in the special number of a national journal (Keith 1993). Gross then published Gaonkar’s essay as the opening chapter of Rhetorical Hermeneutics: Invention and Interpretation in the Age of Science (Gross and Keith 1997), following it with more responses, and capping it all with another lengthy essay by Gaonkar, responding to the responders. In the wake of Rhetorical Hermeneutics, Gross collaborated with Joseph E. Harmon and Michael Reidy (2002) on Communicating Science: The Scientific Article from the Seventeenth Century to

¹ Other than a predictably rampant use of metaphor (in 52 articles) and the occasional appearance of irony (13 articles), rhetorical vocabulary is largely absent from the journal since 1998 (the earliest year I could search at my institution). Only two articles in that period invoke the field directly, in a fairly casual way (Solomon 2003; Mattila 2006). Citations to the significant literature either of rhetoric generally or rhetoric of science specifically are few and far between (only Ceccarelli 2005 and Myers 1990 show up, once each; there are no citations to Gross’s work). And the deployment of words like rhetoric and rhetorical, as in Whitt (1999), on “The Rhetoric of Research Justification,” are inevitably to ordinary-language senses of the terms, meaning something like ‘discourse strategies.’
the Present, also under review here, with a sketchy history of the field foregrounding what Gross sees as a second-wave paradigm for rhetoric of science. He collaborated with Harmon again, for an exhibit of scientific texts and images held at the University of Chicago’s Joseph Regenstein Library, an exhibit which grew into The Scientific Literature: A Guided Tour, the third book presently under review, and, with Laura Gurak, he edited a special journal number on The State of Rhetoric of Science (2005). Over the same period, Gross also produced an important collection of essays re-examining the wellspring text of rhetorical theory, Rereading Aristotle’s Rhetoric (Gross and Walzer 2000), and a superb little book on one of the 20th century’s major argumentation scholars, Chaim Perelman (Gross and Dearin 2003)—both books largely independent of his rhetoric-of-science research—along with more than fifty articles and chapters, some of which have been folded into the books under review, many of which have not. Combine that textual fecundity with his conference attendance, his visiting fellowships, his teaching, and his tireless intra- and inter-disciplinary conviviality, and the conclusion is inescapable: he has influenced rhetoric of science as has no one else. There are certainly other productive, front-rank scholars in the discipline, but Gross has lapped the field, as all of them would no doubt cheerfully acknowledge. He is universally admired and beloved. Alan G. Gross? C’est rhétorique de la science,—presque.

That presque, ‘almost,’ is razor thin, from the side. But, rotate our angle of vision ninety degrees, and it is also unavoidably wide, for two reasons. First, there is Gross’s curious, continuing indifference to the research of many other rhetoricians of science. Second, and surely related, there is the distinctive, if not idiosyncratic, direction he has taken his own primary research. He is by far the largest pike in the pond, but Alan G. Gross is not swimming with the school. There are many factors contributing to the separate and increasingly conservative course he is plotting, but the most obvious reason is the Gaonkar Episode, which reaches directly into the workings of Gross’s first book.

Gross’s choice of title for that book, The Rhetoric of Science, was less the act of a colonizer than a colonist—he is too modest for imperialism—but a colonist with high market savvy. The title was assertive, clear, catchy, a slogan; his publisher, prestigious (Harvard); his analytic targets, texts by members of the scientific pantheon (Copernicus, Newton, Darwin, Watson and Crick); his claims, capacious, uncompromising, threatening. Other rhetoricians of science, with meeker titles, lesser known presses, and more obscure object texts, were doing fine work, but quieter work. Gross wanted attention, more for the field than for himself, and he got it. The book was quickly noticed, and quickly assaulted. Almost every reviewer
outside the discipline of rhetoric found the book too big for its britches. But, Gross-the-marketer had been right, they found the book.

In the slipstream of thinkers like Polanyi and Hanson, Ziman and Kuhn, Feyerabend and Latour, many of Gross’s reviewers were comfortable enough with the claims that science was indelibly linguistic and utterly social, and that the knowledge it made bore the marks of the linguistic and social machinery which produced it. But to use the word _rhetorical_ with any seriousness in this context—overlaid as it was by the ordinary language sense of specious, florid, pointless speech, and by the postmodern sense of anti-rational processes—was beyond the pale.

Worse, it was naked, aggressive expansionism. Sociologist Anna Wynne, for instance, worried that Gross’s book implies that “acceptance of the rhetoric of science would be sociological death” (1992, p. 93). Mathematician and philosopher of science, Jerome Ravetz, sniffed about the “bad press” science was receiving, and, diagnosing the cause as inferiority complexes endemic to the humanities, held Gross’s book up as an especially egregious example from an especially inferior discipline: “Even more than a sociologist,” he complained in _Nature_, “a rhetorician has a professional motivation for reducing science to the same lowly status to which it had been assigned” (1991, p. 30). STS scholar, John Durant, in the _Times Literary Supplement_, howled over Gross’s “wild exaggeration[s] about the analytical scope and significance” of applying rhetorical notions to scientific argumentation. Even ten years later, Alan G. Gross was still a poster child for the blundering recklessness of humanities incursions into science. Philosopher Joseph Agassi, for instance, pilloried the second edition (apparently unaware, though, that it _was_ a second edition, or that there was a growing literature about the book or the field; even Gross’s name is misspelled). “That style matters is trivially true,” he said, in one of his more temperate moments, “and that style is all that matters is trivially false.” His verdict? Once Gross’s excesses are removed, you end up with piddling little: “the translation of the book to remove its exaggerations . . . will remove from it the absurd thesis [only style matters] and land it in a trivial one [style matters]” (Agassi 1999, p. 329; see also Gross 2000; Agassi 2001). The second edition, however, came out the year before the Gaonkar volume Gross edited with William Keith, _Rhetorical Hermeneutics_, after which Gross could no longer be mistaken as a spokesperson for rhetorical excess.

Meanwhile, back at headquarters, rhetoricians were quite sympathetic to Gross’s positions in _The Rhetoric of Science_. But many were perplexed, given the insistence of the title, over its relatively superficial use of the rhetorical tradition, coupled with an eclectic suite of methods drawn from
other sources; it was also hard to miss that Gross barely notices the work of other rhetorical critics of science. Despite a chapter on Darwin, for instance, Gross manages only a pro-forma nod at John Angus Campbell, whose superb series of papers on Darwin’s textual strategies now goes back almost forty years (Campbell 1970; Campbell 1975; Campbell 1986; Campbell 1989; Campbell 1994a; Campbell 1994b; Campbell 2005). Gross’s treatment of Watson and Crick does not cite Halloran (1984) or Bazerman (1981); his chapter on Newton includes no mention of Bazerman (1988); Jeanne Fahnstock (1986; 1989; Fahnstock and Secor 1988) is ignored, and John Lyne (e.g., 1983; 1985; 1987; Lyne and Howe 1986), and Jean Dietz Moss (1983; 1984; 1986; 1988; see also 1993), and Lawrence Prelli (1984, 1989a), and Herbert Simons (esp. 1980; 1989), among others.

Gross’s critical methods in *The Rhetoric of Science,* too, betray an unease about the analytic power of rhetoric not shared by most other practitioners. Aristotle gets an introductory gesture, and is put to sporadic, mostly superficial, work in the book; Chaim Perelman has a minor presence; there is a glance at Kenneth Burke; and that’s pretty much it for the rhetorical tradition. Gross appears more comfortable in the book with the work of Jürgen Habermas, and Victor Turner, and Donald Davidson, and John Searle than the work of Cicero, or Quintilian, or Hermogenes, or Erasmus, or Melanchthon, or Vico, or George Campbell, or Richard McKeon, or Wayne Booth. Indeed, Gross’s eagerness to traffic in the theories of sociologists and anthropologists and especially philosophers might be one of the factors that led scholars from those fields to see rhetoric of science as imperialist, and Gross as a self-styled conquistador. But Gross meant his use of such work as a compliment not as an expropriation.

Rhetoricians, in any case, are temperamentally pluralist, and they saw that Gross’s heart was full of rhetoric even if his toolkit wasn’t. In particular, a theoretical movement developed in the later twentieth century around the motto, *rhetoric as epistemic* (Scott 1967; 1976; 1990; Brummett et al. 1990), in which truth and knowledge “can arise only from cooperative critical inquiry,” with cooperative critical inquiry seen as fundamentally implicating reciprocal suasion: rhetoric (Scott 1967, p. 14). Gross was a champion of this movement (Gross 1990b), arguing that serious tests of its claims would have to be drawn from the hardest, brightest truths, the sturdiest knowledge, that humans had made: from science. *The Rhetoric of Science* was his collection of such tests, and he was uniformly optimistic that the ore had proved out. At his most alarming to some, most bracing to others, he seemed to be claiming that our most powerful epistemic engine, science, was rhetorical all the way down. He became notorious here, famous there, for the claim that science was “rhetoric . . .
without remainder” (1990a, p. 33; Gross and Keith 1997, p. 6; Gross 1992; see also McGuire and Melia 1989, 1991, 1995). That phrase may have been the one that set the teeth of speech-communication scholar, Dilip Gaonkar, on edge.

Gaonkar featured the phrase prominently in his relentless argument that not only is science not rhetoric without remainder, nothing much else is either (1997a, p. 40). Gross had used the sciences as test cases for the epistemic reach of rhetoric; Gaonkar uses rhetoric of science as the test case for the critical reach of rhetoric, which he finds to be short indeed. He argues that the vocabulary of rhetoric, dependent as it was on the ancient craft of training orators, was “too thin” (p. 33) for the critical analysis of discourse, scientific or not, and therefore rhetoricians were forced to borrow heavily from intellectually thicker disciplines for productive methods, a borrowing they pursued “promiscuously” (pp. 37, 38, 47, 64, 71, 76). He prosecutes several representative analyses to argue the case: Gross’s analysis of Copernicus, Campbell’s of Darwin, and Prelli’s of an experimental article on the biochemistry of memory. Gaonkar brings it all home by revisiting an ancient analogy:

Since the time of Gorgias, rhetoric has been compared to a psychoactive drug. In the contemporary usage, it is not clear whether rhetoric is actually a drug, some sort of (in the current pharmacological jargon) “delivery system” for a drug, or a substitute for a drug. As a delivery system, rhetoric has at least two distinct forms: a nondescript mixer (such as water) that dilutes the potency of an alcoholic beverage, or a sweetener (such as sugar syrup) which camouflages the bitterness of a medicine. The difference is critical. In the case of a mixer no one confuses relative tastes of whiskey or gin, even though both are mixed with water. Rhetoric in this sense is a ubiquitous but dull accompaniment, not something worth studying. . . . But in the case of a sweetener, there is room for ethical mischief and ideological distortion, since the underlying flavors are obscured. But the third alternative is the most critical. When there is no drug, no substance, rhetoric itself functions as a drug. . . . In the construction of scientific knowledge/practice/culture, does rhetoric . . . ever threaten to become part of the substance of science? No one doubts that rhetoric functions as the mixer, sometimes as the sweetener, but can one argue that science is a simulacrum (in Baudrillard’s sense), a rhetorical construction without reference? (Gaonkar 1997a, p. 77)

The argument, while spruced up with gin and Baudrillard, is at least as old as Plato (who also invoked Gorgias). It has been answered, and iter-
ated, and re-answered, and reiterated ever since. For our purposes, there are only two points to make about Gaonkar’s recycling. First: very, very few rhetoricians saw any strength in it. There were many responses (see especially Leff 1997; Miller 1997; Willard 1997; Schiappa 2001), but only a small fraction of them showed any sympathy with Gaonkar’s conclusions, or his argumentative strategies, or his evidence. It was, in this sense, a watershed moment for the field: “Gaonkar is clearly good for our business,” John Angus Campbell said in his role as the 1998 president of the American Association of the Rhetoric of Science and Technology: “[S]o little are Gaonkar’s charges against us believed, and so useful are they in garnering our attention, that we boast of an impressive array of new recruits particularly among younger faculty and graduate students” (1999, p. 101; see also Selzer 1998, p. 448). Typically, Gross found the widespread and forceful rejection of Gaonkar’s arguments distasteful, calling the ensuing debate “fruitless [and] . . . best forgotten” (Gross and Gurak 2005, p. 242).

Second: Gross was, of course, one of those very few rhetoricians who saw strength in Gaonkar’s argument. The Gaonkar episode marks a conversion experience for him. His own response in Rhetorical Hermeneutics is fretfully entitled “What if We’re Not Producing Knowledge?” and anxiety drives the whole article:

Surely Gaonkar’s assault on my work, mounted with such panache, requires, if not a retreat, at least a strategic withdrawal to previously occupied positions. For if Gaonkar is right, it seems to me, what I and my fellow rhetoricians of science have been producing in the last decade is not an instance of classical rhetoric’s strength but a demonstration of its intellectual bankruptcy, not knowledge but nonsense. (1997, p. 138)

Gross does not, in the end, take Gaonkar’s argument as a legitimate “dismissal” of his preceding decades of work, or of rhetoric of science generally, but as a severe “admonition” (1997, p. 153), and his work following the episode can definitely be so characterized, admonished.

Rightly so: if anyone in the field was guilty of the sins against which Gaonkar fulminates—methodological looseness and a rhetorical terminology too anemic for the tough work of analyzing scientific discourse—Gross was he (Harris 1991; Miller 1993; indeed, see Gross’s frank 1991a “Response to Harris”). And his obvious unease about the sufficiency of the rhetorical tradition made him ripe for remorse. There is this, too: his current critical work is the better for heeding Gaonkar’s reproach. Gross continues to make the remarkable mistake, however, of believing (and regularly asserting) that Gaonkar’s critique stung the whole field. The introduction to Starring the Text, his refitting of the good ship, The Rhetoric of
Science, bifurcates the field into a guilty first generation and a chastened second generation which abides Gaonkar’s demonstration of “the methodological limitations of the first generation” (2006, p. 14; see also Gross, Harmon, and Reidy 2002, pp. 5–6; and Gross and Gurak 2005, pp. 241–242; and the early suggestions of these themes in Gross 1996a, pp. xii–xxviii). The division is true of his own work, but completely spurious as a characterization of the field generally. Take the three monographs Gross identifies (along with work of his own) as representing the second generation: Leah Ceccarelli’s Shaping Science with Rhetoric: The Cases of Dobzhansky, Schrödinger, and Wilson (2001), Celeste Michelle Condit’s (1999) The Meanings of the Gene: Public Debates about Human Heredity, and Jeanne Fahnestock’s Rhetorical Figures in Science (1999). All are worthy of the praise he gives them, but only Gross (with, one guesses, Harmon and Reidy) sees the Gaonkar episode as somehow generative of their quality.

Ceccarelli’s book is an excellent analysis of three synthesizing, hortative texts, and of their rhetorical receptions: Theodosius Dobzhansky’s Genetics and the Origin of Species (1937), central in effecting the neoDarwinian synthesis which brought naturalist and genetic strands of biology together; Erwin Schrödinger’s What Is Life? (1944), catalytic for the interdisciplinary collaboration of biologists and physicists which birthed molecular biology; and Edward O. Wilson’s Consilience (1998), which attempts to bring the humanities and the sciences together under one grand research programme, but fails at least as spectacularly as the other two succeed. Among the reasons Ceccarelli finds for Dobzhansky’s (and Schrödinger’s) success is a form of noetic chiasmus, persuasively framing each constituency’s perspective in the other’s terms:

whenever geneticists thought about gene frequencies [after Dobzhansky’s arguments], they would imagine populations moving about in space; whenever naturalists thought about populations inhabiting ecological niches, they would think about the alteration of gene frequencies. (Ceccarelli 2001, p. 37)

2. Gross and Gurak also consider Bazerman’s exemplary (1999) study of Edison as representing the post-Gaonkar “new look” of the field (2005, p. 242). However, given that Gaonkar ignores not only Bazerman’s work but that of writing-studies rhetoricians generally (Gaonkar 1997a, p. 80n19; Bazerman et al. 2005, pp. 78–79), and given that Bazerman, one of the few prominent rhetoricians not to comment on Gaonkar’s arguments, repays the compliment, the suggestion that The Languages of Edison’s Light somehow reflects Gaonkar’s influence is another indication of how fully Gross misreads that influence. As several people have noticed, Gaonkar’s neglect of Bazerman’s work, and Greg Myers’s, and Carolyn Miller’s, and John Lyne’s, among others, amounts to “falsifi[cation] by omission” of the field (Selzer 1998, p. 448).
An even more effective tool in coalition-forming is strategic ambiguity, what Ceccarelli calls polysemy. When Schrödinger, for instance, said that “Living matter, while not eluding the ‘laws of physics’ as established up to date, is likely to involve ‘other laws of physics’ hitherto unknown, which . . . will form just as integral a part of this science as the former” (Schrödinger 1944, pp. 68–69; Ceccarelli 2001, p. 75), he sponsored at least three distinct construals of his pregnant pivotal phrase, “other laws of physics,” each construal inspiring a different sub-community of physicists or biologists. Each of these sub-communities pursued these other laws under these partially competing construals, constructing the foundations of molecular biology along the way. Wilson’s Consilience program failed to inspire a similar (or, as he had hoped, an even greater) collaborative synthesis in large part because, despite gestures at reciprocal framing and at multiply evocative goals, and despite a superb command of English stylistics, he strongly privileges a view in which the humanities (therefore, morality, creativity, aesthetics, culture) are wholly under the thumb of the sciences. “Rather than motivate readers for different reasons to engage in collaborative activity,” Ceccarelli says, extracting a lesson from Wilson’s book, “appeals that try to speak to more than one audience but do not properly balance the interests of those audiences are easily dismissed as a form of equivocation” (2001, p. 182).

Condit’s Meanings of the Gene also relies on reception studies to gauge rhetorical effects, but where Ceccarelli focuses on how certain historically situated scientists understood her primary texts—charting their citational framings, the interests they highlighted in book reviews, and their specific patterns of response—Condit focusses on popular discourse, plotting general patterns of response by the American public to the science of genetics in the twentieth century, as reflected in “structured random samples of indexed discourse in magazines, newspapers, the Congressional Record, and news programs” (1999, p. 260). Where Ceccarelli plumbs the motivational effectiveness of three major works, Condit traces broad cultural currents in reaction to, and reflections of, scientific developments generally; she has no primary texts, no major works. Ceccarelli’s and Condit’s works are both rhetorical studies, but they come at the patterns of suasion and identification they study from very different quadrants. Ceccarelli is traditional, using venerable critical tools. Condit brings social-science methodologies to the job.

Condit is especially strong in the way she uses the close-reading techniques of rhetorical criticism on a set of texts that would for orthodox...
rhetoricians just form the backdrop against which significant rhetorical practices played, the setting rather than the scene. She read over a thousand articles, the bulk of them from popular U.S. magazines published between 1919 and 1995, pointedly excluding magazines with particular interests in science, in order to better gauge the overall social texture of beliefs about issues of heredity and the gene rather than the sources of information and opinion feeding those beliefs. Condit is interested in rhetorical formations—currents of discourse, defined by tropes, images, narratives, topics, commonplaces, argument motifs, and the like, at play with and against each other, with and against material formations, with and against social formations, always recognizing that the boundaries among these categories are highly permeable. The rhetorical elements “are not independent of economic forces or spatial demographics or of any of the multiple other material inputs to a social system,” she says, but they are important in their own right because experiences “are always mediated through our language about them.” For instance,

it matters whether one thinks of a genetic practice as “stock breeding” or whether one thinks of it as “recoding” one’s genes. It is not enough to think of both metaphors as merely different versions of the same genetic paradigm. . . . [Nor is it] enough to focus on metaphors, because no single discursive component defines the discourse of an era. The potential meaning of the coding metaphor was severely constrained by the dominant model of gene-environment interactions. And the metaphor’s meaning changed significantly when it was displaced to a commercial era and accompanied by the blueprint metaphor and revised models of gene-environment interactions. (1999, 252–252)

Reading closely, but broadly, following a careful sampling procedure, Condit traces out the cultural meanings of the gene from the heyday of eugenics to the development of the human genome project. (For another approach to the rhetoric of eugenics, which also broaches the human genome project, see Hasian 1996.)

The best of Gross’s putative “second generation” group, the best, in fact, of any rhetoric-of-science group one might put together—indeed, among the best books in rhetoric generally over the last decade, and in science studies of any species, and in argumentation to boot—is Fahnestock’s *Rhetorical Figures in Science* (1999). It infuses argumentation theory with a deep understanding of the inventive potential and persuasive force of unheralded figures (not heavily explored tropes like metonymy, metaphor, and irony, but widely overlooked formal schemes, like gradatio, polyptoton, and antimetabole), probing a remarkable range of scientific argu-
ments to demonstrate her vital point about reasoning, and to investigate the rhetorical machinery of science.

Fahnestock builds on the insight that rhetorical figures epitomize lines of reasoning (an insight she convincingly traces back through Aristotle’s *Rhetoric* and *Topics*), to build an approach she calls *figural logic*. She is almost contemptuous of metaphor, as the star of loosely rhetorical studies of science and argumentation, but Fahnestock uses it effectively to bootstrap her broader case. “The fact that scholars have long recognized metaphor as an epitome of analogical reasoning makes the point,” she notices: “One need only accept that human reason exploits more than analogy, and that, therefore, more figures than metaphor are linked to corresponding modes of reasoning” for her claims about figural logic to go through (1999, p. 24). Take polyptoton, the figure in which a lexeme assumes multiple forms—for instance, *lie*, in the title of Al Franken’s 2003 book, *Lies and the Lying Liars Who Tell Them* (subtitled, in a nicely ironic completion of oxymoron, *A Fair and Balanced Look at the Right*). If a figure is, as Fahnestock contends, “a condensed or even diagram-like rendering of the relationship among a set of terms, a relationship that constitutes the argument” (1999, p. 24), what does polyptoton epitomize? Among other things, it condenses thematic relationships of the sort that bind into coherence the noetic frames that in science go by names like *theories*, *paradigms*, and *research programmes*. Fahnestock, for instance, chronicles two centuries of electrical thought by the fortunes of the root *electri*, from William Gilbert’s usages in 1600 to Joseph Priestly’s in 1766, finding a trajectory from adjective (*electric*) to category noun (*electrics*) to abstract noun (*electricity*) to verb (* electrify*, in turn giving rapid birth to the nominalization *electrification*). “Every developing science,” she suggests on the basis of this case study, “will present a path of polyptotonic expansion in its key terms, the unique signature of its conceptual growth” (1999, p. 194).

But it is probably easiest to see the power of polyptoton in the theme-and-variation relationships among phenomena encoded into familiar taxonomic arrangements. Knowing your alkanes from your alkenes from your alkynes is fundamentally polyptotonic, requiring you to know precisely what consistent core of meaning is shared by these words for their respective compounds. The relational claims made by such chemical nomenclature are by now, of course, largely petrified arguments, with effectively universal assent, but they are arguments all the same. Philosophers of science have long complained about the clumsy, earth-bound boots natural language forces science to walk around in, praising airy, artificial, mathematically infused languages for the precision and power they can bring to scientific expression. But the taxonomies anchoring organic
chemistry, and geology, and biology in almost all of its many varieties, would be unthinkable without natural-language resources like morphology, honed by mechanisms like polyptoton and homoioteleuton (the scheme in which different stems assume the same morphological form—as in the relationship among alkane, ethane, and methane). Nor are morphological schemes alone in this regard. Indeed, ploche, the opposite scheme, in which a lexeme is doggedly repeated in the same form, is the lexical resource that bleached and desiccated formal languages, like predicate calculus, clinging most closely to it. It epitomizes reasoning based on identity, as in \((p \lor q) & \sim q \rightarrow p\). Antithesis epitomizes reasoning from opposition, fundamental to all distinctions, as in this contrast between the olfactory cells of fish and mammals: “In fish, individual mitral cells project several dendrites onto multiple glomeruli, whereas in mammals, single mitral cells project a primary dendrite onto a single glomerulus” (Yoshihara, Nagao, and Mori 2001, p. 835; see Fahnestock 2004, p. 29n2). The whole rational programme on which philosophy and science rest is frequently dated to Parmenides poem, *On Nature*, whose crucial “path” to certain knowledge is paved with polyptoton, ploche, and antithesis:

Come now, listen, and convey my story. I shall tell you what paths of inquiry alone there are for thinking: The one: that it is and it is impossible for it not to be. This is the path of Persuasion, for it attends upon Truth. The other: that it is not and it necessarily must not be. That, I point out to you, is a path wholly unthinkable, for neither could you know what-is-not (for that is impossible), nor could you point it out. (Fragment II)\(^4\)

Incrementum epitomizes directional series reasoning, endemic to evolutionary theory, in which progressive differences along a continuum constitute the sole evidentiary argument for common descent; cladistics is rooted by incrementum. Antimetabole, a fancy-pants scheme in which words or phrases are repeated in reverse order, epitomizes reciprocal schemes like commutative reasoning (\(m + n = n + m\); cf. Fahnestock 1999, p. 133), mutual entailment (von Mayer’s 1842 expression of the

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\(^4\) "Εἰ δὲ ὁδὴ ἔγραμεν ἔρέω, κόμισοι δὲ σὺ μὴ θορν ἀκούσας, ἂν ἔρωτι μὲν μὴ ἔρωτος ἐραί ὑνήσαι, ἢ μὲν ὁποῖς ἔστι τε καὶ ὧς ὅκ ἔστι μὴ εἶναι, Παθοῦς ἔστι κέλευθος - Ἀληθεία γὰρ ὁποῖας ὑποθεῖα ἢ, ἢ δὲ ὡς ὅκ ἔστι τε καὶ ὧς ἅρπην ἔστι μὴ εἶναι, τὴν δὴ τοῦ φράσεως πινακευθὲς ἐμμεν ἀπαρτὸν - οὕτε γὰρ ἄν γνώνῃς τὸ γε μὴ ἔδω - οὐ γὰρ ἀνοικτὸν - οὕτε φράσεως."
conservation of energy, “If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force” —1929, p. 32; Harmon and Gross 2007, p. 106), and complementary causality (Newton’s “If you press a stone with your finger, the finger is also pressed by the stone” —1952, p. 14; Fahnestock 1999, p. 142). And so on (see also Fahnestock 2003, 2004, 2005a).

*Rhetorical Figures in Science* is a *tour de force*, about which Gross is justly enthusiastic. There is, however, nothing remotely second generational about it. Fahnestock herself is no beginner—she has been active in rhetoric of science for as long as Gross—nor has she undergone his conversion experience. Further, she does not so much as mention Gaonkar in her book, choosing to align herself explicitly in the preface with the work of foundational scholars like Bazerman, Moss, Myers, Prelli, and Gross1 (that is, to the Gross that Gross2 now calls “an earlier version of myself” [2006, p. 5]). The same is largely true for Condit and for Ceccarelli, who also pay no attention to the Gaonkar episode in their books, and who also align themselves unapologetically with the familiar early rhetoricians of science. Gross is certainly right that not one of these three have the methodological promiscuity that Gaonkar thunders against like “a grim Puritan father” (McCloskey 1997, p. 106), but there is no argument other than a *post hoc ergo propter hoc* that their methodological constancy is a consequence of that thundering.

The case is clearly different for Gross, who was chastened by Gaonkar, and whose resultant new approach to the rhetoric of science is correspondingly more constant. He doesn’t buy Gaonkar’s arguments wholesale; for that, he would have to abandon rhetoric of science altogether, if not rhetorical criticism outright. For all his fretting that “we’re not producing knowledge,” and for all his promotion of Gaonkar’s case, Gross does push back, defending an Aristotelian critical vocabulary “enriched by the insights into language and its contexts of use that two millennia of reflection have bestowed upon us” (1997b, p. 139). But the shape that enrichment took in his own earlier work was somewhat arbitrary (as all commentators noticed, though none others in as grimly Puritanical terms as

5. “Wenn Fallkraft und Bewegung gleich Wärmer, so muss natürlich auch Wärme gleich Bewegung und Fallkraft seyn” (Mayer et al. 1929, p. 41). Notice (here, and with the following Latin example) that since German is an inflectional language, not only does the example necessarily include polyptoton, but also that there is considerably less linguistic pressure for the logic to be expressed by linear order, and still Mayer (and Newton) adopts an antitemetbole.

6. “Si quies lapidem digito premit, premiture et hujus digitus a lapide” (Newton 1822 [1687], p. 16).
Gaonkar), and his second-incarnational work signals a Vow of Non-promiscuity.

*Communicating Science* benefits from the vow, achieving a high degree of methodological purity, along with an admirable increase in linguistic and cultural scope as well. The methodological purity comes equally from constraining the analytical toolkit and the data pool. Instead of turning now to Habermas for communicative action, now to Searle for speech acts, Gross and his colleagues anchor their approach in Perelman and Olbrecht-Tyteca’s (1969) *The New Rhetoric*, circumscribe three specific domains, and interrogate their data for a carefully chosen set of textual features. The data, they assemble by following a statistically guided sampling procedure, similar in spirit to Condit’s (see also Bazerman 1988, pp. 59–79; Atkinson 1999; Hyland 1998). As Gross and his colleagues point out (2002, p. 5), rhetoricians of science (with Gross 1990a among the most culpable) have often chosen object texts on the basis of star power (celebrities like Darwin and Newton) or proximity (scientists one hallway over from the rhetorician). For *Communicating Science*, texts are firstly confined to articles (books are excluded as increasingly peripheral to the practice of science, over the last century especially, among other reasons), and, secondly, selected solely on the basis of representation. The increase in scope comes from broadening the pool linguistically (and, therefore, culturally). Gross, Harmon, and Reidy also point out (2002, p. 5; also Gross 1996a, p. xvii) that the rhetorical investigation of scientific prose has been hugely anglocentric; even when dealing with texts whose originals were in German or Latin or French, the English translations were taken up without reflection (again, Gross 1990a is prominently guilty). There was also a skewing in terms of scientific disciplines in much of this work, towards physics and biology.

The *Communicating Science* sample includes more than 2,000 articles, representing over three and a third centuries (1665–1995), in three languages (English, French, German), covering the five major branches of science (astronomy, physics, chemistry, biology, and geology), and the five main genres (methodological, experimental, observational, theoretical, and review articles). From this data (drawn, under a random protocol, from a list of elite journals, as identified by Gascoigne 1985 and Garfield 1976), they read 430 articles, top to bottom, and examined short passages (10 contiguous lines, randomly selected), from around another 1,800, seeking to answer questions about style (lexical and syntactic choice), presentation (arrangement, layout, graphics), and argument (appeals, evidentiary strategies, structure). As Bazerman has noted, the study remains quite preliminary, given the great sweep of the topic, and there are ways
the sample might be further mined for the effect of factors such as scientific discipline, genre and arrangement (2004, p. 342), but the Communicating Science project is very impressive, the accomplishment substantial. On both counts, it amounts to Gross meeting, full on, the challenge Gaonkar raised for him.

The results of the Gross-Harmon-Reidy study outline a clear picture of the development of the scientific article. The outline is not entirely new, but it is sharply rendered and solidly based. Stylistically, the length and clausal complexity of sentences has decreased. Scientific prose is famous for its difficulty, to the point that scientists in neighboring fields, or even different internal specialities, can be almost as baffled by an article as a non-scientist. But that difficulty does not arise from sentence structure. Rather, noun phrases have become both longer and denser. Here’s a highly typical sentence from a relatively recent (1989) molecular-biology article, with the highest level noun phrases underscored: “[T]he disparate kinetics of accumulation of LHCPs shown in figure 2a could result from selective inhibition of translation of LHCP mRNA or subsequent rapid degradation of most of the newly synthesized polypeptides” (quoted in Gross et al. 2002, p. 168). As this sentence also illustrates, lexical choice is highly constrained: many of the words are exceptionally precise, heavily freighted, and exclusive to a very local discourse community. The differences with the following seventeenth century sentence from the Philosophical Transactions of the Royal Society, reporting the behavior of a sheep immediately after a transfusion of calf’s blood (“a good stream,” maintained for “the space of 7 minutes,” replacing the “45 ounces and better” of blood just removed from her):

[W]e set her free, and she had no sooner got her liberty, but seeing a Dog near her (which was a Spaniel, that had formerly suffered the transmission of Sheeps-blood into him) she butted with great violence at him three or four times, not appearing at all concern’d at what she had endured in the Experiment (quoted in Gross et al. 2002, p. 34).

It includes eight clauses, with lots of brief noun phrases, in contrast with the recent sentence, which is only one clause long (with three swollen noun phrases). The differences in general comprehensibility come from semantic heft and exactitude of the words in the current example, and the long trains of modification. There are also hints here about the presentational differences that have developed in science, with the recent sentence’s evocation not only of a graphic (figure 2a), but of a graphic participating in a hierarchical series. Many early Philosophical Transactions articles were brief, intended for oral presentation, and conceptually sparse, so their
“subsystems [were] relatively undeveloped” (Gross et al. 2002, p. 43). Gross, Harmon, and Reidy chart the evolution of these subsystems in the development of titles, labels, and headings, the proliferation of textual components like abstracts, methodology sections and results sections, the solidifying of arrangement, the maturation of typographical features, and the incredible ascendancy of graphics (which they treat both in terms of presentation and of visual argument; and, which, incidentally, they deploy well themselves, efficiently rendering their style, presentation, and argumentation data in tables).

Our two sentences also partially illustrate differences in scientific argumentation. The early sentence has a testimonial appeal, a verisimilar evidentiary framework, and a narrative structure; it is a story, flush with realistic details, told by a witness. The recent sentence has an explanatory appeal, an esoteric evidentiary framework, and a logical (disjunctive) structure. It is an account of data, visually presented in an attendant, abstract graphic, flush with technical detail, told by an impersonal voice, idealized above and beyond the notion of individual witnessing. There is far more than this going on—as Gross and his colleagues explicate with precision and fullness—but the nub of the differences manifest in these two passages is between recounting observations and explaining facts. Some seventeenth century science, especially the science we now venerate, explains, and much current science recounts observations (especially of those highly managed events, experiments), but the goals of science have shifted overwhelmingly toward explanation, evolving through a reciprocal feedback system with the rhetorical machinery of science, and the technologies of intervening in nature.

The least satisfying part of Communicating Science, as the authors seem aware (they present it with numerous, recurrent hedges), is their own attempt at explanation, their account of the forces that reshaped the scientific journal article. In the final chapter, they seek “to explain what [they] have described” over the preceding 200 pages; that is, they are after “a theory of change” (Gross et al. 2002, p. 214). They find it in the biggest theory of change there is, Darwinian selection, as adapted in one strain by Stephen Toulmin, and adapted in another strain by David Hull. From Toulmin, they borrow a notion of collectively warranted concepts as units of change, from Hull, the notion of scientists as interactors, systems as replicators. Evolution, of course, works as an account of change, any change. That’s what led Dennett to extol it as “a universal solvent, capable of cutting right to the heart of everything in sight” (1995, p. 521). The trick in making it into a genuine explanation is to identify the selectional pressures, the mutational agency, and the unique variables distinguishing specific cases of evolution. Gross and his colleagues meet the first two bur-
dens, but they do so through an analogic melange that may import a philosophical lineage, but adds no clarity. As near as I can tell, the kernel of their selection theory is a gradatio:

scientists:conceptual-systems::interactants:replicators
interactants:replicators::genotypes:phenotypes
genotypes:phenotypes::articles:genres

For my money, the Toulmin and Hull borrowings bring nothing of value to Gross, Harmon, and Reidy’s account here. Their talk of textual features such as clausal simplicity and noun-phrase expansion as “characters,” of style, presentation and argument, as “character suites,” and of scientific predispositions as “phenotypes,” is all quite helpful, but it would have been more efficient if they had got to the analogy without the philosophical detour. More troublingly, Gross and his colleagues don’t meet the third burden (identifying the unique variables of their explananda) well at all. Yet those variables are precisely the ones that rhetorical theory can add to this sort of analysis.

What is remarkable, for a book that situates itself as primarily rhetorical (Gross et al. 2002, p. 8), which invokes Aristotle as the first theorist of style, and again as the chief representative of the classical theory of arrangement, and whose opening chapter is “heavy-handed in its discipline-building agenda” (Myers 2003, p. 377), is that there is precious little rhetoric when there is theoretical work to do. Not only does rhetoric go missing when a theory of change is needed for the leading scientific genre (or, more accurately, genre-cluster), so does Aristotle, the father of genre studies; indeed, even the very term genre has evaporated by the time Gross, Harmon, and Reidy take on their explanatory work. I put it into the analogic schema above because it is the only word that makes sense. But Gross and his colleagues weirdly prefer to avoid analogy for direct metaphor at this stage, using locutions like “species of articles” and “communicative phenotype of the article.” The term genre—the rhetorical word, but also the most obvious ordinary-language word, for the phenomenon they build their theory around—does not appear even once in the chapter.

For a rhetorician, other striking omissions for the explanatory theory they assemble include the work of theorists Kenneth Burke, Lloyd Bitzer, and Carolyn Miller, among others who have contributed to a view of genre as social action, as well as Charles Bazerman and Dwight Atkinson, who have done longitudinal work on scientific genres (Bazerman from a social-action perspective). Gross, Harmon, and Reidy say that “the rhetorical tradition is useless in explaining alterations in style, presentation, and argumentation over time” (p. 12), but there is little evidence that they even
looked to that tradition for explanatory resources. A rhetorically informed
genre theory—one that advances Aristotle’s approach in this area as Perel-
man and Olbrecht-Tyteca advance it in argumentation—might have al-
lowed them to get more fully at some of the variables that separate dis-
course evolution from biological evolution. Gross and his colleagues
correctly notice, for instance, that the evolution of the scientific article is
teleological (p. 219), but they drop that observation as soon as they make
it. An analysis of efficiency and objectivity, two of the defining drives of
scientific communication, in terms of Burkean motives would certainly
take them further in the direction of an explanation of teleology than they
manage to go. Genre evolution, too, is more Lamarckian than Darwinian,
because agents of that evolution invent and learn; discourse traits are ac-
quired traits. Again, Gross and his colleagues notice this variable, but
again drop it quickly, introducing it only to dismiss it as a possible objec-
tion against using evolutionary theory (p. 13). Burkean social-action could
help here as well, in exploring what Burke identifies as the agent-agency
trait. My complaint here is not that Gross, Harmon and Reidy fail to use
Burke, or any other specific theorist from the tradition, but that they pull
up their collars and cross the street to avoid any and all contact with rhe-
torical theory, hailing others, when there is heavy lifting to do.

Gross’s other book with Joseph Harmon (but sans Reidy), The Scientific
Literature, has no heavy lifting to do. It is a wonderfully light complement
to Communicating Science, though it does have two unfortunate weaknesses.
The first weakness, its coverage, is unavoidable. The scope of scientific
writing—over the four centuries since it pulled itself out of alchemical
treatises and surgical manuals to grow into “the most triumphant, the
most imitated, the most universal form of human discourse ever de-
veloped” (Montgomery 1996, p. 2)—is immense. Each of the five scientific
domains Gross et al. (2002) sample from could easily support its own book
(The Biological Literature, The Earth Sciences Literature, . . .). Drawing so
widely, there are necessarily many omissions, and the extracts that are in-
cluded are necessarily very brief. Again, this is simply unavoidable, on
both counts. Any book much bulkier than this one would not find a pub-
lisher, and we should all be very pleased that Chicago has brought the
book out, given the current market conditions (most notably, the avail-
ability of many scientific classics on the web—some of the sources for
which, the book lists, pp. 309–310). And Harmon and Gross do a really
superb job, under the incredible tension of scope and space, of selecting,
excerpting and framing the scientific literature—constructing a kind of
Michelin Guide, as they note, “meant to inform, amuse, and tantalize
rather than fully to satisfy the inquiring mind” (Harmon and Gross 2007,
p. xx). But it is an impressively broad guide for all that. They include over a hundred and thirty pieces, from many a luminary and even more merely front-rank scientists, all of them with their most significant historical and textual features explained, in the best travel guide fashion.

The second weakness also rests more on the constraints of publishing than on the failings of Harmon and Gross: The graphics are uneven at best, and rarely rise above the just acceptable. (The graphics in Communicating Science and Starring the Text are on the whole considerably better.) Since the book arose out of a library exhibit (which I, lamentably, did not see), it is unfortunate that some arrangement could not have been made to web-mount the exhibit on a more permanent basis (or perhaps include it on an enclosed CD), so that the graphics could be available in higher resolution, as well as longer versions of at least some of the texts. Given the dire straits of much academic publishing, an opportunity to incorporate digital media, especially when it is so strongly called for, should not be ignored.  

The strengths, in any case, more than compensate. Again, the coverage is admirable—not so much in general terms, and every science scholar will find at least a few favorites overlooked, but in terms it shares with Communicating Science, well-thought-out sampling criteria. While all of the excerpts are in English, French and German literature is again represented. All of the major sciences are represented, both in overall distribution and in specific sections. There are sections which sample from scientific controversy, with brief series of back-and-forth pieces, and from exemplars of argument structure, and from chronological periods, and from historically important journals, and from different national contexts, and from different reasoning patterns. One section that all readers will turn to eagerly is “Scientific Writing Style: Norms and Perturbations.” The pieces illustrating norms highlight features like caution, impersonality, clausal simplicity, lexical density and phrasal complexity. The perturbation pieces demonstrate unusual outbreaks of undisguised belligerence, playfulness, or self-conscious eloquence. There is a charming diagram of an experimental apparatus that appeared in the Journal of the American Chemical Society in 1955, which was in almost every respect typically scientific (abstract, precise, accurate), except that there was a small stick-figure man fishing in one of the flasks (containing a 1% suspension of the algae, Scenedesmus, in a 4 mg. KH$_2$PO$_4$/liter solution). Another piece, from a 1935 issue of Science,  

7. There is an online seminar available, related to the exhibit, but unmentioned in the book, which partially serves this purpose, and which would be an excellent teaching aid, available via Columbia University’s Fathom Archive (www.fathom.com/course/21701730/index.html; accessed March 2008).
by ichthyologist, Hugh M. Smith, is a splendid example of ploche, polyptoton, and incrementum in the natural history of fireflies.

The focus of The Scientific Literature is the article, in its various subgenres, which makes the book an excellent source of specimens, with useful side trips, embodying the results and arguments of the more scholarly (one is tempted to say the more scientific) Harmon-Gross book, Communicating Science. It is also, aided by Harmon and Gross’s very careful (but breezy and elegant) framing, a primer for scientists looking for styles and structures to emulate in their own work. They will be entertained and educated, reading Robert Hooke and James Hutton and Sewall Wright, but they will also find useful models, reading Edwin Hubble and Oswald Avery et al. and Murray Gell-Mann; and important lessons on the incorporation of tables, and equations, and images, reading Dimitri Mendeleev and Albert Einstein and Richard Feynman; and inspiration, reading almost any selection; and they will find, as will you, a very wide-ranging, illuminating, and rewarding explication of the scientific literature, reading the very fine commentaries on these articles by Harmon and Gross.

You will find, too, something much like in a rhetoric-of-science handbook. In most of his work, Gross’s defining anxieties are exoteric. He wants to fashion a place for rhetorical criticism in science studies. There are two basic procedures for such an enterprise: to demonstrate the unique contribution rhetoricians can make in the analysis of science, and to provide rhetorical instruments and understandings for other analysts of science (philosophers, sociologists, historians) to use. Most of his books take the former path—The Rhetoric of Science somewhat belligerently, in Communicating Science, conciliatorily—but The Scientific Literature takes the other path. While they hardly mention rhetoric by name at all, Harmon and Gross identify the salient stylistic, presentational, and argumentational features of each specimen in their collection, as well as highlighting some of the selectional pressures that contributed to the exercise of those features in the given context, doing so as the best tour guides do. They won’t turn you into a native, but they will help you find your way around.

In Starring the Text, Gross returns to establishing the unique contributions rhetoricians can make to science studies. In fact, he returns to The Rhetoric of Science, to remove the belligerence, and much else. About half the previous chapters are gone, replaced by newer material, all of the retained chapters are significantly altered, line-by-line, and the whole is placed in a very different frame. Gaonkar accelerated the shift, but Gross had started to back away from his constitutive and ubiquity claims within a year of the publication of the first version (e.g., 1991a, p. 36)—suggesting that he had pushed his claims, and certainly his phrasing, to extremes, in a calculated play for the interdisciplinary hearing he did in fact gain.
Now, he “excise[s] the radical epistemological inferences that were designed to provoke thought,” while also claiming that his “philosophical position” survives intact (p. ix), a highly curious position for a rhetorician to take, and one that Starring the Text does not support.

The new title may strike readers unfamiliar with literary studies or semiotics, as somewhat peculiar, and certainly doesn’t have the marketing panache of the original title. It comes from Roland Barthes’s argument that the first move of semiotic analysis is to feature the text, put it in a starring role, catch it in the spotlight (1974, p. 13; more particularly, Barthes means dismembered pieces of the text, chunks he calls *lexias*). Gross uses Barthes’s notion prominently in the first edition (1990a, p. 4), adopts it for the title of the third version, and prefaces that version with a sketch of its implications. The notion is pivotal to him in the way it clears a space for rhetoric among the disciplines of science studies. Recall the full title of the recent rendition: Starring the Text: The Place of Rhetoric in Science Studies. Here is how Gross frames rhetoric in the preface:

> Is rhetoric a master discipline, encompassing all others? Of course not. The claim that rhetoric is an intellectual tool useful in explicating the sciences rests on an alternate epistemological vision: an insistence that science is just one way of knowing. This claim in turn rests on a fundamental federalism about the domains of knowledge. In accordance with this federalism, rhetoric produces a knowledge different from that of science, different though not inferior: knowledge of science insofar as science is persuasive communication. Rhetoric “stars” the texts, tables, and visuals of science, that is, it makes their hermeneutic unraveling central. This is its role in science studies as one discipline among many joined in a common enterprise, a confederation of equally sovereign intellectual states: history of science, philosophy of science, and sociology of science. (2006, p. ix; see also 1990a, pp. 9–10)

The question-and-answer opening dispels not only Gaonkar’s accusations that Gross “goes global” with rhetoric in his first version of the book (1997a, p. 45), but also those of all the science-study reviewers complaining about his apparent imperialism (Wynne, Ravetz, Durant, Agassi). Once the analyses get under way, he pauses to say “None of [his foregoing rhetorical analysis] suggests that science is only rhetoric; no sane person could reach so bizarre a conclusion” (p. 78). But, of course, that is exactly what Gross seemed to be saying, before Gaonkar’s psychotherapy brought

8. See also Gross et al. 2002, p. viii, “Our goal is not to reduce science to mere rhetoric.”
him to sanity, with his famous ‘science is rhetoric without remainder’ motto.

Even more than methodological looseness, Gross is anxious in his current work to repudiate the strong epistemological positions for which his early work became emblematic, deploying his first-generation/second-generation schema to that end. He associates the strong view, that rhetoric is constitutive of science, with the earliest rhetoricians of science—Campbell, Bazerman, Myers, Gross (2006, p. 5)—and the restraint to “not [to] engage epistemic issues” (2006, p. 17) with the two texts he regards as virtually definitional of the “new phase” (Gross et al. 2002, p. 17): Condit’s Meanings of the Gene, and his own Communicating Science.” And, with Starring the Text, he literally rewrites his own legacy, erasing the offensive “without remainder” passage. Let’s revisit the scene of the crime.

Here is what Gross says in The Rhetoric of Science:

A complete rhetoric of science must avoid this accusation: after analysis, something unrhetorical remains, a hard, “scientific” core. In this chapter I want to test the hypothesis of completeness against evolutionary taxonomy, the science of classifying animals and plants as species in accordance with evolutionary theory. If a rhetoric of this science is possible, we must be able to reconstruct the central concept of evolutionary taxonomy, the species, rhetorically, without remainder. (1990a, p. 33)

So: the comment is hypothetical, particular to a case study, and requires much ellipsis and some insertion to emerge with the general claim phrased as “[science is] rhetoric . . . without remainder.” Still, the general claim is in the spirit, not just of Gross’s book, but of the rhetoric-of-science enterprise more widely. The same year Gross’s remark was published, for instance, Herbert W. Simons characterized all the essays in his rhetoric-of-science anthology, The Rhetorical Turn, as affirming “there is no escape from rhetoric” (1990, x, p. 16). Moreover, to my mind, Gross satisfies his without-remainder hypothesis in the taxonomy chapter. In particular, he shows that “a sharp distinction between rhetoric and rationality is untenable” (1990a, p. 53) in the conceptual construction of the notion, ‘species,’ with obvious implications for conceptual construction of all stripes,—notably of scientific and of rhetorical stripes; rhetoric and ratio-

9. As an entry for the Annals of Lumping and Splitting, it is worth noting that in Gross’s 1996 entry for the Encyclopedia of Rhetoric and Composition, Prelli and Bazerman are lumped together, split away from Gross; they are conservative, he radical, about epistemological issues. In Starring the Text and Communicating Science, all three are lumped for their strong epistemological concerns, with the split now at Gross as the conservative.
nality fully interpenetrate. But here is what Gross says in *Starring the Text* about the possibility of a rhetorical analysis of science without remainder:

Ø

Other, similarly provocative, phrases are weeded out of the book as well. The phrase “*sub specie rhetoricae,*” for instance—as in “*sub specie rhetoricae,* scientists create an ontology” (Gross 1990a, pp. 41–42)—caused some consternation (Melia 1992; McGuire and Melia 1989, 1991, 1995; Gaonkar 1997a) for its suggestion that rhetoric swallows science whole. The new phrasing drops *sub specie rhetoricae,* and even *ontology* (2006, p. 56), which, we can suppose, Gross might have worried could sound too presumptuous for philosophers to hear from a rhetorician’s lips. One can only hope that this more conciliatory, less uppity, approach wins his new iteration as much appreciation as the aggressiveness of the first won attention. And the good news is: the book’s critical successes not only survive the lexical purge, but in places are strengthened and refined by Gross’s increased emphasis on rigor. I only wish I could say the same about the new theorizing.

I want to be very clear about this: I admire Gross tremendously for assuming this new incarnation. Most scholars, myself at the top of the list, stick tenaciously to their first principles, once those principles have developed and proven themselves in scholarly work. It takes a particularly limber and responsible thinker to adopt such markedly different positions so late in a career, when all around him are arthritically resisting those positions, or at least scratching their heads over his abandonment of his previous positions. Moreover, to instantiate them in a work like *Communicating Science,* and to rewrite the very text whose arguments he repudiates, takes more than a supple mind and a deep scholarly commitment. It takes remarkable energy. But—maybe it’s the arthritis talking—I am convinced that he is wrong. He found himself shackled to a bad metaphor, and rather than simply repudiate the metaphor, and refine his arguments in other terms, he has eliminated the position he introduced the metaphor to articulate.

What does it mean to say that one cannot remove rhetoric from discourse or from argumentation or from human symbolic exchange of any sort—or from an entire domain that rests on these activities, like, say, science—to achieve a purified remainder? Put in these terms, it means, effectively, there is only rhetoric. All discourse, the metaphorical entailment runs, is constructed from primordial persuasion particles, so that one cannot precipitate out of the activities or the products or the noetic sphere of science anything one could actually call *science.* This position, I readily concede, is loopy. It can be defended, in standard Gorgianic or deconstruc-
tionist ways, but it can’t be used. All is rhetoric, therefore rhetoric is nothing; si omnia nulla (McKloskey 1997, p. 107). Most rhetoricians, however, don’t see rhetoric in these particulate terms, as if it constituted the fundamental, irreducible corpuscles of symbolic exchange; suasons, perhaps.

If we drop the particulate metaphor and ask again what it means to say that one cannot eliminate rhetoric, cannot shut it out, from discourse, or from science, we can try other lenses to focus on the issue of rhetorical ubiquity, and perhaps also get at the rhetorical constitution of knowledge. Burke, for instance, the modern proponent of rhetorical ubiquity, says that rhetoric stems from “an essential function of language itself” (1969, p. 43). Scott views rhetoric as a “human potentiality” (1976, p. 266), Kennedy as “energy” (1992, p. 7), Meyer, “an intrinsic feature” (1994, p. 2), Perelman and Olbrechts-Tyteca, an “action” (1969, p. 9), inherent in communication. All of these framings, in one way or another, are rooted in Aristotle’s extremely influential, sophistically favored definition of rhetoric as “the faculty [power/force/ability—δυναμις] of discovering in any particular case all of the available means of persuasion” (Rhetoric 1355b).

While Aristotle did not deliver very fully on this definition, restricting his attention mostly to the particular cases of politics, law, and public ceremony, his elemental phrasing is taken in the modern context to signify that “Wherever there is persuasion, there is rhetoric. And wherever there is ‘meaning,’ there is persuasion” (Burke 1969, p. 172).

Saying that Gross was shackled to a metaphor is the sort of thing you might expect of a rhetorician, the sort of thing that possibly marks me as deflecting attention away from a claim, and toward the shape of that claim. But notice that a particulate view and a functional (and an energetic, and a potentiality, and a feature, and an action) view make different claims—which is precisely what rhetoricians mean when they say that rhetoric is constitutive of knowledge. The shape makes a difference. An essential-function claim says that any and all symbolic traffic implicates rhetoric. There is no corner of science, because science is an inescapably symbolic activity, where you will not find evidence of rhetorical functioning. Rhetoric is pervasive, ubiquitous. If you managed to eliminate all rhetoric, then, you would also have eliminated all symbolic activity, including science. But an essential-function view does not carry the implication that you will find only rhetoric in those corners, that there no other functions, incidental or essential, in science or in symbolic traffic generally, nor even that rhetorical methods are the best to probe a given symbolic amalgam, such as a theory, or a given symbolic domain, such as science—merely that rhetoric can not be shut out, which, by the way, is the central point of Gross’s taxonomy chapter. A mouth might talk, might chew, might breathe, might bite off a piece of an ear in a boxing match; a
scientific article might instruct, might convince, might entertain, might inflame, might discourage (as Harmon and Gross 2007 demonstrate). One function needn’t exclude another. A without-remainder particulate view contends that rhetoric is the very stuff of symbolic activity, with the unfortunate implication that if anything else even matters, it does so in derivative, subordinate ways. Gross was widely perceived outside of rhetorical circles to be saying just that in The Rhetoric of Science. His external readers, too, were shackled by the metaphor. But, in Starring the Text, when he has the chance to clarify, he simply retreats. Without remainder goes, and nothing comes in to take its place.

The textual remodeling of The Rhetoric of Science into Starring the Text is, in every way, substantial. Gone are chapters on style, arrangement, public deliberation, and Watson and Crick’s double-helix paper, as well as an epilogue entitled “Reference without Reality.” Remaining, in recognizable form, but with different shades and accents, are chapters on biological taxonomy, on Newton’s Opticks, on the Newton/Leibniz calculus-priority dispute, on Darwin’s notebooks, on peer review, and on the spread of heliocentricity. Added, are analyses of the cold fusion bruhaha and the development of occupational medicine. All of these studies are worth the price of admission. The old ones have been strengthened. The study of the Origin of Species notebooks, for instance, is enriched by incorporating Darwin’s tree diagrams. The new ones are first rate, displaying Gross’s familiar “linguistic sensitivity and virtuosity” (Melia 1992, p. 102); Gross is a superb critic of scientific discourse. While he remains highly comfortable incorporating the work of sociologists, philosophers, and historians, the overt rhetorical quotient is also now considerably higher. Perelman plays a more consistent role in the analyses. Stasis theory and the topoi and Aristotelian pistes are put to sustained work. The new book is also bracketed with Gross’s attempts to position rhetorical analyses of science in terms of philosophical, historical, and sociological analyses. It begins with three chapters building “The Case for the Rhetorical Analysis of Science,” in which Gross seeks to answer Gaonkar directly. It ends with “Science Studies: Where Rhetoric Fits in,” in which he is more concerned with the fears of other science-studies scholars over rhetorical imperialism. In many ways, this is the quintessential text on rhetoric of science: the major statement of its major scholar, full of perceptive critical illustrations, attentive to the landscape of science studies, and clear in its assertion that

10. Curiously, with these two sections, the book fails one of the principal criteria Gross uses to characterize second-generation work in rhetoric of science. The “first phase” in the field, Gross argued with Harmon and Reidy (2002, p. 8), was defined by “the politics of recognition,” which the “new phase” makes obsolete. Gross not only carries over the disciplinary preoccupation that defines The Rhetoric of Science, he elaborates it in Starring the Text.
“the truths of science are not beyond argument; rather, they are achievements of argument; science rests on facts and theories that have been argued into place” (p. 43).

Gross is wrong about the importance of Gaonkar to others in the field, and virtually alone in his belief that Gaonkar’s arguments are compelling, and, therefore, he hangs the historical picture crookedly. But he still gets the landmarks right, and does them justice in his comments. And Gaonkar was important to the recent history of the field, just more as a common enemy than an abided moralist. Gross is also disappointingly cautious about the ubiquity and epistemic reach of rhetoric. But this objection amounts to little more than personal taste. Certainly the reception they gave The Rhetoric of Science indicates that many science scholars want their rhetoric served up cautiously, and lots of rhetoricians are agnostic about these issues. Some even reject Burkean flavours of rhetorical theory and pledge allegiance to a rigidly classical (post-sophistic) picture.

There is still this, however: the curious matter of Gross’s remarkable scholarly indifference to work by other rhetoricians of science. It’s not that he is unaware, or even unappreciative of other practitioners. He routinely reviews rhetoric-of-science books (1990c, 1991b, 1993, 2001), frequently surveys the field (1996a, pp. xii–xxviii; 1996b; Gross et al., 2002, pp. 5–8; Gross and Gurak 2005, pp. 241–247), and has made manifold generous comments on other rhetoricians of science. But their work makes a negligible impression on his own. Fahnestock, for instance, specifically developed some of his (Gross 1990a, p. 37) insights on a theme-and-variation graphic illustrating species definition for hummingbirds in terms of figural logic (Fahnestock 1999, pp. 174–175). Despite a sentence-by-sentence overhaul of the book, however, Gross doesn’t so much as drop a footnote in Fahnestock’s direction when he returns to this graphic, let alone bring her into the entire taxonomic discussion of that chapter, which would have benefited from her work on polyptoton. Miller wrote a paper on kairos, which Gross (rightly) calls “brilliant” (1996a, p. xxii), analyzing the famous pre-Watson-and-Crick failure of Avery et al. (1944) to win a hearing for their linkage of DNA and heredity in terms of “the inertial weight of communal scientific commitment” (1992, p. 321). Yet in his own observations on Avery et al.’s rhetorical failure (2006, pp. 122–123), it doesn’t occur to him to employ Miller. He still finds no room for Campbell’s work in his treatment of Darwin’s notebooks, Bazerman’s in his work on Newton.

Even more dramatically, we find his curious attitude to a recent collection of essays by leading rhetoricians of science investigating the sorts of scientific-dispute morasses frequently labelled incommensurable (Harris 2005). The contributors to that collection include Bazerman (with René
Agustín De los Santos), Campbell, Ceccarelli, Fahnestock, Miller, Prelli, Simons, and Thomas Lessl (along with Paul Hoyningen-Huene, with a superb biographical essay on Kuhn and Feyerabend, keyed to their development of incommensurability). Gross, too, contributed an essay. One could not ask for a more representative or prestigious slate of rhetorical critics of scientific discourse. Yet when Gross re-used many of the arguments and analyses from his contribution, for a paper on stasis theory and incommensurability that came out while the collection was going to press (Gross 2004), he mentions neither the book nor its chapter by Prelli prominently applying stasis theory to incommensurability, nor even his own cognate arguments—out of what appears to be a species of scholarly mortification. His position on incommensurability was considerably out of sync with those of the other rhetoricians in the book, so far out of sync that in Starring the Text, where the (2004) paper reappears almost verbatim, he rejects their consensus on the problem of incommensurability with the shockingly (and uncharacteristically) discourteous label, “so extreme that it can be safely ignored” (2006, p. 181).\footnote{In yet another context, he is neutral with respect to the rhetoric and incommensurability volume (Gross and Gurak 2005, pp. 242–243).} Gross is apparently embarrassed to be seen in their company by respectable science scholars. Again: he participated in the volume, he put his best dissenting case forward, writing a strong account of Kuhn’s later formulations of incommensurability, with a brief illustration via an awkward 19th century reconciliation of wave and selection theories of light, and, in that context, very civilly expressed his disagreement with the largely coalescent conclusions of the others (2005, pp. 196–197).\footnote{Taking his lead from Buchwald (1992), Gross argues that David Brewster’s amalgamation of wave and selection theories into a coherent but cumbersome and unproductive framework ultimately demonstrated that the two theories are “profoundly incommensurable . . . at the level of the conceptual structure of the Lexicon [in Kuhn’s (2000) sense of the term]” (2005, p. 189). This vitiates, for Gross, the views of all the other rhetoricians represented in the volume that incommensurability is a pseudo-problem best treated in terms of argumentative failure, rather than of formal blockage, a failure more of theorists than of theories. The Brewster case, from the pseudo-problem perspective, in fact demonstrates very clearly the commensurability of two theories; that is, their capacity to meet on a common ground, permitting comparison and licensing intertranslatability (albeit, as in linguistic translation, with certain losses and gains). The fact that Brewster’s combination was inelegant and sterile does not prove that the source theories were incommensurable. A simpler explanation would be straightforward incompatibility. One might blend two architectural styles together, for instance, and end up with a mess simply because they don’t work well together. For that matter, one can end up with a lumpy, barren theory working from scratch. On the view of most of the other rhetoricians in the volume, claiming that theories of a common domain are always commensurable is not claiming that they should be combined, or that any combination will necessarily be as successful as its components; rather, it}
scholar. But the fact remains that he can read an entire book by the (other) luminaries in rhetoric of science and find nothing worthy of comment or incorporation when he plumbs the same problem field. Moreover, there is abundant evidence that this utter neglect cannot be explained by pettiness or ego. He is beating not only a different path from most rhetoricians of science—in some fundamental ways, he is beating an antithetical path; perhaps, according to his own take, an incommensurable path.

While Gross is an excellent critic and a careful thinker, that is; while he has a deep appreciation for the power of rhetoric; while he is a pioneer in rhetoric of science; while he is its best known and most dogged proponent: neither his views nor his approach are entirely representative. And his overall take on the field is lop-sided. Rather than demonstrating the limitations of early work, or the effectiveness of Gaonkar’s castigation, works like Ceccarelli (2001), Condit (1999), and Fahnestock (1999)—along with several others, like Atkinson (1999), Bazerman (1999), Waddell (2000), Locke (2002), Harris (1997, 2005), Segal (2005)—demonstrate the continuing health of the field, its methodological richness and its relevance to apparently recalcitrant fields of discourse, like those of the sciences.

In 1996, Gross commented on the intellectual balance-of-trade gap affecting rhetoricians in science studies: “While they readily cite other disciplines, other disciplines rarely cite them” (note the antimetabole-antithesis merger here, epitomizing the logic of negated reciprocity). If it does improve, as all of us practicing rhetoric of science believe it should, the new balance will owe a great deal to the tireless, wide-ranging, scrupulous efforts of Alan G. Gross, la rhétorique de la science. Presque.

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is claiming that such theories can always be brought into sufficient alignment for comparison, negotiation, and mutual translation. As Fahnestock (2005b, p. 391) phrases it, the impression of incommensurability on this view follows from “an active rhetorical investment” of the scientists on differing sides in a theory-vs.-theory dispute, adding that “[w]hen one buys into incommensurability, one declines negotiation.” It is this position that Gross does not share.
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