A Hard Look at Ourselves: A Reception Study of Rhetoric of Science

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This study traces the reception of rhetoric of science work in book reviews by science studies scholars, focusing particularly on critiques of the author’s own book by historians of science, to ground recommendations for improving our status in the interdisciplinary conversation. In describing the work of other science studies scholars, rhetoricians of science should acknowledge the rhetorical contributions of non-rhetoricians and negotiate a shared space rather than attempt to fill perceived lacunae in the literature.

The call for this special issue points out that early works in the rhetoric of science were “characterized by increasingly bold forays into territory previously occupied by those in other areas of science studies.” The metaphor of trespass is telling. Other metaphors used to describe the rhetorician of science include “new kid on the block” (Harris, “Rhetoric” 285), and “‘latecomer’ in the game of ‘science studies’ dominated by historians, philosophers, and sociologists” (Gaonkar 267). I adopted similar language in a recent article when I called the rhetoric of science a “young field that sometimes suffers lack of confidence in communicating outside its peer group” (Ceccarelli, “Rhetoric”). These characterizations of the rhetoric of science point to a status imbalance that casts a shadow on our academic self-esteem.

There is a sense that as rhetoricians of science, we must direct our research to two audiences—our extended family of rhetoricians in departments of communication and English and the interdisciplinary community of science studies scholars who have already established dominance in the academic study of our chosen object of analysis. This is not a dilemma unique to scholars in the rhetoric of science. Even researchers who remain faithful to our discipline’s traditional object field of public address can find themselves in scholarly conversation not only with other rhetoricians, but also with historians and political scientists who are interested in
the same subject and who are suspicious of scholarship from a discipline that both embraces a title corresponding to sophistry and lacks a presence in some of the most prestigious universities.

But as the recent formation of an Alliance of Rhetoric Societies demonstrates, we rhetoricians are taking on the mission of improving our standing in the larger academic community (Zarefsky). In the rhetoric of science, there have been various recommendations for what we should do “to rehabilitate the fortunes of rhetoric and to improve its status vis-à-vis established disciplines” like “history, philosophy, and sociology” of science (Gross, *Rhetoric x*). I am eager to add my own list of proposals to the mix. But it seems fitting that as a rhetorician, I ground my recommendations in some research about the discourse of these academic communities. In a previous project, I examined the persuasive strategies of scientists who wrote interdisciplinary monographs, and I studied the reception of those monographs by scholars from the targeted academic fields, as recorded in book reviews and articles that cited the original work (*Shaping*). Why not apply the same reading strategy reflexively to my own institution’s discourse to see how different audiences are receiving our work? By examining how audiences from various science studies disciplines are responding to the arguments for academic significance that we rhetoricians of science have been making, we might be able to improve our future arguments for interdisciplinary relevance.

My preliminary research along these lines was not encouraging. Many of our colleagues in history, philosophy, and sociology of science treated our early work as illegitimate incursions on the field of science studies. They wrote nasty reviews pronouncing our work a “flagrant violation of etiquette” that knows little about science (Agassi 329, 335), a “hotch-potch” and a “hatchet-job” that is stained with “immodesty” (Ruse 127–28) and marred by “wild exaggeration about the analytical scope and significance” of our endeavors (Durant 19). They proclaimed rhetoric of science an “uncomfortable example” of how sociological and historical studies can be put to use in another discipline but “likely to offer more to rhetoric than to science studies” (Myers 559). And they dismissed the rhetoric of science as a field tainted by “grand generalizations” about “ahistorical categories” that tell us little about science that we do not already know (Ashmore, Myers, and Potter 329). These assessments seemed to confirm that our trespass onto territory already claimed by other academic workers was being treated as a hostile move, the work of gatecrashers who should be quickly dispatched. As Alan Gross put it in response to one particularly vicious review, this is an unfortunate case of disciplinary politics: “Latecomers, such as literature of science and rhetoric of science, have been regarded, understandably, as interlopers rather than as contributors to the conversation” (“Science” 449).

As the call for this special issue indicates, the next generation of books in the rhetoric of science made forays that were less bold, more “self-conscious” and “characterized by greater methodological clarity and more focused goals.” But the
reception of our work in the science studies community has changed little. For example, Celeste Condit’s careful historical research into rhetorical formations that capture the public understanding of the gene over much of the twentieth century has secured her more than a million dollars in grant money from the National Institutes of Health, but her treatment of the subject in her book is judged “rather commonplace to a historian, and if viewed critically is clearly too simple a scheme” (Turney 149). Gordon Mitchell’s book on the use of scientific argument in controversies surrounding the politics of missile defense received the National Communication Association’s Winans-Wichelns award for distinguished scholarship in rhetoric and public address, but it is dismissed by a historian of technology as poor history that “relies primarily on secondary sources to document these accounts; readers familiar with the issues will find little that is new” (Roland 159).

Given my own assessment of research in the rhetoric of science, I am tempted to identify these negative judgments as the flank-protecting maneuvers of the academic hegemony. But this would be too easy a move for me to make as a scholar whose academic community is being judged. If a reception study is to offer any lessons for the rhetorician attempting to speak to the science studies community, it will have to examine negative assessments of our field closely and charitably to see if they have any legitimacy beyond political expediency. And even if political expediency is the primary motivation of these attacks, a closer examination should help us to understand why rhetoricians are being construed as interlopers to be dismissed rather than as neighbors to be engaged; after all, there is no intrinsic reason why the territory of science studies cannot accommodate all. Is there something about the rhetorical choices made by rhetoricians of science that encourages a dismissive attitude on the part of other scholars interested in how science operates?

To answer this question, I have taken a hard look at the single case that is most familiar to me—the rhetorical construction and reception of my own book, *Shaping Science with Rhetoric*. In this book I studied three cases of scientific texts whose authors claimed to be seeking interdisciplinary collaboration, and in the spirit of interdisciplinary address, I made explicit my own goal of speaking beyond my narrow community of fellow rhetoricians, with specific appeals to scientists and historians of science who might be interested in the influence of these texts. How were my interdisciplinary appeals received by this target audience, and can a candid appraisal of the design of those appeals help account for that response?

I can afford to swallow my pride and engage in some self-criticism because, on many levels, my book was a success. The preliminary reception was especially heartening. A prominent philosopher wrote a positive review in *Science* (Rorty) that brought the book to the attention of scientists. Ernst Mayr, a scientist who was a participant in two of the cases I had studied, as well as a historian of science in his own right, initiated a correspondence with me indicating that he much enjoyed my book; in further correspondence, we engaged in a substantive discussion of the rhetoric of the principal scientists whom he knew so well. But another scien-
tist-cum-science studies scholar wrote a review that was mostly negative (Philippidis). In the months that followed, reviews by scholars interested in science writing were positive (Bowden; Gopen; Andrews), as were the reviews by my rhetoric of science colleagues (Miller; Lyne; Gross, Rev. of *Shaping*; Keith; Harris, "Shaping"). That I had effectively addressed my own community of rhetoric scholars was confirmed when the book received the Rhetoric Society of America book award. But the reception of professional historians of science was mixed. The book received a couple of positive reviews from young historians of science who had already taken an interest in the rhetoric of science and who had been adopted into our community; both argued that my research enriched the history of science (Keas; Reidy). But mainstream historians of science were less persuaded that I had something of interest to say to them. Two historians of science whose work I had cited in my book wrote reviews that, while recognizing some "strengths" in my work, were on the whole devastating (Smocovitis, Rev. of *Shaping*; Abir-Am, Rev. of *Shaping*). A closer look at what these detractors objected to is instructive.*

The most damning criticism in these reviews is that I misrepresent the history of science literature. Vassiliki Betty Smocovitis identifies this as a problem with the historiographic sections of my book:

[H]er choice, reading, and representation of the secondary literature is designed to make her point about rhetoric and not because she is deeply engaged with the historiographic debates. … This oftentimes results in oppositions between players convenient for rhetorical purposes that do not accurately reflect actual historical debates. At other times, historians are shoehorned into badly fitting or narrow categories to fit Ceccarelli’s purposes. Some historians and their accounts are entirely left out for convenience’s sake. (Rev. of *Shaping* 420)

Pnina Abir-Am makes a similar criticism, using similar language. She says I am "conveniently using" the work of historians “as empirical source material” for my “supposed interpretations” (298). At the same time, she adds, I have “systematically” obscured my debts to the previous scholars I do cite and some references are “so trivially inadequate” as to suggest that my book’s multidisciplinary appeal is mere “rhetorical strategy” (298). In Abir-Am’s final paragraph, she offers her hypothesis for why my characterization of the relevant history of science literature is so deficient:

Perhaps spending so much time in the company of a rhetorical perspective which, by her own account, is often regarded as ‘amoral empty talk that politicians use when

*After I completed this article, a third review by an established historian at science came out in *Isis* (Dietrich). It followed the same pattern as the other two.*
they skirt the real issues in public debates’ (p. ix), the author gave no formal attention to the ethical dimension of rhetoric, especially its deceptive potential. Yet, somehow she managed not to miss its gist in the practice of seeking to please everybody by such renowned means as long lists of superficial citations, failing to cite invalidating and/or preempting works by others, and chasing some elusive mainstream consensus that is presumably the key to rising in the academic interdisciplinary fields of science studies. (Rev. of *Shaping* 298)

The fact that I did not please Abir-Am or Smocovitis with my characterization of the history of science literature is proof positive that such sophistry, if it were my intent, is not the key to rising in the interdisciplinary fields of science studies. But what are we to make of the charge by both of these historians of science that my characterization of the literature is mere rhetoric, unethical in its systematic twisting of the literature to my own purposes? It is not unusual for historians of science to critique books for failing to cite the right people in the right way. In Smocovitis’s recent review of a book by Robert E. Kohler, she complains about the way this “veteran historian of science” fails “to engage previous historical scholarship,” especially when it comes to his treatment of the evolutionary synthesis (Rev. of *Fielding* 1666–65). She continues: “[H]is treatment here is oversimplified at best and often misleading. Similar patterns of limited or no engagement with established historical scholarship are repeated throughout the book” (1666). But notice how Smocovitis’s critique of a historian’s characterization of the literature does not imply that the error is a “convenient” one; although the oversimplification served his interests, she does not claim that the deception was a deliberate violation of academic ethics, dishonorable in its selfish “rhetorical” intent.

Neither historian gives a specific example of how my reading of the literature is inaccurate, nor do they point to a specific claim I make that is invalidated by a historian’s account that I ignore “for convenience’s sake” (Smocovitis, Rev. of *Shaping* 298); this makes it difficult for me to assess the content of their critique. In addition, neither reviewer questions my reading of the primary texts or my argument about their influence on the history of science. In fact, in a review that concludes with four paragraphs of devastating attack, Abir-Am begins with three paragraphs of praise for my book’s “several attractive features that should commend it for a wide readership” (Rev. of *Shaping* 295), followed by five paragraphs of neutral description, and then three more paragraphs of praise for such historical contributions as my “unrelenting pursuit of numerous reviews” (a practice for which she “cannot commend the author enough”) and my “particularly interesting” demonstration that seemingly conflicting historical interpretations of a passage in Schrödinger’s book were all adopted by different interpretive communities at the time (Rev. of *Shaping* 297). She even cites me in a later article as the most recent author to have discussed the impact of Schrödinger’s book, thus directing other
historians to my work ("DNA" 199). So as history of science, my book was not a complete travesty. But the fact that both historians who critiqued my book shared a belief that I mischaracterized their field of study, and the fact that both attributed it to the questionable ethics that they assumed were embraced by the professional rhetorician, is significant.

The lesson that I take from this critique is that latecomers to the conversation cannot jump in with an outsider’s depiction of the established literature and hope to carry the day. This is an especially important lesson for rhetoricians, who have the burden of overcoming a presumption of sophistry that adds a moral component to perceived academic error. Had I engaged in a dialogue with either Smocovitis or Abir-Am before publication of my book, seeking advice on which scholarship I might be missing in my characterization of the literature, I would have been more likely to escape the charge that I was deliberately misreading their work to serve my own purposes. Additionally, had I not tried so hard to carve out a place in the history of science literature for my own research, I would have been more likely to describe their field in a way that they would recognize as accurate and fair. As Smocovitis points out, in my attempt to promote the value of rhetoric as a field of study, I failed to respect the viewpoints of those I addressed: “[Ceccarelli] points to numerous examples of alleged disputes (both major and minor) in the history of evolutionary and molecular biology centering on these texts and then argues that her new rhetorical readings show that historians have been arguing needlessly” (Rev. of Shaping 419–420). Thanks to Smocovitis and Abir-Am, I see now that my representation of the rhetorician as a newcomer who can offer, at the level of textual analysis, a unification of conflicting explanations in the history of science was imprudent; if the conflicts I saw truly exist in the history of science community, the last thing they need is an outsider pointing them out and then with a sweeping gesture claiming to have the solution!

A related criticism of my book offered by both these historians of science was my failure to recognize the work of historians of science who are already doing rhetorical scholarship. As Smocovitis puts it, I appear unaware of the fact that intellectual historians “have been engaging in precisely the kind of ‘close reading of texts’ and rhetorical inquiry” that I am promoting in the book (Rev. of Shaping 420). Abir-Am cites two articles of her own on the development of molecular biology that include the word “rhetoric” in the title, and she complains that neither article is cited in my book (Rev. of Shaping 297–98). Although these articles focused on an earlier and a later period in the development of molecular biology than the period in which Schrödinger’s book was inserted, and a cite of them just to show they existed would seem a crass tactic of trying to please scholars with superficial lists of citations, I am forced to admit that my neglect of these articles was due as much to my failure to recognize them as rhetorical scholarship as to their apparent lack of relevance to the subject of my case study. They do not cite the literature of
my field, nor do they make use of the tradition of rhetorical scholarship in which I have been trained.

But this is a shortsighted perspective. My field does not have a trademark on the word “rhetoric.” In my book, I tried to differentiate my own rhetorical focus on the microstructures of textual strategy from the diachronic narratives that I was finding in the accounts of historians. But this was a misguided effort. There are plenty of professional historians who see themselves as doing a close reading of texts, and there are plenty of professional rhetoricians who see themselves as engaging in a diachronic study of institutions and cultural practices. Rather than focus on the broad differences between my approach to rhetorical scholarship and the approaches to historical scholarship that I was finding in literature, I would have done better to acknowledge and even emphasize similarities between my work and the work of rhetorically sensitive historians, pointing to the places where the research of both sets of professionals can comfortably overlap.

In both of these cases of criticism of my book by historians of science, it was my characterization of the history of science literature that caused the most censure. As Gross has argued, the suggestion “that literary and rhetorical scholars have no right to comment on science because they have not even mastered the literature” is a common topos of disciplinary politics (“Science” 448). But rather than dismiss their complaints as nothing more than politics as usual, I plan to take them seriously. Here a lesson from the rhetoric of interdisciplinarity in science is fitting—scientists who write texts that successfully catalyze the unification of fields have developed the skill of negotiating the professional interests and assumptions of the different fields based on an intimate knowledge of both, or at the very least, an insider’s knowledge of the field that has the most to lose by giving up ground in an interdisciplinary conversation. The lesson from my close reading of the critical reviews of my own book is that I must not only attend to the conversations of historians of science by doing what I consider to be a careful literature review that carves out a space for my own work; I must engage in a dialogue that helps me to better see those scholars as they see themselves. By doing so, I can begin to position my work in relation to theirs in a way that is nonthreatening and nonirritating and can work to overcome the presumption that my field is deceptive and immodest.

The specific problems with my book may or may not be representative of the difficulties encountered by other rhetoricians of science who are being dismissed by critics from the history, philosophy, or sociology of science. Other rhetoricians of science will have to take a hard look at the interdisciplinary reception of their own work to determine this. Such candid self-analysis is unpleasant, but if we are to improve the status of our discipline, it is a necessary first step. The critiques of our work by the “established” science studies disciplines may or may not be fair, but they tell us a lot about how our appeals are being received and thus can help us to modify those appeals, if appropriate, to better communicate our worth to our academic neighbors.
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