

Keeping the Rhetoric Orthodox: Forum Control in Science

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Academic disciplines certify knowledge through publication in scholarly journals; therefore, peer review of journal articles is one method of authorizing someone's speech. It is possible, however, to see peer review and other strategies as methods by which elites silence or de-authorize voices that pose a threat to their status. This article discusses four methods of forum control—peer review, denial of forum, public correction, and published ridicule. Examples are drawn from cases in science.

Orthodoxies practice forum control, a supervision of official publications and speech, which may result in the denial of access to the community's authorized means of communicating with its members. For example, in the early Church, the writer of the Second Epistle of John warned his readers not to allow teachers holding heretical views to have access to the meetings of the Church (verses 7-10). Early in the second century, Ignatius, Bishop of Antioch, recommended that "communication with false teachers cease utterly" (Schoedel 12). At the end of the second century, Irenaeus published *Against Heresies*, a treatise containing five volumes, which stigmatized Gnostic teachers, effectively removing them from orthodox forums of the early Church (Sullivan, "Identification"). When Galileo tried to circulate his *Dialogue Concerning the Two Chief World Systems* widely, the Pope stopped its circulation (Biagioli 330), and eventually the book was prohibited by public edict (Finocchiaro 291).

Forum control of this kind need not be considered sinister even though it is nothing short of censorship. Religious or ideological groups, special-interest groups, or specialized groups (such as professions or disciplines) should have, in my opinion, the freedom and the responsibility to define their own boundaries, control their own definitions of membership, and certify that publications that receive

their imprimatur represent orthodox views. Although techniques of forum control are necessary, they are, nevertheless, exercises of political power, and they are sometimes used in unethical ways. Some of the examples of forum control in this article seem to be abuses of power, but not all are, and the practice of controlling a forum is not by definition unethical. To establish a set of criteria by which we may determine whether or not a particular instance of forum control is ethical is beyond the scope of this article; indeed, such an article would require wider knowledge of circumstances, a deeper understanding of the philosophy of ethics, and more clarity of moral judgment than I can lay claim to. The scope of this article is restricted to a study of how forum control is practiced as a rhetorical act; therefore, it should be read as a study of the dynamics of forum control rather than of the ethics of forum control.

Although forum control can be identified in almost any social setting—for example, a family meal, a tribal council, a departmental meeting, a worship service—in this article I am particularly interested in the way it is practiced in the scientific community. I have not restricted the study to a particular field of science; instead, I refer to science and deviant science in general terms, a practice which may give the impression that I think of science as being monolithic. I applaud Charles Taylor's approach in which he explores "how competing research communities within traditional science construct interdisciplinary demarcations so as to advance proprietary interests over particular research domains . . ." (*Defining Science* 5). However, my focus in this article is not on the boundary struggles among research communities but rather upon generalized techniques of exclusion. Therefore, this study refers to science as a socially constructed unity, which, I believe, operates as a mythic unity among many practicing scientists and within the public consciousness. Thomas Gieryn's "Boundary-Work and the Demarcation of Science from Non-Science" describes the rhetorical constructions which have been successful in creating this mythic unity.

Sociologists of science often refer to science as either deviant science or as orthodox science. That science today is referred to as an orthodoxy is not surprising considering that it has certain characteristics that are often associated with orthodoxy: it has an authoritative institutional structure (Elias), a body of malleable doctrine (Kuhn), definitive hierarchical and power relationships (Cole and Cole), and a tendency to turn doctrine into dogma when its authority is threatened (Lessli; Taylor, "Of Audience"). Although these characteristics are part of what I mean when I refer to an orthodoxy, I also have in mind the root meanings that come to us from classical Greek, *orthē* (straight) and *doxa* (opinion). Orthodox speech, therefore, is speech that represents the community's common vision (Rosenfield 65), and the authorized spokesperson embodies the *ethos* of the community (Halloran, "On the End" 621).

It is important to study forum control in orthodox science, I believe, because science is popularly believed to be driven by an objective search for truth. In popular thought, scientists discover truth rather than construct it, and the truth discovered is readily apparent to the unbiased reader if she or he is intelligent enough to understand scientific language. It is by now obvious to those who have followed studies in the sociology and rhetoric of science that this popular image is inaccurate. As Michael Ruse says, "science simply cannot be taken at face value" (12). In some cases, it may actually be a politically motivated enterprise that seeks to maintain power for any number of reasons, not the least of which is continued funding. It follows that the study of scientific discourse should be open to analysis of what Andrew King calls the rhetoric of power maintenance. King shows that several strategies, such as ridicule, crying anarchy, setting impossible standards, and co-optation, are used repeatedly by those in power when they try to debunk competitors. Whereas King focuses on instances of these strategies within speeches, the present study goes beyond King's in arguing that whole texts or speeches can be seen as attempts to control the content of what is said or to close off discussion. To achieve these ends, a group must control its forums, and to do that it needs to be able to authorize and de-authorize those who would speak as representatives of the group.

"Authorization," therefore, is an important concept in this article. To "authorize" is to grant license to speak in a recognized forum. To be authorized and to have legitimacy are similar conditions. As John Schutz points out, "concepts of authority and legitimacy have been collapsed and equated" (6); however, they are not the same (7). Whereas "authority is the interpretation of power" (3), legitimacy is a condition of respect attributed to someone who holds an office. The office lends legitimacy. Authority is a condition of respect derived from the rhetor's apparent embodying of knowledge and power. Authority may be defined as charismatic power; legitimacy may be defined as institutionalized authority. To authorize speech is to give official recognition to the rhetor's authority, to give to the rhetor the group's imprimatur (literally "a license to publish"). As Chaim Perelman and Lucie Olbrechts-Tyteca point out, a judge (substitute the word "referee") "is qualified to judge not because he [gendered pronoun in the original] is neutral . . . but because he is impartial: being *impartial* is not being *objective*, it consists of belonging to the same group as those one is judging, without having previously decided in favor of any one of them" (60, emphasis in original). Authorized speech, therefore, is that which qualified representatives of a community have deemed worthy to represent the group. The authorization process is circular: orthodox judges, who rise in status by being recognized as authorities within the current power structure, serve as gatekeepers and perpetuate orthodox perspectives. Like the legislators

Michael Halloran studied in "Doing Public Business in Public," referees are concerned with representation. In the publication of scholarly journals, they present "an image of the community to the community" (Halloran 122), and in so doing define it.

In some cases, the person speaking or writing is not recognized at all, as when a software writer documents a program for users or when a public relations writer sends out a press release. In such situations, the speech itself is authorized rather than the rhetor. Authorization under such circumstances is the official sanction of the speech as representing the organization's position. Stephen Doheny-Farina analyzed the struggle for identity experienced by a woman who wrote for a family-planning clinic as she learned to say things that represented the clinic's rather than her own convictions. By submitting text for review that represented what the directors of clinic desired to put forward, the writer became a medium for the voice of the clinic. Are such writers authors? It is only when the writer "articulates and rearticulates meaning" that she can be thought of as an author, regardless of whether or not her name appears on the document (Slack et al. 31). On the one hand, the writer was articulating and rearticulating meaning, but ultimately, the demands of the organization's image overwhelmed the voice of the writer. It follows that such authorized speech, because it is identified with those who authorize it rather than with those who write it, is less open to authorial freedom than other types of speech; in fact, the more strict the orthodoxy, the less room there is for authorial freedom.

We can define forum control, therefore, as the process of authorizing or de-authorizing speakers, writers, texts, or speeches. Over the past several years I have collected information about controversies in science that exercised forum control in explicit ways. Most of these instances were attempts by orthodox biologists and paleontologists to expunge creationist views from scientific forums. However, thanks in part to input from referees who read an early draft of this article, I have expanded the collection to include instances of forum control involving other issues. This collection has finally grown large enough so that it is possible to divide forum control into four types: peer review, denial of forum, public correction, and published ridicule. Two of the four instruments (peer review and denial of forum) are non-public acts; the other two (correction and ridicule) are public acts that, although responsive to earlier discourse, attempt to forestall future unauthorized speech. This distinction is similar to H. M. Collins and T. F. Pinch's division of the modes of rejection: "Implicit rejection operates when rival knowledge claims are ignored by orthodoxy, whilst explicit rejection is characterized by controversy where the objects of dispute are articulated by individual scientists or opposed groups of scientists" (239). However, the similarity between my categories and those of Collins and Pinch goes only so far as to recognize one as private and the other as public.

Peer Review

Several studies have explored the role of peer review in the formation of certified knowledge. Peer review, as an instrument of authorization, works most benignly when reviewers help the writer to improve the text submitted for review or to find ways of making the text appear to be relevant to readers within the field. Such review borders on collaboration as reviewers help writers find connections between their own message and the concerns of the community. A good example of peer review that works benignly is the review process that I experienced in response to the first draft of this article. Knowing that the topic of the article was important and that the gist of my work made sense, I submitted it for review, even though it was not very well developed. All three reviewers confirmed my judgment that the topic was fascinating, but a couple commented that my attitude toward the process of forum control was ambiguous, causing the readers to be uncertain about underlying agendas. The reviewers also pointed to several other potential cases of forum control that could be used to help support the article's argument. In short, the peer review worked just as I hoped it would—it dislodged me from a condition of writer's block brought on by not being able to see the article in the context of peer readings and interests. Reading the reviews, I was able to see how to contextualize my work and how to bolster it.

To be relevant, a contribution to the growing body of doctrinal knowledge must be woven into the fabric of existing knowledge, and the writer must use the language conventions of his or her audience (Foucault; Overington). James Porter explains that, to achieve relevancy, "The discourse must demonstrate that it contributes knowledge to the field, it must demonstrate familiarity with the work of previous researchers, it must conform to the accepted standards for research design, and it must show sufficient data to support its conclusions" (108). Others refer to the process of making speech relevant when they say that the writer must place "her work within an intertext" (Berkenkotter and Huckin 58), or must decontextualize the work done in local settings and recontextualize it within a framework of interest to the discipline (Knorr-Cetina 99). Because of this requirement, scientific change is conservative even when it advances: "To label a paper with the honorific 'new' is not to disparage the past but to credit its authors with having mastered a set of tacit practices and conventions that enable them to 'use' the past in order to transcend it" (Kaufer and Geisler 288).

Despite the value of peer review in helping to secure relevancy, it can also be seen as having a coercive effect on writers. The review process drives authors toward reinforcing the dominant view of the discipline. As Greg Meyers' analysis of revisions of a proposal written in response to peer review shows, later versions of the proposal showed a closer fit between the scientist's work and the discipline because it

related the scientist's work to the "consensus of the field" (53). Similarly, Daryl Chubin and Edward Hackett point out that peer review in science works against innovation, and, notwithstanding the perception that scientific research is "grounded in principles of free and open inquiry, yet journal peer review can create and reinforce scientific dogma" (90). In Carol Berkenkotter and Thomas Huckin's study of a biologist who adapted her paper to suit the reviewers, they conclude their comments by describing her attitude toward the changes: "Her distaste for the disingenuous, her cynicism regarding the 'phony story' she had to construct, suggests that she was very aware of the difference in recounting local history in her lab and contextualizing that history within a narrative framework, and that she considered only the former as constituting true science" (58). For many, creating the "phony story" of the science article is just part of the genre:

There are, to be sure, minor deceptions in virtually all scientific papers, as there are in all other aspects of human life. For example, scientific papers typically describe investigations as they logically should have been done rather than as they actually were done. False steps, blind alleys, and outright mistakes are usually omitted once the results are in and the whole experiment can be seen in proper perspective. (Goodstein 32)

For others, the constraints imposed by peer review in order to achieve authorization seem coercive, forcing the writer to be disingenuous or even silencing the voices of some who would seem to have a right to speak in the forum.

It is not unusual to hear complaints that juried publications censor the views of those who do not agree with those in power. Patrick Moore, for example, in "Instrumental Discourse Is as Humanistic as Rhetoric," claims that journals in the field of technical communication are controlled by rhetorical theorists who exclude from publication those who hold an instrumentalist view of technical communication. It is often difficult to distinguish legitimate claims of suppression from bogus claims because reviewers are not always impartial; they often have goals or hold ideological views that affect their judgment about what constitutes authorized and unauthorized speech. Conversely, those charging that suppression has occurred may attribute motives to reviewers that they do not have and may be poor judges of their own work.

Celeste Condit gives as an example a case that seems to be a clear abuse of peer review, which involved her own attempt to publish a letter in the journal *Science*. Her letter was a response to an article titled "Sex Differences in Regional Cerebral Glucose Metabolism," which had appeared earlier in the same journal. In her letter, which now appears in *Rhetoric Society Quarterly*, Condit argues that scientists' decisions about what kind of research to do and about what data to look for are influenced by ideological assumptions and that a different set of assumptions would have led the researchers in question to

conduct a different study and to look for data to support different categories. Her letter was sent out for review and rejected because reviewers claimed that it was “emotional” and “ideological” (91), and that it requested research that was beyond the scope of the original article. Condit’s analysis of other letters published in the journal shows that the first two criticisms are justifications for the decision not to publish rather than criteria that mark authorized speech in the journal. The third criticism may indeed be true, but the authors’ inability to conduct the research suggested by Condit seems a poor reason to deny publication of her letter.

In summary, peer review as an instrument of forum control takes place in private, the reviewers serving as representatives of the larger community. Because they are members of the group to which the paper is directed, they can be impartial, but not objective, judges of whether or not the material is of high enough quality to be published, as well as whether or not it is likely to be of interest to the readers of the journal. Instructions sent out to referees often ask them to comment specifically on this factor. For something to be interesting it must connect with what is already known in some way. Entirely novel material is not interesting because the reader cannot find ways of attaching it to her own growing schema of knowledge; in other words, entirely novel material lacks relevancy. Similarly, heretical views may so challenge business as usual and existing power relations that peer reviewers reject work not because of its inferior quality but because of its threat to those in power.

Denial of Forum

Although peer review of papers submitted for publication may be the most well known and accepted method of denying forum, it is important to look beyond peer review of articles: the private action of denying forum takes many forms other than rejecting a paper for publication. It seems that people who espouse minority opinions on controversial topics—such as origins, environmentalism, AIDS research, and sexual identity—are most likely to be denied forum. Creationists have been the most persistent of those excluded from modern science. Because they usually share religious beliefs as well as views about origins, creationists have formed organizations on the margins of science and have produced a large body of alternative publications, some of which focus directly on the issue of exclusion. Dozens of cases in which creationists have been banned from the forums of science, or denied the credentials that give access to scientific forums, have been documented in Jerry Bergman’s *The Criterion*. Graduate students have been denied Ph.D.s despite excellent records because they were creationists; faculty have been denied tenure on the basis of this one issue; others have been dismissed from employment.

Perhaps the most widely publicized case of forum denial is that of Forrest M. Mims III, an amateur scientist with dozens of publications in magazines like *Modern Photography*, *Physics Today*, *Popular Electronics*, *Electronics Hobbyist*, and *PC Magazine* and books published by McGraw-Hill, Prentice Hall, and Radio Shack, with total sales exceeding three million copies (Eastland 32). I turn to this case because it is well known and because it is clearly a case of forum denial based on the writer's heretical private views rather than on the quality of his publications. Although Mims does not hold an advanced degree in any of the sciences, he has made a career out of being an amateur scientist. The Mims case involves access to a popularizing forum rather than to a major research journal; nevertheless, it shows the process of forum control at work.

As Robert Felt tells the story, C. L. Stong, the longtime author of *Scientific American's* "Amateur Scientist" column, once told Mims that he would be writing the column one day. In 1988, Mims let the magazine know that he was interested in the position, and so, in July of 1989, the senior editor, Jonathan Piel, phoned Mims and asked him if he would like to take over the column. Evidently the only one being considered for the position, Mims was flown to New York, where he went through a day of interviews. All went well until Piel asked what other publications Mims wrote for. When his answer included some Christian publications among the others, Piel asked Mims if he accepted Darwin's theory of evolution. He said he did not. Mims' beliefs on this subject seemed to him to be inconsequential because the column was not one in which the subject of origins or of evolution would come up (it was a column in which experiments suitable for amateur scientists were described). Nevertheless, *Scientific American* decided not to hire Mims as the column's author, despite the editors' acknowledgment that his sample columns were excellent. They agreed, instead, to publish three of his columns in exchange for Mims' agreement not to seek legal action on the grounds of discrimination.

The case caused quite a stir, with stories running in the *Wall Street Journal*, the *New York Times*, and the *Washington Post* in late October of 1990. It became the topic of several letters and commentaries in such publications as *The American Spectator* (Eastland) and *Skeptical Inquirer* (Felt; Mims; Gardner). The Texas affiliate of the ACLU wrote a letter to *Scientific American* expressing dismay over the magazine's apparent discrimination on the basis of religious beliefs. The AAAS Committee on Scientific Freedom and Responsibility wrote a letter to Mims expressing its commitment to the idea that a person's private beliefs should not serve as criteria for judging articles submitted for publication.

The case of Forrest M. Mims III is an interesting case of forum control by orthodox science. Because *Scientific American* is considered a respectable scientific magazine, its editors must be concerned about its reputation and about maintaining its orthodox image. Had he been

an occasional contributor, Mims would not have posed a problem for the editors, but because he was to be the author of a regular column, his beliefs were a source of potential embarrassment. Someone could claim that Mims had received authorization to speak as a representative of the magazine, which in turn enjoys the authorization to represent science to the wider public. Rather than risking a “public relations nightmare,” as Piel called it, the editors chose instead to deny Mims a forum for which he was well qualified (Eastland 33).

It is not just creationists who claim to have experienced suppression in science. Brian Martin has done much to publicize suppression of scientific speech related to environmental topics. In *Intellectual Suppression*, Martin and his co-editors document cases that involved the cutting of research grants and denial of promotion which seem to be directly tied to scientists' making controversial claims about the effects of certain practices and chemicals on the environment. Recently, he has begun to publish material that has been denied publication elsewhere on a Web site at the University of Wollongong in Australia. Others are now claiming that they are being denied forum because their findings threaten powerful and highly funded AIDS research. A paper by Louis Pascal, which suggests that AIDS originated in polio vaccines in the late 1950s in Central Africa, was denied publication but is now available at Martin's Wollongong Web site. Peter Duesberg, a professor of molecular biology at Berkeley, successfully published a paper in which he claimed that HIV does not cause AIDS, but Peter Gorman claims that it was published only because of his reputation as being one of the world's leading experts on retroviruses and that he has now fallen into disrepute among scientists because of the views expressed in the paper.

I have chosen to carve out this second category of forum control—denial of forum—in order to point out that peer review is only one of several means of denying forum. What all the instances share in common is their preemptive nature and their privacy. Rather than responding to speech that has already appeared in the community's forums, it keeps heretical speech out of the forums, selecting in advance which opinions are worthy of consideration and discussion. Furthermore, denial of forum often involves an assessment of the character and overall orthodoxy of the would-be speaker, as the case of Forrest Mims shows.

Correction

Peer review and denial of access to forums are non-public acts that cultivate authorized speech or suppress unauthorized speech. Correction, conversely, is a public rhetorical act that regulates how insiders use scientific forums. No one doubts that science needs to be monitored, as the respective cases of David Baltimore (Hilts) and

Fleischmann and Pons make clear (Gieryn, "Ballad"). The Baltimore case is named after David Baltimore, a thesis director for Thereza Imanishi-Kari. Imanishi-Kari was the primary author of an article which appeared in *Cell* in 1986, but Baltimore's and three other scientists' names appeared as co-authors. It turns out that the article reported findings based on work that was never done and that it reported results that were contrary to actual findings. It is named after Baltimore because he was the advisor and because he refused to investigate Imanishi-Kari's notebooks when problems with the work first began to appear. Fleischmann and Pons are the scientists who announced that they had come up with a process that produced fusion through a chemical process, a process known as cold fusion. Their claim was soon discounted by the scientific community, but because they had announced their discovery in a news conference, their findings were publicized on national news broadcasts, and the general public has been slow to acknowledge that cold fusion is still only a dream.

But orthodoxies tend to tighten strictures when members feel threatened, and this tightening may lead to public chastisement. That some orthodox scientists feel under attack is clear, as this sentence in *Higher Superstition* makes clear: "In view of the enormous range of left-wing criticism of science in terms of philosophical assumptions . . . the only way to compose a coherent rejoinder within a reasonable space is to examine a range of specimens" (Gross and Levitt 13). Congruently with science defining itself more strictly in the face of public challenges like those discussed in *Higher Superstition*, it also seems to have become less tolerant of minor procedural improprieties and of doctrinal dissent within its ranks (Lessl). Evidence of this increased will to discipline appears in a wide range of corrective texts in scientific journals and magazines, from the relatively mild reprimand of a respected colleague who fails to observe proper procedural etiquette, to the censure of a colleague who holds and teaches unorthodox views. A reprimand doesn't silence the subject; a censure is an attempt to silence.

A good example of a mild reprimand is John Maddox's "Extinctions by Catastrophe." In his role as the senior editor of *Nature*, Britain's leading general science journal, Maddox is the author of many columns, or editorials, about current scientific issues. Also, because of his position and because he has the full credentials of a legitimate scientist, he has great symbolic authority within science, sometimes even being referred to as the "mouthpiece of the orthodox scientific community" (Picart 15, 16).

In "Extinctions by Catastrophe," Maddox addresses the difficulties caused by circulating pre-prints of an article before the article appeared in the February issue of the *Proceedings of the National Academy of Sciences*. The authors of the article, David M. Raup and J. John Sepkoski, had distributed pre-prints, in which they had shown statisti-

cal evidence for a regular reoccurrence of mass extinction events every 26 million years, and had called on astrophysicists to give an extraterrestrial explanation for the cause of such evenly spaced extinctions. Some astrophysicists who received the pre-prints responded so promptly that their first drafts reached *Nature* before the Raup-Sepkoski article had been published, causing no little consternation at *Nature*, where it was impossible to judge the quality of a response to an article which had not yet appeared.

So, in the April 19, 1984, issue of *Nature*, when the five articles responding to Raup and Sepkoski (and giving explanations for the apparent periodicity of mass extinction events) appeared, another article, written by Anthony Hallam, also appeared that raised questions about Raup and Sepkoski's findings. The appearance of a controversy apparently in media res appearing on the pages of *Nature* evidently seemed incongruous to the editors, and so Maddox wrote an editorial in which he chastised Raup and Sepkoski. In his comments, Maddox tells readers not to blame the authors (Raup/Sepkoski and Hallam) for their apparent conflicting hypotheses; rather, they should blame "the conventions, acceptable or otherwise, which regulate, and sometimes fail to regulate, such developments" (685). Maddox goes on to discuss "proper" publishing and distribution procedures. Re-counting the history of the Raup-Sepkoski paper and their apparent circulating of pre-prints, Maddox complains that the practice is "discriminatory, excluding from the circle of those in the know people who happen not to be on the authors' mailing list." Furthermore, this practice produces "a kind of nonsense." He then instructs the readers about proper behavior in such situations:

In the normal course of events, it is entirely proper and indeed essential that people should talk about their discoveries at conferences, and give their colleagues advance notice of what they plan to publish. . . . But the concatenation of all these events can create serious doubts in people's minds about the proper attribution of novel ideas to the participants. (685)

Maddox's editorial is a mild reprimand: he does not overtly blame Raup and Sepkoski for circulating pre-prints, but he points out the trouble it has caused and the potential for producing nonsense, a veiled crying of anarchy (one of King's rhetorical techniques of power maintenance). It is clear that Maddox intended to rebuke Raup and Sepkoski but to do so with the finest of British gentility.

Whereas reprimand is a mild form of correction, the formal censure of a colleague is far more severe. The story of Richard Ikenberry, a now-retired professor of microbiology at the University of Nebraska at Kearney, is a case in point. I became aware of this case while teaching in Nebraska and followed up on it by interviewing Ikenberry several times and collecting the documents in the case. A Ph.D. from Iowa State University, a full professor for over twenty years, and a former evolutionist, Ikenberry became a creationist after

extended study of the subject. Not only did he change his own mind, he decided to let his students know about it, and so he offered a public lecture on the subject in February 1985. The student newspaper, the *Antelope*, announced it in the January 31, 1985, issue. Six days later, a letter, signed by Dr. Ikenberry's colleagues in the biology department, appeared in the same paper. In it they denounce creationism as "an invalidated hypothesis . . . [which] has no place in 20th century science." A protracted debate followed in the same paper. On February 7, an unsigned editorial tried to establish a compromise by claiming that Darwin believed God created through evolution, but in the same issue, a letter from a physics professor contained a vitriolic attack on Ikenberry. Soon after, on February 12, a number of student letters appeared, all defending creationism. In turn, as can be imagined, more establishment figures voiced their opinion that evolutionary science is valid and creationism isn't. The debate continued for some time, drawing in students, faculty, graduate students, and campus ministers. Eventually, the intensity of the debate died down, but the issue did not go away.

Over the next few years, Ikenberry's department held two special meetings to deal with his apparent heresy. Meanwhile, Ikenberry continued to give talks supporting creationism, mostly in church groups, but also in special optional sessions of his classes. Furthermore, he would occasionally offer anti-evolution and pro-creation comments in his lectures. Finally, members of the biology department persuaded the Dean of the College of Natural and Social Sciences, Hal Bertilson, to write a letter of censure to Ikenberry (dated February 6, 1991). He also published an article in the college's scholarly journal, *The Platte Valley Review*, in which he discusses Ikenberry by alluding to a parallel example of deviancy from within his own discipline:

[I]t is a violation of responsible portrayal of knowledge for a professor to give students the impression that the dominant view in psychology is that animal rights are as important as human rights and that, consequently, animals should not be used in experiments. . . . Research consistent with [the ethical standards of the American Psychological Association] in the interest of creating new knowledge is not only appropriate but morally required. (11)

Ikenberry responded to the Dean's first letter privately; however, not persuaded by his arguments, Bertilson issued a second letter of censure (dated January 9, 1992), in which he insisted that Ikenberry desist from promoting creationism upon threat of his own further "intervention."

Censure, as this case shows, is an attempt to silence deviant insiders. Ikenberry had already achieved access to one of science's most important forums—the college classroom—but he began to use that forum to teach, in a limited way, his own heretical doctrines. It took a formal censure with an unspecified, but ominous, threat from his supervisor to control the forum, and even then Ikenberry was not

silenced. He continued to give lectures in alternative forums on creationism. The biology department countered by creating a new, required course in the evidences for evolution—a kind of evolutionary apologetics course.

What do these cases show about correction as an instrument of power maintenance? Reprimand and censure, unlike denial of forum, are reactive instruments of forum control. They seem to be employed when speakers who are already recognized as authorized spokespersons for the community engage in practices or advocate opinions that upset orthodoxy in some way. As methods of disciplining or of chastising those on the inside in an attempt to keep the rhetoric of insiders orthodox, correction and censure require either that someone in a superior hierarchical position “pull rank” (as in the Maddox example) or that the community band together to define the behavior of one of its members as unacceptable (as in the Ikenberry case).

Ridicule

Whereas correction is a public reprimand or censure of insiders, ridicule, usually in the form of an exposé, holds heretics up to public scorn in displays of derision, attacking heretical belief and usually denying opportunity for rejoinder in the same forum. From the perspective of the orthodox, those who write exposés are the “bonny fighters” for the faith. Despite his somewhat controversial character, G. K. Chesterton gained a reputation as Christianity’s bonny fighter because he was willing to argue with well-known writers and thinkers in the early twentieth century. For instance, in *Heretics*, he takes on Rudyard Kipling, Bernard Shaw, and H. G. Wells.

Similarly, even though Stephen Jay Gould is a mildly controversial figure within science (Lyne and Howe; Miller and Halloran), he has emerged as orthodox science’s popular apologist. Much like Chesterton, he is a well-read scholar and a gifted stylist, or as he puts it, a “totally unconscious writer” (Haynes 64). His *An Urchin in the Storm* is a collection of critical exposés very much in the mode of Chesterton’s *Heretics*. He writes widely for scientific journals and magazines (Lovejoy), and authors a monthly column in *Natural History*.

I have chosen two of his book reviews for closer inspection: “On the Origin of Specious Critics” and “Impeaching a Self-Appointed Judge.” These two book reviews qualify as exposés. Exposés work by creating a temporary and qualified identification with their subjects, while simultaneously creating a disassociation from them (Sullivan, “Identification”). They expose the nakedness of their subjects and adorn them in satirical attire. They usually employ four techniques of power maintenance described by Andrew King: ridiculing, crying anarchy, setting impossible standards, and co-opting.

In "On the Origin of Specious Critics," Gould reviews Jeremy Rifkin's *Algeny*, in which Rifkin argues against genetic engineering. Part of Rifkin's strategy in this book is to argue that traditional Darwinism reflects the economic conditions of the industrial revolution and is about to be replaced with an information-based model of science which reflects modern society's infatuation with the computer. To make this argument, Rifkin tries to show that Darwinism is a theory in disarray. In "Impeaching a Self-Appointed Judge," Gould reviews *Darwin on Trial*, by Phillip Johnson, a member of the law faculty at Berkeley. Johnson's book was considered, at that time, to be the best argued popular exposé of the major philosophical and logical weaknesses of Darwinism (Michael Behe's *Darwin's Black Box* has now replaced Johnson's book at the top of the list—for a critique, see reference to Orr).

Both reviews exhibit the characteristics of an exposé outlined above. In both, Gould establishes identification with his subject by showing shared concerns. Rifkin is concerned about genetic engineering's possible consequences: so is Gould. Johnson participates in interdisciplinary research: so does Gould. He rebuts the arguments of both, using practically the same arguments: both Rifkin and Johnson are guilty of holding up supposedly unrepresentative statements by scientists; both quote scientists only partially, thereby taking their words out of context; and both bring up old errors long since corrected by science. He exposes the nakedness of the books' arguments by pointing to a few "factual errors"; and he simultaneously adorns both in satirical garb, calling Rifkin a "specious critic" and Johnson a "self-appointed judge." He ridicules both (Rifkin's scholarship is "slipshod"; Johnson's book is so poorly written that Gould's third grade teacher would rap his knuckles for it); he sets impossible standards by demanding that both authors have state-of-the-art knowledge of science equivalent to Gould's; and he co-opts both, showing himself to be as concerned about genetic engineering as Rifkin and as sensitive to the Bible as Johnson.

The ultimate rhetorical effect of both exposés is to silence the voices of the authors and thereby to control the scientific forum. Gould tries to make a case that these books are not worth reading and certainly not worth discussing. One could argue that he is placing them on the index of heretical and dangerous books. Branding them in this way puts them outside the pale of orthodox reference in the same way that serious reference to Bloom's *Closing of the American Mind* has been ruled outside orthodox practice.

Before departing from this discussion of ridicule and exposé, I feel that, in fairness to science, it is important to acknowledge that the same sort of thing happens in the humanities. When Alan Sokal published his now notorious "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity" in *Social Text* and then revealed that it was a hoax, much to the embarrassment of the

editors and to many in the humanities, no less a “bonny fighter” than Stanley Fish sallied forth on behalf of the duped. He wrote an exposé just as biting as any written by Stephen Gould. It was titled “Professor Sokal’s Bad Joke,” and it appeared in the May 21, 1996, edition of *The New York Times*.

Fish uses many of the same rhetorical strategies as Gould. He claims that Sokal misrepresents those he criticizes. Referring to Sokal’s claim that social constructivists believe that the physical world has only socially constructed properties, Fish says, “none of his targets would ever make such statements.” He goes on to suggest that Sokal is misinformed, correcting Sokal’s definition of social constructionism:

Professor Sokal takes “socially constructed” to mean “not real,” whereas for workers in the field “socially constructed” is a compliment paid to a fact or a procedure that has emerged from the welter of disciplinary competition into a real and productive life where it can be cited, invoked and perhaps challenged. It is no contradiction to say that something is socially constructed and also real.

He depicts Sokal as a poor logician: “This [keeping distinctions in activities clear] is what Professor Sokal does not do, and this is his second mistake. He thinks that the sociology of science is in competition with mainstream science—wants either to replace it or debunk it—and he doesn’t understand that it is a distinct enterprise, with objects of study, criteria, procedures and goals all of its own.” He even goes so far as to instruct Sokal in the ways of science:

On the science side, although scientists don’t take formal votes to decide what facts will be considered credible, neither do they present their competing accounts to nature and receive from her an immediate and legible verdict. Rather they hazard hypotheses that are then tested by other workers in the field in the context of evidentiary rules, which may themselves be altered in the process. Verdicts are then given by publications and research centers whose judgments and monies will determine the way the game goes for a while.

Finally, he accuses Sokal of undermining “the intellectual standards he vows to protect” by violating the concept of trust on which all inquiry must depend.

Fish’s article and Gould’s reviews are good examples of ridicule or exposé. They are public attempts to de-authorize publications that could be perceived as dangerous to the community. Not content to let voices from the outside launch attacks on the orthodoxy unchallenged, the bonny fighters of the faith go out to do battle in a relatively neutral public forum to diminish the stature of the opponents’ champions. Such is the ideal—Hector against Achilles on the neutral plain; David against Goliath between the armies of Israel and Philistia. Nevertheless, as the examples of Gould’s reviews show, the practice of public ridicule often takes place safely behind the walls of

the orthodoxy's fortifications, appearing in less than neutral publications such as popular science magazines, where rejoinder is often difficult or impossible. But perhaps that doesn't really matter since the primary audience for these pieces is usually exasperated insiders, who wonder why someone doesn't respond, rather than the general public.

Conclusion: Denial of Access and the Emergence of Alternative Forums

We are used to thinking of the review process as the primary method by which science protects its knowledge domain. However, in this article, I have tried to show that there are also other means of forum control employed by orthodox scientists to exclude and silence deviant or heretical discourse. Exclusion and ridicule deny scientific forums to unwanted outsiders; peer review and public correction manage scientific discourse of insiders, sometimes suggesting minor changes, as in the peer review, but sometimes insisting on silence, as in the censure.

Nevertheless, when people are denied access to established forums, they find ways of getting their work out. Sally Dennison has told stories of major literary figures who were denied publication early in their careers and, therefore, helped create alternative publishing outlets. These writers include T. S. Eliot, Virginia Woolf, and James Joyce. Some of the work discussed in the body of this paper is available only through alternative publication outlets. Condit's letter to *Science*, for example, appears in a journal devoted to rhetoric studies. Publishing in alternative forums involves allowing what one has to say to be contextualized in ways that the writer had not originally planned. Condit's letter was written directly to readers of *Science* and she hoped to contextualize her speech in the established conversation of research on sex differences. Instead, it now appears in the ongoing conversation known as rhetoric of science studies, and the nature of the letter changes from a direct address to a case used as an example. This process of recontextualization changes the nature of the letter even though it is exactly the same document as it would have been had it been published in *Science*. The context in which a publication appears frames the utterance, linking it to earlier utterances, and inviting future responses, but only within the conversation authorized by the journal's imprimatur.

Driven from legitimate scientific forums, some scientists considered to be heretical by orthodox science seek out other forums, sometimes forming their own professional organizations, establishing their own presses, and launching their own journals. These shadow forums seldom achieve legitimacy in the scientific community, but they give the appearance of legitimacy to people outside the academic world.

An even more successful strategy employed by creationists has been to engage evolutionists in public debates, usually on college campuses. As David Klope has shown, this alternative forum, which is invested with the values of free, unencumbered public debate instead of with the values of professional, esoteric expertise, has afforded creationists the opportunity to engage in a kind of guerrilla rhetoric in which they often embarrass unprepared evolutionists. Not able to control these forums, scientists now offer special sessions at professional conferences like the AAAS on how to debate creationists. Scientists who have not learned how to argue in these forums are counseled to avoid them altogether, thereby taking away the only apparently legitimate, though non-scientific, forum left to creationists.

Yet another alternative forum, the World Wide Web, is rapidly appearing. Pascal's paper on AIDS, which I referred to earlier, appears on a Web site devoted to publishing suppressed scientific work. Alan Sokal, unable to respond to Stanley Fish's critique in *The New York Times* with as many column issues as he needed, republished Fish's critique and his rejoinder on his own Web site, but Fish's critique is now buried in a list of links eight pages long, making Fish appear to be only a minor player in a much larger debate. By recontextualizing the article in this way, he co-opts it, making it magnify the significance of his own work. It remains to be seen whether World Wide Web publishing can achieve legitimacy, whether or not the stigma of self-authorization can be expunged from a medium in which anyone can publish anything with little training. Serious publishing on the World Wide Web—whether in the form of large archives like *The English Server* <eserver.org>, which houses over 20,000 works; or *Perseus Project* <www.perseus.tufts.edu>, which houses many digital copies of classic texts; or in the form of refereed journals like *Kairos* <english.ttu.edu/kairos/homepage.html>; or magazines like *Bad Subjects* <eserver.org/bs>—are authorizing the speech of some who seek alternative forums, but these forums are in the process of gaining legitimacy themselves; therefore, the authorization they lend is still of debatable value.

Future research on forum control in science should go beyond analysis of the rhetoric of scientists. Although some have analyzed the role of the public media in authorizing science (see, for example, Condit 97-100; Sullivan, "Exclusionary Epideictic"; Nelkin; Fahnestock), more studies are needed to help us understand the public media's role in shoring up the image of science and in reinforcing its autonomy. Does the media provide an alternative forum for suppressed research, or does it always support and magnify scientific orthodoxy? What role do programs like the National Science Foundation's "Informal Science Education Supplements" (which makes money available for disseminating information about the research NSF funds) play in reinforcing orthodoxy and in suppressing heterodoxy? Perhaps Martin is right when he asserts that "from the

point of view of the interests of powerful non-scientific groups, the scientific power structure serves a valuable function of social control" (38). Or perhaps there is an unchallenged social assumption that science and technology are ultimate goods, which have now become the ground for legitimizing other social actions (Habermas). Or perhaps closer inspection will show that forum control in science is seldom abused and that representations of science in the press are relatively neutral.

Finally, why is it important to study forum control in science? As I suggested in the introduction, taking a closer look at forum control helps demythologize science. We begin to see it as a human endeavor with political dimensions. But to what end? Why should science be demythologized? One answer may be that it is an incredibly powerful orthodoxy in modern western culture. Public funding for science dwarfs funding for the arts and humanities, largely because of the myth of scientific objectivity, because of highly publicized successes in medicine, and because of promised technological applications. It may be time for the public to look more carefully at how the public monies are distributed, and I don't mean just redistributing them so that the arts and humanities enjoy a larger share. There are many needs that should be considered, which, unfortunately, are often overlooked because of lopsided budgets.

However, this issue does not account for my interest in forum control in science fully. Actually, for me personally it is only a minor concern. What is of more interest to me is what the institutionalizing process has done to science. The original dream of science as the democratic, joint effort of many people making observations and working together to understand nature was, in my opinion, a noble though unrealistic vision. As science has become more complex, as the cost of doing science has increased, as the levels of education and specialization have become more extreme, and as the stakes have become higher, science has become an esoteric practice, a privileged society, open only to those who can satisfy the stringent requirements of science and align themselves with a recognized scientific institution. This increased sophistication and institutionalization is perhaps unavoidable, but when the sophistication is accompanied by scientism, a set of assumptions about what constitutes legitimate science and what doesn't, then science has the added stricture of imposing orthodoxy on its members, thereby cutting off lines of possible research and, perhaps, blinding itself to avenues of understanding.

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